Dear Michal Malinowski,

we thank you and the anonymous reviewer #1 for the second review of our manuscript. We are grateful for the constructive minor comments which helped us to even further improve the manuscript.

Please find below the detailed reply to the comments. All reviewer comments are shown and highlighted as bold text, followed by our answers as indented normal text. Line numbers in our response refer to the tracked manuscript.

If the manuscript gets accepted, we kindly ask you to allow us replacing the general data services link in the section *data availability* of the manuscript by the precise link that refers to our data publication and which is generated at the moment.

We appreciate your time and hope that our revised manuscript now qualifies for publication in Solid Earth.

Yours sincerely,

Maria Leonhardt

Response to referee #1 comments

Major comments of reviewer #1

1) Ln 101,102: please indicate the minimum magnitude of the two data sets incl. 5456 and 55707 events. It would be also useful to mention what type of detection/picking was used for the two data sets.

Authors:
We added the minimum moment magnitude of both event sets to the paragraph:

"The reprocessed seismic catalog with description of its properties is available as separate data publication (see section data availability) and consists of 5,456 events with $M_W \geq -2.47$ that were detected and located during and after the stimulation (industrial monitoring) and reprocessed in our study. A total of 55,707 events with $M_W \geq -0.95$ were further detected during and after the stimulation but were not located or processed later on."

Details of the type of detection/picking are mentioned in the description of our data publication. To avoid repetition, we decided to not mention them in the manuscript again.

2) Ln 117: 'A total of 2,958 reprocessed events were located…': please specify what was the reason for relocation only about 60% of the catalog events: too large RMS, or position too far from the well or something else?

Authors:
The hypocenter position of the events close to the injection well was the crucial factor for considering only ~60 % of the located 5,456 events for relocation and further analysis. We focused only on the 2,958 events that were located close to the injection

well *OTN-3* within a defined target volume at an epicentral distance of less than 5 km around the well and at depth of 4.5 to 7 km (as also mentioned in lines 118-120).

3) Ln 212: Only the two shallower clusters elongate along SHmax, please be more precise.

Authors:
To be more precise, we changed the sentence as follows:

"Elongation of the two shallower clusters in SE-NW direction is sub-parallel to the local maximum horizontal stress S_H^{max} = 110° (Kwiatek et al., 2019; Heidbach et al., 2016; Kakkuri and Chen, 1992)."

4) Ln 214: What do you mean by saying '…spans ~700 m depth. This exceeds 215 vertical relocation precision…'? I believe this is not surprising as your estimated relocation precision is 52m (Ln 129).

Authors:
This is correct. We simply wanted to indicate that the thickness of the seismic cloud in vertical direction is a real feature.

5) Ln 219 - 220: The statement 'The post-injection seismicity … seems to be mostly confined to three isolated clusters, with two of them located on the NW flank of the injection well OTN-3' is not very clear - I do not see two grey clusters at this position. Possibly you can omit this statament as it does not bring valuable info.

Authors:
We agree, the statement does not bring enough information and therefore we deleted this statement and combined the first part of the sentence with the following sentence:

"The post-injection seismicity shows no spatial migration and the largest post-stimulation events with magnitudes between M_W 1.0 and M_W 1.5 occurred at the NNW and SSE outer edge of the main cluster."

We also deleted a similar sentence in the Discussion in lines 342-343.

6) Ln 293: 'We further analyzed qualitatively the polarity pattern of events with polarities estimated from cross-correlation based technique of Shelly et al. (2016) ..': It is not clear whether this analysis is also shown in Fig. 8.

Authors:
In Figure 8 we showed the most repetitive polarity pattern of all focal mechanisms, regardless if the focal mechanisms were obtained with manually picked polarities or with estimated polarities by the Shelly approach.

To avoid any confusion, we updated the sentence in lines 286-287 as follows:

"Regardless of manually picked or estimated polarities, the most repetitive polarity pattern observed at each station for a particular family is plotted in Fig. 8a-c."

7) Ln 295 - 305: it is not easy to follow this section without illustrating the situation in a plot/table.

Authors:
We included a table to the supplements (Tab. S1) which shows for each family and station how many of all events with focal mechanisms (in percent) display the same polarity pattern as the most repetitive polarity pattern presented by the focal mechanisms in Fig. 8a-c.

We further included the following sentence to the manuscript in lines 287-288:

"For each family and station, the percentage of FM events showing this repetitive pattern is presented in Tab. S1."

We further slightly updated the paragraph (lines 298-307) for a better understanding.

8) Ln 377-382: The stress computations need be better explained. The point is that stress varies with depth and is thus unique for each event. And further, how do you estimate the pore pressure to get effective stress?

Authors:
The stress at 6.1 km depth has been first derived in study of Backers et al. (2016), but we do not have depth-dependent stress profile. As indicated in the text, stress field orientation and stress shape ratio is very similar to that derived from application of BRTM method. Unfortunately, the amount of data disallows to discuss any spatial changes in the stress orientation. Thus, we focused on quantifying which fault plane families are more likely to fail first in the determined stress field (no presence of fluids). In the presence of the enhanced pore pressure, fault planes with higher instability coefficient (i.e. locate closer to failure envelope) would fail first. Such analysis requires, however, detailed data on spatial and temporal changes in the stress field, which is note possible, and also out of the topic actually discussed in this part of the manuscript. Text fragments in lines 314-322 and 382-389 have been modified to explain better the stress state and Mohr circle.

9) Ln 384 - 390: Fig. S5 should be moved to the main part as Fig. 11 and Table S1 as well.

Authors:
We included Fig. S5 and Tab. S1 to the manuscript as Fig. 11 and Tab. 1, respectively.

Minor comments of reviewer #1 on language

10) Ln 279: 'very different in between families' - 'in' appears superfluous.

Authors:
We deleted the word "in" in the sentence.

11) Ln 280: between three -> among three.

Authors:
Thank you for the hint. We updated the sentence by replacing "between" with "among".

12) Ln 316: 'The stress inversion of the induced seismic events represents a local reverse faulting regime.' Stress inversion as a method (correctly Inversion of focal mechanisms for stress) cannot represent a faulting regime. I suggest 'The stress obtained by focal mechanism inversion represents…'

Authors:
We changed the sentence as follows:

"The stress obtained by focal mechanism inversion represents a local reverse faulting regime."

13) Ln 386: S5 should read as S6, same for Ln 393.

Authors:
In the revised supplements, we did not include a Fig. S6 and therefore the references to Fig. S5 in Ln 386 and Ln 393 in the manuscript were correct. However, by now including Fig. S5 as Fig. 11 to the manuscript, we also updated the references in the manuscript.

Particular comments of reviewer #1 on Figures

14) Fig. 2: To make the caption more informative, please give the number of events shown (2958?)

Authors:
We included the numbers in the captions of Fig. 2:

"The magnitudes of 2,958 absolute located and 1,986 relocated events are shown as grey and orange dots, respectively."

15) Fig. 3: Please indicate the profile of the depth section (b) in the map view (a).

Authors:
We updated Fig. 3 showing now the profile of the depth section of Fig. 3b.

16) Fig. 8: It would be useful to give the number of events for each family also in the caption, better than in the text

Authors:
We updated the first sentence of the caption of Fig. 8 by including the number of events of each family:

"Mean fault plane solutions (black lines) calculated from best FPSs of 99 events forming family I (a), 60 events forming family II (b) and 27 events forming family III (c)."

17) Fig. S3: The figure caption is not correct. According to your reponse 14) the light gray symbols show all events, not those with Mw>=1.

Authors:
Thank you for the hint. We updated the first sentence of the caption as follows:

"Map view of all relocated events."

18) Fig. 10: In the caption you state that 'A stress ratio of R = 0.53 was used for stress inversion.' This sounds confusing as R is the result of stress inversion.

Authors:
To avoid any confusion, we changed the sentence in the caption of Fig. 10 as follows:

"The stress inversion resulted in a stress ratio of R = 0.53."

Particular comments of reviewer #1 on data publication

19) In Fig. 5 you show the Gutenberg-Richter distribution of the new catalog. It would be however much more iknformative to show two curves: one for the original catalog and another for the new one. Then one would see at which magnitude levels most new events were detected.

Authors:
The Gutenberg-Richter distribution of the original catalog is already presented in the supplementary materials of Kwiatek et al. (2019) and thus we refrain from showing this plot in our data publication again.