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Interactive comment on "Potential influence of overpressurized gas on the induced seismicity in the St. Gallen deep geothermal project (Switzerland)" *by* Dominik Zbinden et al.

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Summary and evaluation by reviewer

Zbinden et al. present a modelling study of the induced seismicity triggered during stimulation of the St Gallen EGS project, Switzerland. The study applies methods and models developed by the authors in previous work. Therefore, the novelty here is in the specific application (St Gallen) and its idiosyncrasies (involvement of gas). The primary finding is to confirm a hypothesized conceptual model using a numerical model that approximates multi-component (water and gas) fluid flow and seismicity triggering (a stochastic "seed" model). The model has difficulty capturing all complexities associated

with the stimulation, e.g., borehole processes, breaching of the fault seal, but these are acknowledged and discussed by the authors. Overall, I think is a well-executed study, technically sound and fairly presented. I have listed below a few technical and editorial comments that the authors may wish to consider, although none are major items.

We would like to thank the reviewer for the positive review and the comments, which were all considered in the updated manuscript. Our detailed response to each comment can be found below. We would like to clarify that, although the St. Gallen deep geothermal project has been considered an EGS in some studies (e.g., Breede et al., 2013), we prefer to classify it as a hydrothermal project, as no hydraulic stimulation for targeted shearing of fractures (i.e., hydro-shearing) adjacent to the injection well was performed.

Breede, K., Dzebisashvili, K., Liu, X. et al (2013). A systematic review of enhanced (or engineered) geothermal systems: past, present and future. Geotherm Energy 1, 4. https://doi.org/10.1186/2195-9706-1-4

(1) Reviewer comments (2) Author response* (3) Changes in the manuscript

*Line numbers refer to the initially submitted manuscript

Detailed Comments:

(1) Abstract: final sentence - "important implications" - could be specific about which implications of the study you think are important.

(2) We now explicitly mention that the study could have implications for future deep hydrothermal projects where potentially overpressurized gas may be in-place.

(3) "This study may have implications for future deep hydrothermal projects conducted in similar geological conditions with potentially overpressurized in-place gas."

(1) L32: "rock-fluid interaction" suggests geochemistry in many circles, when I think you are referring to fluid destabilisation.

(2) We understand that the term “rock-fluid interaction” is not completely clear. Here, we meant any kind of interaction between rock and fluids (water and gas) including thermal, hydraulic, chemical and mechanical processes. Therefore, we now use the term “thermo-hydro-mechanical-chemical interactions” to avoid any confusion. Note that we do not further consider chemical and thermal processes in our simulations, since we neither model the acid stimulations nor expect a significant cooling effect over the relatively short-term and small volume injection that occurred in St. Gallen (L 304-307).

(3) “Hence, it is crucial to get a more accurate understanding of the thermo-hydro-mechanical-chemical interactions occurring at reservoir depths.”

(1) L39: "recently, a M 5.5..." awkward phrasing

(2) The reviewer is correct, we have changed the sentence accordingly.

(3) “recently, a Mw 5.5 earthquake struck the city of Pohang (South Korea) (Ellsworth et al., 2019; Grigoli et al. 2018), the largest earthquake recorded at an EGS site up to date (Kim et al., 2018).”

(1) L42: would be appropriate to cite McGarr 2014 here

(2) The reviewer is correct, we cited Eaton and Igonin (2018) that summarized recently proposed approaches to estimate the maximum induced magnitude. We now cite McGarr (2014) and another recently proposed model that relates the total injected volume to the maximum arrested rupture (Galis et al., 2017) instead of Eaton and Igonin (2018).

(3) “This earthquake has challenged recently proposed models that relate the maximum expected seismic magnitude to the total injected fluid volume (Galis et al., 2017; McGarr, 2014).”

(1) L49: "gas kick" introduced without being defined - an early definition would aid readability

(2) We now define the term “gas kick” where it is first mentioned in the text.

(3) “. . ., gas entered the borehole from an unidentified source at a pressure greater than the one exerted by the fluid column in the borehole (a so-called gas kick). The gas kick . . .”

(1) L61: very pedantic but "secondly" is not a word IMO. However, if you’re going to use the ’ly’ then be consistent (e.g., firstly in prev sentence)

(2) We now use “firstly” in the previous sentence to be consistent.

(3) “Firstly, we describe the temporal and spatial evolution of the seismic sequence associated with the injection. Secondly, we present . . .”

(1) L271: distribution for coefficient of friction is quoted but not for other parameters in seed model

(2) In the seed model, other parameters, such as the shear modulus of the fault, Poisson’s ratio, fault cohesion and stress drop coefficient were constant with no normal distribution around its mean. In addition to the friction coefficient, parameters with a normal distribution were the magnitude of the horizontal and vertical stress and the orientation of the horizontal stress, the latter corresponding to a normally distributed orientation of the fault strike (while the horizontal stress is held at a fixed orientation). Stress values and orientations are quoted in L 323-330, while the seed parameters with their distributions are listed in Table 2.

(3) No changes in the manuscript.

(1) L311: I don’t think this is explicitly mentioned - is the gas modelled in TOUGH2 methane or air?

(2) The gas is air, which we now clarify in L 308-309. Methane gas and air (ca. 78 % nitrogen) are both in a supercritical state at reservoir conditions (e.g., Nasrifar and Bolland, 2006), i.e., their dynamic viscosity is similar to a gas (in the order of 1e-5

Pa s) and their densities are between a liquid and a gas (about 150 to 300 kg m-3). Therefore, the use of air instead of methane is an appropriate approximation for the purposes of our study.

Nasrifar, K., and Bolland, O. (2006). Prediction of thermodynamic properties of natural gas mixtures using 10 equations of state including a new cubic two-constant equation of state. Journal of Petroleum Science and Engineering, 51(3-4), 253-266. https://doi.org/10.1016/j.petrol.2006.01.004

(3) “In order to model the multi-phase fluid system, we employ an equation of state with water and air as liquid and gas phase, respectively.”

(1) L478: With arbitrarily seeded stochastic simulations, your ability to "reproduce the extension (extent) of the observed seismicity" can be challenged as simply a random feature of the realisation. Have you run multiple realisations and confirmed that the observed extent falls within the modelled distribution?

(2) We thank the reviewer for this comment. In the initially submitted paper, we already accounted for the 1000 realizations, but we did not calculate the average of the spatial distribution of the realizations. We now also quantify the extension of the simulated and observed induced seismicity: the mean extent of the 1000 model realizations is 0.133 km2 with a standard deviation of 0.025 km2, while the area of the observed seismicity cloud is 0.214 km2 (only considering events with a magnitude larger than the magnitude of completeness). Hence, the extent of the observed seismicity is greater than in the simulations, even if one standard deviation is taken into account. We now clearly indicate these results in the manuscript.

(3) “Regarding the spatial distribution of the seismicity, our model approximately reproduces the extension of the observed seismicity cloud (Fig. 3e and Fig. 3f), although the simulated seismicity cloud is somewhat smaller than the observed one: the mean extent of the seismicity of the 1000 model realizations is 0.133 km2 with a standard deviation of 0.025 km2, while the area of the observed seismic events (with magnitudes

greater than Mc, see below) is 0.214 km2.”

(1) L489: pedantic maybe, but "nicely" is perhaps a value judgment best left to the reader

(2) We agree with the reviewer, thus we removed the word “nicely”.

(3) “. . ., the simulation captures the strong increase of seismicity after the main shock at about 1 day.”

(1) Finally, one thing I missed in the discussion was some comment on model uniqueness. A lot of choices have to be made about parameter values in your model. Even if these are the best, most defensible values, they could still be wrong. Which parameter values do you feel are least well constrained? If the model was rerun (incl. recalibrated) using different plausible values, would you arrive at similar conclusions (either qualitative or quantitative)?

(2) Our model with a hydraulic connection between the injection well and the reactivated fault is based on one of our previous studies (Zbinden et al., 2020). We found that several fracture zone parameters (permeability, porosity, compressibility) affect the pressure response at the well and on the fault. For calibrated models, however, the response in terms of pressure and stress changes was comparable, which leads to similar conclusions. We now write in Section 5.1:

(3) “Zbinden et al. (2020) found that several fracture zone parameters affect the pressure response at the well and on the fault. For calibrated models, however, the response in terms of pressure and stress changes was comparable, thus leading to similar conclusions.”

Additional reply to the last reviewer comment:

The properties of the hydraulic connection are also critical for the simulation of the gas kick, the well control and the main sequence of the induced seismicity. There, the most uncertain parameters are the location and overpressure of the gas plume, and the permeability of the breached parts of the fault seal. We argued in L 446-449 that changing the permeability of the breached fault seal is similar to varying the overpressure of the gas reservoir, as both influence the timing and strength of the gas kick (effect of permeability of the breached fault seal on pressure evolution is shown in Fig. 10 in the manuscript). One different location of the gas plume was tested in a scenario without a fault seal (Fig. 14), and further possible scenarios were discussed in L 602-606. We mentioned that if the gas was stored elsewhere, it may not be directly linked to the seismicity. However, given the stratigraphy and the observed delay between the stimulations and the gas kick, we consider it most likely that the gas was stored in the permo-carboniferous trough and in the Muschelkalk layer (L 161-172, Fig. 3 and 4).

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