REPLY TO EDITOR CORRECTIONS

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"Potential influence of overpressurized gas on the induced seismicity in the St. Gallen deep geothermal project (Switzerland)"

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

Tarje Nissen-Meyer (Topical Editor)

Dear authors,

Many thanks for your detailed and attentive replies and updated manuscript. I am now happy to accept your manuscript for publication in SE after inspecting your changes. I suggest technical corrections only very minor suggestions, but leave it to your valued judgment to update these:

We are grateful to Tarje Nissen-Meyer for the positive assessment of the manuscript and the technical corrections. Please find a detailed response to these comments below.

If a reviewer misses the description of parameters (only friction coefficient in seed model), chances are that others may overlook it similarly. I suggest mentioning around the quoted section where you describe the other parameters.

We moved the sentence describing the exact values for the friction coefficient from Sect. 4.1 to 4.2. Moreover, we now clearly write that friction and the state of stress are the only parameters in the seed model that follow a normal distribution, while all other parameters are constant. We now write in Section 4.2:

"We assume the seeds to have a coefficient of friction of 0.6 ± 0.05 and a cohesion of 1 MPa. … Note that all parameters in the seed model, except for the coefficient of friction and the state of stress that follow a normal distribution, are assumed to be constant. A list of the seed model parameters is given in Table 2."

I would find it insightful to refer to the Nasrifar paper for those who wish to follow up on the choice of the gas model

We now more accurately motivate the choice of the gas model and refer to Nasrifar and Bolland (2006).

“Methane and nitrogen (air contains approx. 78 % nitrogen by volume) 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 and their density is between a liquid and a gas. We therefore consider the use of air instead of methane to be an appropriate approximation for the purposes of this study.”

Please use "extent" instead of "extension of observed seismicity" as suggested.

Corrected.

“… our model approximately reproduces the extent of the observed seismicity cloud …”

I appreciate the detailed explanation of the 5m fault core thickness, and wouldn't mind seeing a bit more of this in the updated manuscript. I believe readers may benefit from this discussion.

We now discuss in more detail the choice of fault core thickness and its influence on the strength of the simulated gas kick. In Sect. 5.2, we write:

“Similar to the permeability of the breached fault seal, we expect the thickness of the fault core to influence the strength of the gas kick, because reducing the thickness would result in a higher pressure gradient between the two reservoir compartments, which would cause more fluid flow across the fault after the seal has been breached. In our numerical model, the thickness of 5 m corresponds to the width of the fault core elements. Individual fault cores are usually thinner than 1 m (e.g., Shipton et al., 2006), but faults may contain multiple narrow cores (e.g., Faulkner, 2010) so that the sealing part of the fault can be thicker, which can justify our assumption in the model. Moreover, note that according to Darcy's law, an increase (decrease) in permeability of the breached fault core would correspond to a decrease (increase) in thickness of the fault core, since in both cases the fluid flow across the fault would be equally affected. For instance, decreasing the fault core thickness by one order would correspond to the scenario with a breached fault seal permeability of 10^14 m2. Hence, the sensitivity study on the permeability of the breached fault seal is equivalent to examining the effect of fault core thickness on the strength of the gas kick.”

CharLotte Krawczyk (Executive Editor)

Dear Dominik,

in addition to the corrections advised by Tarje Nissen-Meyer, there is also one reviewer comment and your answer, I would like to direct your attention to: "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." This thought may arise for several readers, so that I'd suggest to include your rephrased answer somewhere in the beginning of your final revised manuscript, so that a statement clarifies your view.

I'm looking forward seeing your manuscript published in SE, Lotte.

We thank CharLotte Krawczyk for this suggestion. We have now clarified our view in Sect. 2 as follows:

"Although the St. Gallen deep geothermal project has been considered an EGS in some studies (Breede et al., 2013), we here clearly classify it as a hydrothermal project, since no hydraulic stimulation for the targeted shearing of fractures (hydro-shearing) was performed adjacent to the injection well (see below)."