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Solid Earth
Discussions

Interactive comment on "A numerical sensitivity study of how permeability, geological structure, and hydraulic gradient control the lifetime of a geothermal reservoir" *by* Johanna F. Bauer et al.

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Reply to the review by RC1:

We thank the reviewer for the positive evaluation, for the critical questions, and the insightful comments, which help us to improve the manuscript. The final corrections of the manuscript will be provided and marked up in the final response to all reviews. Please find our answers below:

General comments:

1. Manuscript structure: Reviewer comment 1.1: For any generic numerical study,

appropriate input parameters and real-world analogs are important. I would therefore recommend to merge the first part of the "Discussion" (lines 312-327) with the "Introduction" and to move or even repeat some parts in the "Methods" section, in particular the "Scenarios" section. The reader of the manuscript would greatly benefit from a direct real world example for the chosen permeabilities, porosities and in particular background hydraulic gradients (BHG) right in the "Methods" section.

Authors reply: We agree with the reviewer and will in agreement with the comments of reviewer 2 improve the Introduction regarding this matter. We will now introduce the values we used for permeability, porosity and BHG in the Introduction and discuss how these compare to typical values values for sedimentary basins worldwide. With our manuscript, we, however, present a non-site specific, numerical sensitivity study that investigates the influence of various reservoir parameters on geothermal reservoir lifetimes and how exactly they have to be known to provide reliable estimates on the lifetime of a geothermal reservoir. For this reason, we did not only chose parameter values for permeability and porosity that are desired in geothermics, but also values that lie above and below them. Since our sensitivity study is not site specific, we only present real world scenarios in the discussion (see also point 8.1). This is why we prefer to keep the current structure of our manuscript.

Reviewer comment 1.2: Especially, the various BHGs require some geological scenarios (what can cause a directed BHG? Topography, overpressure, ...?). Also, the authors might consider merging the entire discussion with the results section for better readability.

Authors reply: We agree with the reviewer and see the necessity to explain in more detail in the introduction why we choose to investigate the influence of the BHG and provide examples of settings in which BHGs are to expect or in which they have been observed. We will also justify the values we used for the BHG's magnitude, and refer to the according literature e.g. Fan et al. (2013, Science), Gleeson et al. (2016, NGS) and Grauls (1999). We, however, prefer, regarding the second point addressed here, to keep the results and the discussion sections separate.

2. Convection: Reviewer comment 2.1: Convection is not considered in the numerical modelling to save computational cost. As the authors state correctly, convection is likely to be neglected in sediment layers. However, in fault zone-controlled reservoirs, convection is known to have a big impact on the initial temperature field (e.g. Soultz-sous-Forets). Please at least discuss the possible impact of convection on this study's results related to fault zones or consider running a few models that account for convective flow to highlight the impact.

Authors reply: We agree that faults/fault zones can have significant heat flow by density-driven convection. We are also aware, as the reviewer states, that there are several real world examples of faults in which free convection has been observed and we will include this fact in the discussion. However, in many scenarios it is also likely that, due to the heterogeneous nature of faults, convection is not present. However, there a very few published examples in the literature. Our models likely underestimate the lifetime of fault-related reservoirs, because they do not include density-driven convection and thus heat supply from deeper levels. The effect of density-driven convection, however, at least to a certain degree, would be to counteract the negative influence of the channeling effect of a fault (see also points 7.4 and 7.6). Regarding the reviewer's suggestion to rerun these models with density-driven convection, would mean that these scenarios are not comparable anymore with the other parts of our study. We will follow the reviewer's suggestion and will now address the possible effects of density driven convection in the discussion section.

3. Bottomhole pressure (BHP) and flow rate: Reviewer comment 3.1: The authors work with a fixed flow rate, which for the low and medium permeability scenarios results in impossible bottomhole pressures well above the lithostatic stress.

Authors reply: The reviewer is correct that the pressures for the low permeability scenarios are extremely high or even impossible. However, since we chose for our numer-

ical sensitivity study to investigate the impact of a range of parameters and parameter values (point 1.1), it is inevitable that some of the combinations represent unrealistic scenarios. These results are nethertheless part of our study and as such help to draw the picture and to understand the effect of the investigated parameters within geothermal reservoirs. Without them, some of the effects would not have been identified by us. In consequence we are convinced that they constitute an integral part and should not be rejected. We hope the reviewer can agree with this and is also referred to our answer to point 3.6. We also wish to note that in case of the medium permeability model, the high pressure could be easily corrected in the model by changing i.e. the depth of the well or the reservoir, or the borehole diameter (see line 334-339 of our manuscript).

Reviewer comment 3.2: Nevertheless, this is only mentioned briefly at the end of the manuscript. Here the authors also state that in these cases "the BHG is outperformed by the artificial flow field caused by the very high bottomhole pressure". This has to be mentioned directly in the "Methods" section. The actual value of the low and medium permeability models has to be questioned.

Authors reply: We agree with the reviewer: we will add in the method section that artificial flow field and BHG interact. That some of the models return unrealistic BHP, i.e. represent unrealistic scenarios, will be mentioned in the introduction and the method sections. Please see also our reply to your point 3.1. Regarding the medium permeability values, please see line 334 - 339 (discussion paper).

Reviewer comment 3.3: The BHG appears to be one of the main drivers, but it is completely overruled by the impossible BHPs in the low and possibly also medium perm-scenarios. In that way, only the low and medium perm model without BHG (0 mm/m) might have some value since the shape of the HDI should not be impacted in that scenario (or is it?).

Authors reply: The value of these models is that they show that if the artificial flow-field introduced by the bottomhole pressure is stronger than the BHG, the importance of the BHG ceases (Fig. 2a, b). Even though, these models represent unrealistic cases in terms of the bottomhole pressure. To show the same effect in a model suite with higher permeability we would need a BHG far smaller than used in our study. Another example is that the impact of layering on thermal breakthrough times, is less well observable in the high permeability models (because the bottom hole pressure is too low in these cases to investigate the effect; please compare Figure 4a with Figure 4g) and can only clearly seen in the unrealistic low permeability models. We think therefore that these (unrealistic) parameter combinations are an integral part of the study and should not be omitted.

Reviewer comment 3.4 In addition, wouldn't the induced BHPs also impact the flow velocity in the reservoir and therefore also thermal breakthrough (I am not certain here, but at least mention and discuss)?

Authors reply: Flow velocities are limited by how much water is injected and produced from the system, and are therefore not a function of the BHP's. The main effect of the BHP can be seen in its interaction with the background hydraulic gradient. We agree with the reviewer and will now mention this point in the revised manuscript.

Reviewer comment 3.5 As a consequence, I would recommend to exclude all other low and medium perm scenarios with a BHG > 0 mm/m. Otherwise please discuss accordingly and inform the reader in the "Methods" section about a) the unrealistic BHPs, b) their impact and c) why the models might still have some value.

Authors reply: These models are an integral part of our study. Please see our answer to points 1.1, 3.1, and 3.3.

Reviewer comment 3.6 Alternatively, the models could be rerun for different flow rates (e.g. with a fixed draw-down pressure, which is a much better technical parameter to be controlled and more or less independent of the geology/petrophysics).

Authors reply: Firstly, if we had chosen a fixed draw-down-pressure, we would have had to deal, at least in part, with extremely low or high flow rates, i.e., the amount of injected cold water would change. Consequently, we would not be able to analyze the interaction and impact of the tested petrophysical and structural parameters, which is the main focus of our manuscript. Secondly, for making a large series of models that can be compared to each other and in which the effects of individual parameters can be isolated, the option would be to either to use a fixed bottom hole pressure, which will induce unrealistic flow rates in some models, or fixed flow rate, which will result in unrealistic pressures. The choice for fixed flow rate is because this has the least disturbing effect on the model results because the amount of injected cold fluid stays the same. With other words, to rerun some of the models with fixed draw-down pressure would not correspond to the setup of our study, rather it would alter the results of these particular models and therefore destroy comparability. Please see also our answer to your comment 3.1 and 3.3 above, where we answer similar questions.

Detailed line-by-line comments

4. Abstract Reviewer comment 4.1: Well written, please consider to avoid usage of acronyms (BHG and HDI).

Authors reply: Here we follow the standards of the Journal that require the introduction of acronyms in the abstract.

5. Introduction: Reviewer comment 5.1 Line 33: Maybe better say hydrothermal than deep geothermal (petrothermal/HDR is also deep geothermal, but only produces from fractures).

Authors reply: We follow the advice of the reviewer and replace "deep geothermal" with "hydrothermal".

6. Methods

Reviewer comment 6.1: Very minor, but almost all sentences start with "We. . ."

Authors reply: We agree and we will reformulate this part.

Geometry of the model: Reviewer comment 6.2: The horizontal extent of the model seems to be rather small (only 4 km), while the vertical extent is very high (2.3 km). It is not clear if this extent only represents the reservoir or also overburden and footwall sediments. Please specify.

Authors reply: We think that the best way to approach this issue is: that in our sensitivity study, the whole model domain should be seen as a potential reservoir volume, i.e. our study investigates which parameters control and or influence the volume that actually can be utilized as a reservoir. We will improve the text accordingly to avoid potential misunderstandings. For your comment on the lateral extent, please see our answer to your comment on line 105 below (see also point 6.5).

Reviewer comment 6.3: Line 91: The rescaling of the well diameter and "length" is confusing. Please explain in more detail, how and why the rescaling has been done and what is meant by "length" and "active part" (perforated production zone?).

Authors reply: We will follow the suggestion of the reviewer and rewrite this part accordingly. Standard well diameters are a few decimeters. This in turn would need a very fine mesh. To avoid this issue we used a larger diameter for the wells. To account for the unrealistic high diameter and thus the area of the "perforated production and injection zone" we choose to adjust the area via its length to a size that is in a realistic range.

Temperature:

Reviewer comment 6.4: Line 97: The gradient's unit is wrong (should be 0.047 degC/m not per km). Also, please briefly explain why the respective gradient and surface temperature have been chosen. Especially, since the gradient is very high and the surface temperature is very low.

Authors reply: Thanks for identifying this mistake. We corrected the typo 0.047°C/m.

Since we carried out a non-site specific numerical sensitivity study, we chose a realistic gradient that allows for electricity production at this depth. The surface temperature was chosen arbitrarily to be 0°C. This is in our opinion neither particularly high nor low, especially when considering that our numerical sensitivity study is not site specific. Nethertheless, the effect on the model results can be neglected, since a slightly increased surface temperature would alter not the temperature at target depth or the model results significantly.

Reviewer comment 6.5: Line 105: This explanation of the model size should be move to the geometry section (2.2). The explanation itself is not really convincing: the model probably could have been extended to 10x10 km without significantly more cells, since no high resolution is required at the boundaries and far away from the wells. Please at least mention/discuss possible effects here and in the discussion section.

Authors reply: In Line 103-104 of our manuscript, we describe that the temperature boundary conditions do not affect the model results, i.e. the size of the model domain does not affect the model results. The sentence in line 105-106 is thus obsolete. We will delete the last sentence. This solution also makes merging the description of the model geometry unnecessary. The only limitation by the comparatively small model domain is that we cannot examine in all cases the complete geometry of the HDI (hundred degree isotherm).

Fluid flow: Reviewer comment 6.6: Please explain the setups of the various background hydraulic gradients here or later (see next comment).

Authors reply: Please see our answer to point 6.7.

Reviewer comment 6.7: Also please explain how the variation is implemented. Figure 1b is not doing a good job explaining the variation. Is the BHG varying from the center towards a certain direction? Or from one "edge" of the model domain to the opposite one? Is the BHG a differential gradient in the reservoir or the entire cube? Since this seems to be such an important parameter, please try to be as precise as possible.

Also, please provide some geological scenarios that justify the chosen variations in hydraulic gradient.

Authors reply: The BHGs are valid for the whole model domain, i.e. the BHG is not varied in the individual models, but interacts with the artificially introduced flow field. The BHG is applied as a pressure gradient on the model boundaries. We will explore this in more detail and improve Figure 1b. Please see also our answer to point 1.2.

Scenarios: Reviewer comment 6.8: Line 127: At 2-3 km burial depth, a matrix permeability of 10-11 m2 (10 Darcy) seems a bit high and probably impossible, when combined with 3% or 14% porosity. Please discuss or at least think about removing the high-perm-low-poro scenarios (or give an adequate geological scenario). In general, please consider giving some real world analogs/examples for the chosen poro-perm scenarios. The sandstone reservoir literature should be full of good examples.

Authors reply: We improve the method section to clarify this misunderstanding and add that the permeability values are not linked, respectively provided/controlled by the matrix porosity. We used instead a continuum approach (Berkowitz et al., 1988; Lege et al., 1996; Kolditz, 1997), that uses a replacement media for the fractures and which provides mean hydraulic properties of a given fracture system. This is in our opinion a justified assumption, since permeability is in consolidated sediments often to large parts provided by fractures (Bear, 1993; De Marsily, 1986; Hestir and Long, 1990; Nelson, 1985). Please see also our answer to point 1.1.

Reviewer comment 6.9: Line 145-146: It would be nice to have some real-world justification for the chosen fault permeabilities. There is a lot of literature available.

Authors reply: We accept the suggestions of the reviewer and justify the chosen parameter values in the introduction. Please see also our answer to point 1.1.

Reviewer comment 6.10: Lines 149/150: Please provide some geological scenarios that justify the chosen variations in hydraulic gradient.

Authors reply: We follow the reviewer's suggestion and explore the topic in more depth in the introduction. Please, see our comment above to point 1.2.

7. Results:

Reviewer comment 7.1: Line 165/166: According to figures 2e & 2f, this is only true if the BHG is applied in the direction of the injection well (fig. 2f).

Authors reply: The reviewer is correct. Here we provide/describe the ranges of reservoir lifetimes observed in scenario 1, for different reservoir permeabilities. These ranges depend naturally also on the other parameters varied in our multi parameter sensitivity study. This is why we choose to present our results in different plots, e.g. lifetime vs, permeability, and lifetime vs direction of the hydraulic gradient.

Reviewer comment 7.2: Line 180: This makes sense, but how realistic is it to have a rock/sediment with a permeability of 10-11 m^2 and a porosity of only 5% or 14%?

Authors reply: Please see our answer to your comments to Line 127 (point 6.8).

Reviewer comment 7.3: Line 236: Why is the stabilization at 100°C?

Authors reply: In our study, we investigate the effect of multiple parameters; there are certain combination that can produce similar results, in this case the convergence to 100°C in Figure 2j, 6g, 7g, 8d, 8g, 9g. To analyse this in more depth, would require a different sensitivity study with a different setup. We will also modify the sentence to: "In the presented model runs as shown in Figure 4, temperatures stabilize at a final temperature of about 100°C."

Reviewer comment 7.4: Line 237: Wouldn't you expect a significant effect of convective flow in a vertical fracture?

Authors reply: We assume that this question is likely caused by the fact that we were not clear enough about how permeability is implemented. See also our answer to point 7.6 and 2.1. We will also rephrase the sentences in line 238-239 (discussion paper) to: "..., compared to the other directions, as common in fractured reservoirs (Figs. 1e, 5)." We did not introduce additional vertical fractures in this scenario, but increase the fracture anisotropy in the given plane. This question would be necessary to answer if we would have used a discrete fracture model. We are convinced that this question will be answered after we improved the method section regarding the implementation of permeability and porosity. Also in natural fracture systems the vertical extent of fractures is commonly restricted, i.e. many fractures stop at sedimentary contacts/layers and thus density-driven heat flow would be hindered in the vertical direction, as the reviewers agrees in point 2.1.

Reviewer comment 7.5: Line 253-254: Please rephrase or put more detail. What do you mean by: "a closed geothermal loop may not be feasible"?

Authors reply: We agree with the reviewer and rephrase the sentence to ". . ...the establishment of a closed geothermal system becomes unlikely."

Reviewer comment 7.6 Line 258: Not sure what we can really learn from this part, since many real-world projects have shown the significant impact of convection on the temperature field of fault-controlled reservoirs (e.g. Soultz-sous-Forets).

Authors reply: We discuss this limitation now. The main point will be: Whereas our models likely underestimate the lifetime of fault-related reservoirs that allow for convection, they allow for improved estimate of how strong the effect of convection should be to counteract the negative influence of the channelling effect. Thus, it shows the importance to know the budgets of both the channeling effect and the effect of density driven convection to make assumptions about their effect on the potential lifetime of a geothermal reservoir. Please see also our answer to point 2.1 and to the lines 363-368 of our manuscript, where we refer to a real world example of this observation.

Reviewer comment 7.7: Line 258f: What is the permeability of the matrix (host rock)?

Authors reply: See line 141f (Discussion paper). We agree with the reviewer and now

repeat the value of the bulk permeability of the host rock in this section to improve readability. Please note our answer to point 6.8 in which we clarify how permeability is implemented.

Reviewer comment 7.8: Line 291: ". . .BHG, does the temperature stays. . ."

Authors reply: We thank the reviewer and will correct the sentence.

8. Discussion

Reviewer comment 8.1: Line 313-328: Maybe this part would be much better placed in the introduction and in some parts in the "Scenarios"-part (see previous comments on mentioning analogs etc).

Authors reply: Our study is a non-site-specific sensitivity study (with simplified models). We use this part as an introduction in the discussion section to show how parameters such as porosity and permeability can be highly variable. We discuss that even comparatively small variations of these parameters have a strong effect on a reservoir's performance. Thus, we think that the structure of the manuscript, as it is, is justified. We are aware, however, that it would be possible to tell the story in a different way. However, now we will discuss in more detail the implication of figure 10 for our study and geothermal energy in general. Further, we will better explain in the Introduction the aim of our study. This will also include a point regards the variability of geological systems. See also our answer to point 1.1.

Reviewer comment 8.2: Line 335: How does the bottomhole pressure impact the influence of the BHG? In particular in the low-permeability case? Please mention earlier (e.g. in the Methods or Scenarios section(s)).

Authors reply: The influence of the bottomhole pressure on the BHG depends on the ratio between both. If the bottomhole pressure is higher than the BHG it dominates and vice versa. The low permeable cases are due to high bottomhole pressures unrealistic, but allow to investigate the effect of other parameters like permeability contrasts. In our opinion, these points are preferably placed in the discussion section. We, however, will state that some of the scenarios are unrealistic and that both fluid systems interact and we will specify that point in the method and discussion section. Please see also our answer to point 3.3.

Reviewer comment 8.3: Line 335f: Here is the answer of the last comment: “the BHG is outperformed by the artificial flow field caused by the very high bottomhole pressure”. Actually, the bottomhole pressures in the medium and low permeability cases are impossible in nature. The question is then, what is the meaning of the modelling results? An elegant way to avoid this problem would be to work with a constant draw-down instead.

Authors reply: We agree with the reviewer please see above. The low permeability/ high fluid pressure models underestimate the effects of the background hydraulic gradient. The importance of the findings of the low and medium permeability models as well as the use of the constant production and injection rates are justified above in points 3.1, 3.3, 3.6. We hope that answers the questions raised by the reviewer.

Reviewer comment 8.4: Line 361: Please consider providing some geological scenarios for variations in BHG.

Authors reply: We assume that this is a misunderstanding. We have not introduced variations of the BGH within one individual model, but we assigned different BHG to individual models. We improved the method section to clarify this issue. Please see our comments to points 1.2 and 6.7. We also, as requested, provide improved introduction regarding the BHG.

Reviewer comment 8.5: Line 379f: “Notably, in the low and intermediate permeable models, where permeability contrasts are higher than 1 order of magnitude, none of the tested BHG configurations could compensate for the small volume”. Or is this again related to the unnaturally high BHPs in the low and medium permeability scenarios? Please discuss.

Authors reply: We agree with the reviewer, and add that the unrealistic high bottomhole pressures do not allow the BHG to affect the system. We also correct the typo “higher than 1 order of magnitude” to “higher than 2 orders of magnitude”.

Reviewer comment 8.6: Line 387: instead of “borecore”: core from boreholes.

Authors reply: We thank the reviewer and we will correct the sentence accordingly.

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