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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.

Anonymous Referee #2

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The manuscript is well written, original and of interest. It is worthwhile to be published in Solid Earth Journal.

In the following, my (hope useful) comments and suggestions to improve the manuscript. General comments:

Figures in general have a small scale for (small) colored dots and a (uselessly) large scale for the vertical. Furthermore, the colors used are the same. This is misleading the reader. My suggestion is to use different color codes for the two parameters (depth and BHG) and change the relative dimensions of the two scales, since the focus of the manuscript is on the BHG (color-coded dots).

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Discussion paper

Figs.2a-c (as well as other corresponding plots) either have inverted y-axis scale (sic!), or I did not understand the figure and/or the text (cfr. lines 161-165). This produced some initial misunderstanding of the work (the text is not properly describing what is presented in the figure). In many experiments the temperature stabilizes at around 100°C (Figs. 2j, 6g,7g, 8d, 8g, 9g). The reason for this coincidence with the HDI not clear or explained. The author should justify this "convergence" in the various models.

In Fig.1a the projection of the wells provides the impression that their trajectory is oblique. The Author should either correct the figure or describe the reason for oblique wells as well as quantify it. In the Figures the Authors should include the number of experiments represented (i.e. the number of dots in the single figure). The author should discuss the case of a strong variation in the results (e.g. Fig. 1a, red dots for BHG=20 mm/m, at permeability 10-11). Numbers are very small to pretend some statistics (mean, sd), but they could mean unreliable results and should be discussed.

The "line connecting the same experiment" is not clear. In Fig. 2 the yellow line connect one yellow dot per figure, and it is easily understood. On the other hand, in other figures (e.g. Fig. 4a-b, green lines) the do connect multiple dots in the same figure. This is confusing: how many numerical experiments were responsible for each dot in each figure (I assumed one)? Maybe they partially overlap.

Specific comments

In lines 34-39 the Authors discuss the poor improvement in porosity due to the presence of fractures. This is true, but the author are not considering the main role provided by fractures in improving the effective porosity by connecting isolated pores, as is normally achieved in tight-gas reservoirs (gas-shale). In general, the manuscript is not discussing on the difference between total porosity and effective porosity. I guess that the porosity they consider in the numerical experiment is merely the effective one, and this should be clearly mentioned. On the other hand, a brief note on the role of influence of fractures on effective porosity is required to complete the introduction and the

discussion paragraphs.

Equations in Line 72,81,82 seem correct, yet references for the general audience (as Solid Earth also has) are required.

Line 91-92. The limit to 20m is not easy to be understood (e.g. where these 20m were located along the well). Maybe a better way to express this correction would be to express it as a percentage of the well hole surface (the cylinder), or by presenting the equivalent reduction in permeability between the well cell and the surrounding ones in the mesh.

Line 96: I guess that the geothermal gradient is in reality expressed by m and not by km... Line 105 and 113. I guess that "computational costs" really intends the more appropriate expression "computational time". It would be of interest to the readers to quantitatively justify this sentence: add in lines 69 71 information on the used computer platform and the approximated run-time for a single numerical experiment.

Line 126 and through all the experiments. My opinion is that a permeability of 10exponential-11 m2 is unfair to be reached in a reservoir at the used resolution of the model, with the exception of karst cavities. The Author might include here a descriptive correspondence to the reservoir permeability (e.g. tight reservoir for 10 exponential -15 m2, medium-high permeable reservoir 10 exponential -13 m2, karst structures 10exponential-15 m2).

Lines 143-147 more references on measure of permeability in fault core are important here (e.g. works by R.J Knipe and/or Q.J Fisher)

Line 161-165. As mentioned, the only way I found to correlate text and Fig2a-c is to invert the Y-axis scale. Anyhow the description, even with this correction, does not correlate for 10exponential-11 permeability experiments, that scatter results all along the entire span 0->200 a apparently without any rule (e.g. red dots). Did I understand properly the figure? If not, a more careful introduction to the figure and description

might be necessary.

In the Figs. the meaning of the represented surface is not completely described. The Authors refer to “HDI shape”. I am not sure but Iguess that, considering the experiments, these surfaces represent the envelope of the volume where the temperatures become lower than the HDI due to the successful heath extraction. An explanation on the meaning of the HDI shape is required in the text (and maybe in the caption for the fast readers. . .).

Line 173-174 the probability concept should be better introduced.

Line 178: I guess the Authors intend Fig.2g and not 2e.

Line 198 “three series”. This is not clear: I see in the figure3 different permeability (these are the three series), 4 permeability contrasts and 8 different orientation for BHG with 4 possible gradients, for total of 3x4x8x4=384 combinations. Then just three BHG shapes, but for the same permeability (same series). This might be confusing. A more complete description of the model procedure might help to understand the results.

Fig4b is not clear, and in general figs 2,4,5 are not easy figures. Same color dots appear both on high and very low times to breakthrough. This could mean the excessive scattering of results, or that results are from experiments with different, not specified, parameters. I think to have properly understood the relations between the dots in Fig. 4a,b and the reason for the limited connection presented in Figs. At the present stage , the figure is very difficult to be understood (also due to the high number of combinations in the experiments – i-e- the number of parameters used - and the limiting 2D of the journal pages. . .). The Author could try to improve the correlations by either using different symbols for each experiment (good luck, it would be a big effort with questionable results) or by adding a reference number to each dot. The diagrams have a relative small number of dots and al lot of empty space. A simpler alternative might be to add in the text the clear description of a correlation among dots as an exam-

ple. There are also some evident overlap of dots (just comparing among figures) and this should be described (or slightly move one of the dots within the resolution of the results).

Line 215-220 again: the cited 70 years seems to correspond to 130 years in Fig.4d, second column. Is there again reversed the Y-axis scale?

Line 235. As previously mentioned. Why at 100°C? This should be justified by the Authors Fig.5 the origin of dots on top of the plots a-c (i.e. at >200a) is not clear.

Line 264-265 Fig. 5g shows that temperatures stabilize at 100°C. How this happens at exactly the critical temperature chosen for the HDI? Is this input in the model? Some explanation is needed. Lines 340-349 Here is perhaps the proper space to discuss the total porosity and the effective one I discussed above. As I understand, the chosen porosity is intended to be 100% effective. A sentence explaining this should be anyhow added to the article.

Line 371. This assumption may be too forced, and I am sorry for the referenced articles. Secondary fractures and faulting allow permeability to take over thinner clay layers that lose their sealing property. This is more difficult in thicker clay layers. I understand that in the useful proposed model are necessary simplifications, but it is not the case for the complexity of real geothermal reservoirs. Line 399. I do not see evidence in Fig. 5b to justify this sentence. At my sight, the resulting timings are fully independent from the BHG values (colored dots). May be the Authors are referring here on the BHG orientation of Fig.5c.

Line 423-414 Fractures and secondary faulting associated to faults have generally various angles to the faults and only a minority lies parallel to it (cfr. Riedel). This results in: fracture intersections, fracture opening by the stress induced from the kinematics along the fault (friction). These factors guarantee the higher permeability of fault damage zone to a certain extent, as described in the literature. To be explicit: “often-observed” of “fault-parallel fracture anisotropy” does not correspond to either field outcrops and

cores across fault zones, apart from S-C structures, where in any case C planes are generally subordered in number to S ones, My suggestion is simply to eliminate the "often-observed" attribute.

Line 429 The previous concept is repeated here: useless redundancy and same comment.

Line434-435 the use of the terms "opposed/opposite" to indicate opposite (!) dipping is misleading. A rephrase would solve it.

Lines 62, 442: they were 1027 (from line 150). This is an interesting and serious number of runs and it would be effective to remark this number both in the introduction (say, "over one thousand numerical experiments") as well in the Conclusions "(1027)". My impression is that "large series" or "a series of" would be –alas – interpreted as much smaller number in present-day publish-or-perish scientific environment.

Line 457: This is not so simple. This sentence does not take in consideration the improvement of the effective porosity that is induced by fracturing that in turn may be enhanced by the oriented stress that develops in presence of strong BHG. Since the point about effective porosity changes is not taken into consideration in the presented models, my suggestion is to specify this in the sentence (referring to "in many cases" might be not sufficient).

Line 459-462. On the contrary, results from this work well represent the first step to model real, complex geothermal reservoirs with their Stochastic modelling by adding in the mesh the proper random values! And I am sure that the "computational costs" at that stage will be an insignificant obstacle. This might be a further point and a better conclusion to your article (follow the Hollywood-movie style: end always your articles with a true, positive sentence on your results. . .)

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