**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.**
**Referee #3 Owen Callahan**

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Dear Owen Callahan,

We thank you for your review. In the following response, we will answer the questions and concerns raised by you.

| | **General comments:** | |
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| **1.1** | Model parameters, including selection of porosity-permeability combinations, length of model duration, selection of 100 C isotherm, are not sufficiently justified, and may not be relevant to operating geothermal fields. | We consider feasible ranges of all parameters, including values below and above typical benchmark parameters. This is prerequisite for a sensitivity study. From the combination of all the results, we determine particular parameters and their values that exert control on the geothermal reservoir. The necessity of this approach was provided and explained by us in our response to R1 point 3.3.<br><br>Porosity and Permeability<br>In accordance to R1 and R2, we have now improved the introduction and introduce the values for permeability, porosity earlier in the text. We discuss how these compare to typical values in different geothermal settings. We now explain better the modelling approach that we use.<br><br>100°C and threshold<br>See also points 2.2, 2.10, 2.25:<br>A universally applicable (economic) threshold cannot exist, because of the different site-specific demands of geothermal power plants e.g., district heating, electricity generation, output, depth of the reservoir. The 100°C isotherm or the 100°C threshold must be arbitrary, at least to some degree, even not taking into account that it must somehow balance with the model duration. We chose 100°C because it is sometimes referred to as minimum temperature that allows electricity production with |

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| | | binary cycles (e.g., Bhatia, 2014; Buness et al., 2010; Erec, 2004; Huenges, 2010; Mergner et al., 2012). We have rephrased the sentence and improved references.<br><br><u>Model duration</u><br>points 2.9 and 2.18:<br>There are two points to be made here. Firstly, we do not investigate the lifespan of hydrothermal power plants, but rather the role of individual reservoir parameters on the thermal development of geothermal reservoirs (see line 53-54 SED). Secondly, there is a balance between threshold temperature and duration of the model. If, as requested by the reviewer, we had chosen 150°C, then the effect of the parameters on the thermal lifetime, would be less clearly shown. If we, in addition, had only run the models for 40-50 years, then the majority of the model runs would not reach the important/higher threshold, i.e. there would be no data to show. For instance in scenario 1 (Fig. 2a, b, c) and 2 (Fig.4a, b, d, e, g, h) we could not identify the impact of the different parameters.<br><br>We strongly disagree that this study is not relevant to operating fields. The model results give an indication of the importance of different hydrogeological parameters on the lifetime of geothermal reservoirs, and even if the modelled lifetime of the reservoir exceeds the lifetime of a geothermal power plant, the relative importance of different parameters remains the same. |
| **1.2** | Use of references and citations is inconsistent. In some cases, statements with long lists of references are too vague to be useful (i.e. not clearly tied to particular geothermal fields or a specific type of inquiry (numerical, field, experimental: : :)) and in other cases the listed references do not seem appropriate for citing in their current context. | We feel that there is some room for improvement. However, we disagree that the references are too vague.<br>See our comments to your detailed criticism below. |
| **1.3** | The structure of the paper fails to emphasize the role of BHG nor does it discuss enough real world scenarios were the impact of BHG, or even suspected impact of BHG, can be shown. As it stands, | We strongly disagree. The results, the discussion section, and the conclusions contain a sufficient information about the BHG; in our opinion, balanced together with the other parameters. The only part that can be improved with regards to the BHG is the introduction. This was our answer to the reviews by R1 and R2. |

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| | almost all of the conclusions are about BHG, but BHG only gets 3 lines in the introduction. | That we cannot discuss the effect of BHG for a large amount of real world scenarios is because of, to our best knowledge, the lack of data and case studies in the literature. BHG has not been considered in equivalent studies in literature before our paper.<br>We appreciate that the reviewer likes our findings regarding the BHG. We will improve the introduction concerning the BHG. We, however, disagree that almost all of the conclusions are about the BHG, all of the conclusion points emphasize the important role of permeability and permeability heterogeneity as well. The BHG - even though you agree that this is an important parameter- is an underestimated parameter. It is, however, still just one of the parameters that we investigated in our manuscript, and its ranking in the modelling, as a whole, needs to be understood. |

## Specific comments

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| **2.1** | Line 11: This sentence neglects economic factors. Rather than "can be exploited" maybe describe geologic factors influencing economic viability, as you do in the introduction. | Our manuscript considers geological reasons for geothermal lifetime. Our manuscript does not focus on economic factors and we have therefore removed any reference to this subject in the introduction. |
| **2.2** | Line 17: 100°C isotherm is not well justified. See additional comments below. | This is the abstract of our manuscript, we here report solely the threshold we use and are convinced that any justification of the 100°C isotherm at this place would be misplaced.<br>See also point 1.1. |
| **2.3** | Line 29: The first few lines of this paragraph make it seem like these references pertain to hydrothermal settings specifically. In this current configuration, Laubach et al. (2009) does not seem like an appropriate reference as they do not describe fracture patterns in hydrothermal systems, nor do they explicitly describe the impact of fractures on permeability or volume (other than tangentially) but rather compare fracture and mechanical stratigraphy. | This line introduces the difficulties to predict reservoir properties. This is the case of permeability. Permeability is commonly provided by fractures. In Laubach et al. (2009), they point out that fracture patterns are difficult to predict (and therefore also permeability). Thus, we are convinced that citing Laubach et. al., 2009 here is justified.<br><br>For instance, Laubach et al. (2009) wrote:<br>"*In subsurface studies, current mechanical stratigraphy is generally measurable, but because of inherent limitations of sampling, fracture stratigraphy is commonly incompletely known. To accurately predict fractures in diagenetically and structurally complex settings, we need to use evidence of loading and mechanical property history as well as current mechanical states.*" |
| **2.4** | Line 32: Manning and Ingebrigtsen (1999) concerns theoretical permeability at the crustal scale and in metamorphic rocks in particular. The link between this reference and the statement are again tenuous unless more clearly explained. | Here we write that permeability and porosity in general are rock properties that are highly heterogeneous, independent of rock type.<br><br>Manning and Ingebritsen (1999) wrote:<br>"*Near the Earth's surface, permeability exhibits extreme spatial variability (heterogeneity) and anisotropy, both among geologic units and within particular units".*<br>Thus, in our opinion, the reference is justified.<br><br>However, we will rephrase the sentence accordingly: |

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| | | They are, independent of rock type, often highly heterogeneous because of layering, localized fracturing, and diagenesis (e.g., Aragón-Aguilar et al., 2017; De Marsily, 1986; Lee and Farmer, 1993; Manning and Ingebritsen, 1999; Zhang, 2013). |
| **2.5** | 2.5.1 Line 37-39: The logic here is odd. You describe high porosity in sedimentary geothermal systems, then say fracture porosity in sedimentary rocks is low (are dam sites really the best analog, i.e. Snow, 1968?), but that fractures dominate geothermal systems.<br><br>2.5.2. Separately these statements may all be true, but fractures commonly dominate in geothermal systems because geothermal systems are commonly not hosted in sedimentary rocks.<br><br>2.5.3. Also, you may want to specify “clastic” sedimentary reservoirs, as fractures can be very significant contributors in carbonate rocks. | 2.5.1: We do not understand the logic of the reviewer's comment. We wrote that fractures have a dominant control on rock permeability in geothermal reservoirs, even though their contribution to bulk porosity is negligible compared to matrix porosity. We are convinced that we have communicated this correctly in our discussion paper.<br><br>*Snow (1968) is highly appropriate in this case, because*<br>*(1) Snow (1968) analysed fracture porosity in different rock types,*<br>*(2) The fact Snow (1968) used outcrops at dam sites is highly relevant here, because the known permeability at the dam sites were an asset to calculate the fracture porosity.*<br><br>2.5.2: We strongly disagree that geothermal systems are commonly not hosted in sedimentary rocks. There is a large number of examples worldwide for geothermal systems in sedimentary rocks (see Moeck, 2014). We also strongly disagree with the reviewer’s point that fractures only play a minor role in geothermal systems hosted in sediments. For instance in the Upper Rhine Graben, the permeability is controlled by fractures in lithified sedimentary rocks (Meixner et al., 2014; Egert et al., 2018).<br><br>2.5.3: We disagree. This study could be used for both clastic and carbonate reservoirs. The range of parameters used in this study covers both cases. Fractures dominate most deep geothermal systems. |
| **2.6** | Line 42-43: The statement about specific failures needs referencing. | We are aware of this issue. However, this is tricky, since such negative examples are commonly not published in scientific literature. In our experience links to webpages on failed projects disappeared over time. Nevertheless, we will provide links to websites, if possible. |
| **2.7** | Line 45: Beall (1994) does not appear to be about declines in production fluids nor fault damage zones, but rather to be about tracer tests and what can be learned about fluid saturations. | We have deleted the reference to Beall (1994). |

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| **2.8** | Line 48-50: BHG is a huge part of your overall paper but has a tiny role in the introduction. This should be much larger, with specific examples of where it has impacted production. It could be your primary hypothesis and seems like the major contribution, but it is not firmly established in the introduction. As it stands, the introduction does not lay the necessary foundation for the paper, not establish a clear hypothesis, but it could be reworded to emphasis BHG (see comments about 363-368). | Please see our answer to your general comment (point 1.3) above.<br><br>In accordance to R1 and R2, we have improved the introduction regarding the BHG. However, we are convinced that, even though the BGH is important, that the introduction as well as the other parts of the manuscript should remain balanced regarding the investigated parameters.<br><br>We strongly disagree that the aim of our manuscript was not sufficiently communicated.<br>In lines 51-56 (discussion paper), we did provide the objective of our manuscript.<br><br>We, as requested by R1 and R2, have modified this part and included more details. |
| **2.9** | Line 58: The lifespan of 200 years in not well justified. This is longer than the nominal lifespan of geothermal powerplants (which may be closer to 30-50 years). Furthermore, most of your graphs show major deviations between scenarios early in the life of the model. I'd change the approach and the figures (graphs) to emphasize time frames that are more relevant to plant economics. | See our answer to point 1.1. |
| **2.10** | Line 61: Regarding 100°C as a threshold. On cursory examination, I did not find reference to this number (which seems very low and rarely economic unless the system is particularly shallow, productive, or in a great market) in the DiPippo volume. Instead, look into Bertani (2005) for some examples of typical producing (and presumably economic) values. Furthermore, I would expect major economic and | We do not consider efficiency loss. We also do not carry out an economic feasibility study; see our objective. In our introduction (discussion paper), we communicate that we carried out a sensitivity study in which we investigate the influence of petrophysical and other parameters on the thermal development of geothermal reservoirs. Thus, the points addressed by the reviewer are not the focus of our manuscript.<br>See our answer to point 1.1. |

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| | efficiency loss well before your production temperature declined from 150 to 100°C. Bertani, R. (2005). World geothermal power generation in the period 2001–2005. Geothermics, 34(6), 651-690. 10.1016/j.geothermics.2005.09.00 _ | |
| **2.11** | Line 66-68: Consider emphasizing BHG instead of all the others. | We have, according to the comments by R1 and R2, rewritten the last section of the introduction, with focus on the objective of our study. In our opinion, the presentation and discussion of the results is well balanced concerning the different investigated parameters. |
| **2.12** | Line 94-95: The issue with well spacing seems to distract from BHG, until you specifically related the impact of BHG on effective well spacing. The introduction of parameters overall could take more care. | With our manuscript, we do not concentrate solely on the BHG. We present a sensitivity study in which we examine different parameters for their importance. One is well spacing. No changes needed.<br>Again, the reviewer draws all the attention to the BHG. In addition, the effect of the BHG on well distance is made. We described it in short but appropriately and with the possible details in lines 187 – 197 (discussion paper). |
| **2.13** | Line 97: Change to 0.047C/m-1 | We thank the reviewer and corrected the typo. |
| **2.14** | Line 97: Is a linear gradient throughout justified? In higher permeability systems you may expect isothermal reservoirs. | Numerous studies have shown that a linear geothermal gradient is a good first order approximation for temperatures that are determined by heat conduction only. The initial temperatures in the model represent temperatures that are undisturbed by fluid flow, and therefore can be represented by a linear geothermal gradient. In some high permeability systems, thermal convection or topography-driven flow could affect background temperatures to an unknown degree. However, the focus of the paper was to explore the effect of induced fluid flow between the injection and production well on subsurface temperatures. Using a different initial geothermal gradient for different parameter sets would make it difficult to compare the different model runs. |
| **2.15** | Line 117-119: You have a high geothermal gradient given limited vertical advection. Perhaps this study really is best described as analogous to hot sedimentary aquifers, rather than more conventional fault-fracture hydrothermal systems? I don't recall seeing this distinction. | Our model scenarios describe both situations, i.e., we have model runs for geothermal reservoirs with fracture anisotropy, faults and for layered sedimentary aquifers. The geothermal gradient that we use is relatively high, but not unusual. The reason for not varying the geothermal gradient for the different model scenarios is discussed in the reply to the previous point. |

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| **2.16** | Line 127-129: Is the combination of porosity of 14% and a permeability of 10-15m2 realistic? | To carry out a sensitivity study, we also need to combine different parameter values, even if they are sometimes unrealistic. This is inevitable in a one at a time sensitivity study. The base case value of a permeability of 10-13 m2 and a porosity of 14% is certainly realistic. We did not consider co-varying porosity and permeability in our sensitivity study.<br>We understood also from the comments from R1 and R2 that we had to improve our methods section regarding this matter. We have now modified it and describe how permeability and porosity is implemented. |
| **2.17** | Line 140: A 7 m wide fault core is quite large. Can you include references to justify this model parameter? | 7m is wide, but not unusual; see for instance Childs et al. (2009). Furthermore, we chose to model the fault core with this thickness to avoid the high computational cost of very fine meshes. At any rate, the thickness of the fault core is somewhat irrelevant, because the fault core was modeled as an impermeable unit. |
| **2.18** | Line 154: Again, the model time of 200 years, while perhaps arbitrary, is not particularly relevant to producing geothermal fields. | Please see our answer to point 1.1 |
| **2.19** | Line 193-197: This is an interesting finding, but it is lost in the paper because the structure is not set up as a test of the influence of BHG compared to other parameters (see lines 66-68). Couching this section in terms of BHG would bring more coherence to the results and discussion. | We thank the reviewer.<br>We investigated many more parameters and we do not agree that this point is lost. Instead, we feel that our manuscript is well balanced when discussing the contributions of BHG but also the other parameters that were included in the sensitivity study.<br>see also point 2.11 |
| **2.20** | Line 202: 10-15 m2 seems very low for a sandstone with 14% porosity. Better geologic constraints on parameter space would make the results more defensible (see notes Line 720). | Please see our answers to points 1.1 and 2.16. |
| **2.21** | Line 317: There seems to be a disconnect between statement and reference here. I don't think Alava et al. (2009) discuss porosity or permeability, and if it is a different parameter they describe it should | We have rephrased this part accordingly.<br>Instead of:<br>The variability of these and other petrophysical parameters increases with scale (Alava et al., 2009; Freudenthal, 1968; Krumbholz et al., 2014a).<br>We now write: |

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| | perhaps be clearly specified separately instead of grouped with other references. | The variability of these (Freudenthal, 1968; Krumbholz et al., 2014a) and other (petro)physical parameters (Alava et al., 2009; Lobo-Guerrero and Vallejo, 2006) increases with scale. |
| **2.22** | Line 337: Although bottom hole pressures exceeding lithostatic may not be unreasonable, it is not clear that your model responds to these conditions by fracturing, nor would this condition be favorable (or even permissible) in a permitted injection well. Constraining your model space to geologically reasonable conditions would make the results more useful. | The model does not include any fracturing, i.e., lithostatic pore pressures only affect fluid flow, and not permeability or porosity.<br>Constraining the parameter space to sub-lithostatic pore pressures would result in a loss of information, because either parameters would have to be varied together (i.e., adjusting injection rate along with permeability) which would make it much more difficult to compare models and to isolate the effect of a single parameter |
| **2.23** | Line 342: Aren't pores and fractures always filled with fluid? | We agree with the reviewer and will delete "commonly" in Line 342. |
| **2.24** | Line 342. "Since pore space often exceeds: : :" is not needed in this argument, as you say "high porosity" later in the sentence. The "since" statement is distracting, as there are many counter examples. | We will rephrase the sentence. |
| **2.25** | Line 348. Again, regarding parameter space, if 10-13 m2 is the threshold, why bother with the very low permeability cases? | See our answer to point 1.1. |
| **2.26** | Line 363-368: This passage makes the point that your models considering BHG are important, but it needs to be expanded, and more rigorously explored and cited (there should be many examples of fields that target outflow zones for reinjection and upflow zones for production). I'd also consider moving a version of this into the introduction when you describe the importance of BHG. | Regarding the many examples: we are not aware of many published examples of geothermal fields that discuss or report BHG. See also our answer to point 1.3. However, we will improve the introduction regarding the BHG. The BHG, as our study shows, cannot be analysed or ranked as a standalone parameter, it must be seen in combination with other parameters. |
| **2.27** | Line 388: Check "metre" for journal style. | We used British style English throughout the manuscript, as allowed by the Journal. |
| **2.28** | Line 406: "scales" to "scale" | Done |

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| **2.29** | Line 411: I would either cite or change this first statement. | The statement is, in our opinion, sufficiently referenced after the following sentence. See Line 412-413 in discussion paper. |
| **2.30** | Line 411-424: Another and significant reason there is an interest in fault zones is that fault zones are fundamental parts in many producing geothermal fields because they provide the necessary vertical permeability and advection of heat and fluid so that high temperatures are shallow enough to be economically exploited. I think your passage misses this by focusing on the complexities of faults instead of the constraint that many fields and models will by necessity involve faults. | We thank the reviewer for the suggestion and will add a statement about faults as thermal anomalies. However, we consider the effects of the fault on the reservoir itself and do not consider the possible thermal anomaly that allows for a shallower exploitation (see line 117-118 SED). |
| **2.31** | Line 439-441. This passage is probably not necessary. | We disagree, we think it is important to discuss or least mention the restriction of our study. |
| **2.32** | Line 445. Although the ranges may be real, the combination of ranges seem less plausible. | The combination of ranges may seem less plausible, but this combination was necessary to see the effects of individual parameters, and is a standard approach in one-at-a-time sensitivity analysis. See point 1.1. |
| **2.33** | Line 472: There is an extra space resulting in a broken link. | Corrected |
| **2.34** | Line 648 (Figure 2 g). Please consider a shorter time span and temperature range. The timespan of 200 years and wide range in T (40-180°C) masks the more relevant changes early in the lifespan of a well or geothermal field.<br>Furthermore, smaller drops in temperature would nonetheless have major impacts on plant efficiency. This comment applies even more to your fault-controlled models that show major changes in the first few years. | See our answer to point 1.1. |

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| **2.35** | Line 720 (Figure 10). It would be nice to see these plotted together as x-y, so you could support your use of 14% porosity and low permeability. Because this is described as a more generic model, might it also make sense to show values from other geothermal fields producing in sedimentary basins? | This is not possible, because the data are derived from several publications. Same region, but different places. In addition, most of the data are not linked (with the exception of Bauer et. al. (2017)). The purpose of this figure is to show just how variable rock properties are.<br>See point 1.1. |

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