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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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Bauer et al. (2019) present results of a sensitivity analysis exploring the impact of various geologic parameters (porosity, permeability, sedimentary layering, faults, and fracture anisotropy), well separation, and background hydraulic gradient (BHG) on breakthrough time of and shape of a 100°C isotherm in a generic sedimentary geothermal system. The results lay out a convincing argument that BHG is a significant and perhaps under appreciated aspect of geothermal field longevity, although the manuscript in its current form does not establish the importance of BHG early enough, nor does it stay focused on BHG as a central part of the hypothesis, and therefore loses some

impact.

General comments (see specific comments for details)

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.

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.

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, almost all of the conclusions are about BHG, but BHG only gets 3 lines in the introduction.

Specific comments

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.

Line 17: 100° C isotherm is not well justified. See additional comments below.

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.

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.

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. Separately these statements may all be true, but fractures commonly dominate in geothermal systems because geothermal systems are commonly not hosted in sedimentary rocks. Also, you may want to specify "clastic" sedimentary reservoirs, as fractures can be very significant contributors in carbonate rocks.

Line 42-43: The statement about specific failures needs referencing.

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.

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).

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.

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 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.2005.09.005

Line 66-68: Consider emphasizing BHG instead of all the others.

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.

Line 97: Change to 0.047°C/m-1

Line 97: Is a linear gradient throughout justified? In higher permeability systems you may expect isothermal reservoirs.

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.

Line 127-129: Is the combination of porosity of 14% and a permeability of 10-15m2 realistic?

Line 140: A 7 m wide fault core is quite large. Can you include references to justify this model parameter?

Line 154: Again, the model time of 200 years, while perhaps arbitrary, is not particularly relevant to producing geothermal fields.

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.

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).

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 perhaps be clearly specified separately instead of grouped with other references.

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.

Line 342: Aren't pores and fractures always filled with fluid?

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.

Line 348. Again, regarding parameter space, if 10-13 m2 is the threshold, why bother with the very low permeability cases?

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.

Line 387: Suggest changing "borecore" to "core" or similar.

Line 388: Check "metre" for journal style.

Line 406: "scales" to "scale"

Line 411: I would either cite or change this first statement.

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.

Line 439-441. This passage is probably not necessary.

Line 445. Although the ranges may be real, the combination of ranges seem less plausible.

Line 472: There is an extra space resulting in a broken link.

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. 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.

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?

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