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Peer Review File

Permeability Partitioning through the Brittle-to-Ductile Transition and its Implications for Supercritical Geothermal Reservoirs

Editorial note: Parts of this Peer Review File have been redacted as indicated to remove third-party material where no permission to publish could be obtained.

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

This paper was interesting to read and provides a set of rare experiments because of the complexity of achieving these conditions in the laboratory. The methods employed are interesting and the results are clear, and despite the small number of experiments, reasonably consistent with each other. Experiments such as these are novel and the implications for permeability of ductile rocks has been demonstrated experimentally, which is also novel. The examination of the partitioning of strain and permeability sheds light on the experimental results. The work supports the claims made in the discussion, however there is some inconsistent terminology and instances of lack of clarity that need to be addressed. I have also pointed out that the objectives of the research should be clarified in the introduction. In addition, there is additional detail needed for the methodology to ensure understanding of the interpretations as well as reproducibility of the results. All of these have been pointed out in the following:

1. There are a number of small typos. Please refer to annotated pdf for these.
2. Line 113: specify that the samples were deformed axially.
3. Lines 113-114: the statement "corresponding to conditions of the brittle to ductile transition in granite" is ambiguous, since it refers to all statements prior to it. For example, the strain rate is not related to the conditions of the brittle to ductile transition.
4. Figure 1, caption: specify that the thick lines in panel D are moving averages (I assume) of the thinner lines. Specify how many points are averaged (5, 15, etc.?)
5. Lines 119-122: the description of the stress-strain curves is not well linked to the figures. First, the yield point is not shown on the figures, which would help with the link. Second, normally I would take the yield point at the point of deviation from the linear-elastic deformation, and beyond this point I would not say that stress has "…a rapid rise to a maximum stress…". This sentence should be reworded.

6. Line 125: for the statement "… a more compliant…" than what? than for the samples at 600 and 800 degrees? Try to rephrase this statement without the comparative term, otherwise, specify what is being compared.
7. Lines 124-128: the deformation of samples at 200, 400 and 800 degrees are explicitly described, but 600 is not. Please add a description of the sample at 600 degrees.
8. Lines 140-141: the statement "All brittle samples…" would include the sample at 600 degrees, which was classified as brittle in line 120. Please modify this statement, as well as the statement in lines 141-142, which uses the plural "specimens" implying that the 600 degree sample is ductile, in contradiction to line 120.
9. Line 152: the term "semi-ductile" is used here, which is inconsistent with the use of the term "ductile" elsewhere (note that "semi-ductile" is also used in line 132). Please select the preferred term and use it consistently.
10. Lines 167-168: it is not evident from Figure 2D what the grain crushing is. Is it the cracks surrounding grains and subgrains that can be seen in the blue box labelled 6?
11. Figure 2: the panels are labelled with lowercase letters, whereas in the text these are given as uppercase letters. The caption does not describe what the blue boxes are; this is especially important for boxes 1, 3, 4 and 6, which appear in two different planes on the vertical and radial sections. The caption does not specify that the left and right panels are radial sections, while the middle panel is the vertical section; regardless of the red and orange annotations, the caption should also contain this information so the reader is sure of what they are looking at.
12. Lines 170-171: it is not clear what is meant by "…curved around a trend plane…"
13. Lines 174-176: without annotations on Figure 2, it is hard for the reader to know what is being referred to here. Making use of the blue box might help. Or does this refer to the ruptures sleeve in the lower left of the left panel of Figure 2D? Or is it the upper right of the middle panel of Figure 2D?
14. Lines 177-185: I would like to see some quantification of the cracks to complement the qualitative descriptions. This could be done using image analysis programs such as ImageJ (Fiji). After reading the supplementary material, I see that ImageJ has already been used in this project, therefore please use this to provide quantification of the changes in the fracturing.
15. Lines 192-195: is this a general statement or does it refer only to the semi-ductile

sample(s)? The first clause of this statement should be first highlighted in Section 2.1, possibly inserted at line 119. Then the second clause, starting at "suggesting" could be here. The use of the word "does" in line 193 makes it seem like the word "not" is missing. If this statement truly does mean that the initial permeabilities are similar, then please remove the word "does". What is meant by similar here? The initial permeabilities range from 2x10-19 to 6x10-19. Do you consider these as similar or dissimilar? I do not see how the fact that the initial permeabilities being similar suggests that confining pressure suppresses thermal cracking. I am missing the causality. Please expand, perhaps by using references that have shown thermally-induced cracking at low or 0 confining pressure (e.g. Mordensky et al., 2019).

16. Lines 198-200: what are you referring to by "this transition"? the brittle to ductile one? A transition related to the deformation? Reword this sentence, because as it is now, the "this" refers to "deformation", which I don't think is your intention.

17. Lines 200-203: this again highlights the need to clarify which samples are considered brittle and which are considered (semi-)ductile. You have made a separate between the samples at 200 and 400 from those at 600 and 800, but the mechanical distinction is not clear from the stress strain plots. This indeed is an interesting point worth exploring: the mechanical behaviour would group the samples at 200, 400 and 600 degrees together, but the permeability would group the samples at 200 and 400 degrees separately from those at 600 and 800 degrees. I would like to see a discussion of this.

18. Line 203: the way this statement is currently worded, it implies that permeability at 800 degrees soars 30-fold after macroscopic failure. However, the permeability increases consistently (albeit in steps) over the duration of the test, and since there is no failure, this should be stated in this sentence. I would replace the word "soar" with "increase".

19. Line 207: this is the first time the word "fault" has been used. Are you referring to the deformation bands? Either define what the fault is in the context of this experiment, or use consistent terminology if this is referring to the DBs.

20. Line 212: what are the global coordinates? How do they relate to the directions of loading (I assume z is axial, therefore based on a radial image?) and the dip directions of the DB? Is there nothing to say about similarity/difference of the permeability in the x, y and z directions? Why are they so consistent for all but one, or maybe 2, sub-volume(s) (4 and 5)? Note that Callow et al (2020) and Pengg et al (2014) were both on porous media, which

should have evenly distributed pores, whereas this exercise was done on localized fractures, does this affect the consistency of the results in the x, y and z directions? What do the differences tell us about flow localization in the 800 degree sample compared to the 200 degree sample?

21. Line 232: provide a sentence stating that the computed permeability for the 200 and 800 degrees specimens are compared in Table 2 to highlight the differences between the lowest and highest temperatures, or some other segway to explain why only two specimens are given, while this exercise was completed for all four.

22. Lines 234-236: since permeability is given in m2 in the figures and elsewhere, and is most commonly given this way, please do not give it in μm2. Also, sometimes the words "sub-volume containing the" are used and sometimes not. I would suggest simplifying all sub-volumes to "DB", "damage zone" and "bulk" and using this consistently throughout.

23. Table 1: please refer to this table in the text, perhaps at the end of line 232. Please give the permeability in m2. The caption says "marks in Figure 2". Specify that these are the blue squares.

24. Line 238: replace "brittle" with "200 ◦C specimen". Replace "permeability of the damage zone remains comparable to its brittle counterpart" with "permeability of the damage zones of both samples are similar".

25. Lines 239-242: rewrite to: "On the other hand, the permeability in the bulk in the 800 ◦C specimen comparable to that of its DB and its damage zone, and is an order of magnitude greater than the permeability in the bulk in the 200 ◦C specimen."

26. Lines 255-257: the computed permeability only shows a little over one order of magnitude difference between the DB and bulk, hardly enough to say that one is permeable and the other is impermeable. I would use different terms here, perhaps "less permeable" for the bulk would be sufficient.

27. Line 258: the word "wide" to describe the DB in the 800 ◦C specimen is not substantiated. In Figure 2 the SB in the 200 ◦C specimen is similar width to the DB in the 800 ◦C specimen.

28. Lines 258-260: the DB for the 800 ◦C specimen has a higher permeability than sub-volumes 1, 2 and 6, so the statement "as permeable as the brittle, sharp fracture" should be "more permeable than the DB in the 200 ◦C specimen".

29. Line 264: but the permeability of the damage zone for the 800 ◦C specimen, sub-volume

5, is much lower than the other sub-volumes, so the permeability is clearly quite heterogenous, which is not what is implied here.

30. Lines 298: be careful using the word “impermeable” in such a general statement. The bulk rock referred to could be anything, including a sandstone. Either specify that this statement refers to low permeability rocks, like intrusives or metamorphics, or use “less permeable” to describe the bulk.

31. Line 308: I would remove the word “preferential”, since the strain is still localized in the DB, it’s just that some of it is also occurring in the bulk.

32. Lines 328-331: this statement should come at the beginning of the manuscript, and form one of the key motivations of this work. Throughout the manuscript I have been asking myself: so what? How could these findings be applied in developing a supercritical geothermal field. Indeed, the text mentions that more work is needed to link these experiments with that question. Which leaves us with: what is the impact of this work? It would be better for this work to be framed not in the sense that it would be great to generate permeability at depths and temperatures corresponding to supercritical fluids, but rather to challenge this cutoff limit. Then the reader has an answer to the “so what?” questions from the beginning and can frame this research in that sense. I would suggest putting this somewhere after line 64. Then in the discussion, you could make must stronger statements about showing that permeability can indeed be developed at the BDT, and actually, that permeability can be really good!

33. Lines 333-334: this suggests that all experiments were conducted under semi-ductile conditions, which is contradictory to how some experiments are considered representative of brittle conditions.

34. Line 339: again, be careful of the word “impermeable”. Indeed, to anyone looking to extract fluids from these rocks would call them impermeable, even once fractured, since they are well below the exploitable permeability threshold. It would be better to say something like “less permeable”. Also, you say that this fracture controls permeability, but since in these experiments the fracture and bulk are in series, then the bulk controls permeability, not the fracture. Of course, in another setting, flow parallel to this fracture would be significantly higher, as could be calculated using the cubic law rather than Darcy’s law.

35. Line 340: again, remove “preferential”

36. Lines 341-342: again, the word “permeable” here follows the same rules as for the comment above. None of these permeabilities are “permeable” in the fluid extraction sense, and conversely, even something with permeability of 10-22 is permeable, since fluid can flow through it. It would be better to say something like “similar permeability” or “only slightly lower permeability”.
37. Line 346: it would be worth noting, either here, or even at the beginning of the conclusions, that you demonstrated that in fact, rocks in the semi-ductile regime can have deformation-induced increased permeability.
38. I am not sure if seismicity should be highlighted here. This is not really a conclusion of the work, it’s really only a benefit of targeting more ductile rocks for geothermal exploitation.
39.
For the supplementary material:
1. Lines 35-39: please describe how the confining pressure, pore pressure and axial load (stress) were applied. Were they applied sequentially, as is inferred from this text, or were they applied simultaneously, eg. while maintaining some ratio?
2. Line 86: what is the meaning of the word “horizontal” here? The figure shows radial images, which for a specimen standing on its end, would also be a horizontal slice. But are all slices horizontal? If so, then what is the meaning of x, y and z directions for the permeability? Surely for this you needed three orthogonal slices? The orientation of the slices needs to be explained further, as also pointed out for the main text.

References
Mordensky, S. P. et al. Increasing the Permeability of Hydrothermally Altered Andesite by Transitory Heating. Geochem Geophys Geosystems 20, 5251–5269 (2019).

Reviewer #2 (Remarks to the Author):
The article is very well written. The applied laboratory methods are on world-leading level and the research itself is very interesting. The reviewer have no doubts on the results itself and they should be published in this high-ranked journal.

However, from my point of view one very relevant point is not discussed in the manuscript. Figure 3 is a good example. The results the authors get in the laboratory (3B) are transferred without any comment/argument/elaboration to interpret a behavior at the field scale. There is a huge uncertainty with this transformation from one scale (cm-scale) to the other (km-scale). This so-called "upscaling" is here very crucial. I strongly recommend that the authors implement some extra-lines of text to tackle/discuss this issue.

Reviewer #3 (Remarks to the Author):

The paper proposes a revolutionary idea: deformation in a distinct fracture results in a less permeable structure than semi-brittle deformation leading to a more distributed zone of small fractures and deformed rock. This deviates from a long-held believe that more ductile deformation would result in lower permeable structures, such that the brittle-ductile transition would also be a boundary between permeable, potentially productive geothermal reservoirs with distinct fractures as fluid pathways and less permeable structures limiting exploitation of geothermal fluids.

The study presented here is thus remarkable and challenges our traditional thinking.

I personally think that the relevance for our understanding of rock deformation and its effect on permeability is greater than for geothermal energy, as the exploitation of supercritical geothermal fluids faces many other challenges, not least their rare occurrence at drillable depths. Thus the relevance for geothermal energy is somewhat overstated in the manuscript. I don't think this is needed.

The experimental and analytical techniques are innovative and sound. These are great experiments and really exciting results that definitely deserve publication in Nature Communications.

The critical point to be discussed, however, is the limited number of experiments. With one rock sample for each temperature, potential sample-to-sample variability and ranges of results are not accounted for. Repeated experiments at identical conditions are needed to validate the results and corroborate the claims made here, especially when quantitative results are considered.

The discussion of the permeability distribution in the sample and the way it is measured experimentally shows a critical issue, that is only partially resolved by the permeability analysis from digital images. The permeability in the samples is really heterogeneous. If the fractured zone in the colder samples does not connect the two ends of the sample, the quantification of permeability differences between the samples is not directly measurable, as mentioned in your discussion. This limitation needs a bit more critical evaluation, including the limitations of deriving permeabilities from image analysis (not just the effect of pressure).

Nonetheless, the general observation of increased overall permeability in apparently less fractured rock is still an amazing implication that just needs a bit stricter discussion of the assumptions and limitations of the applied methodologies. It definitely deserves to be published soon.

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

This paper was interesting to read and provides a set of rare experiments because of the complexity of achieving these conditions in the laboratory. The methods employed are interesting and the results are clear, and despite the small number of experiments, reasonably consistent with each other. Experiments such as these are novel and the implications for permeability of ductile rocks has been demonstrated experimentally, which is also novel. The examination of the partitioning of strain and permeability sheds light on the experimental results. The work supports the claims made in the discussion, however there is some inconsistent terminology and instances of lack of clarity that need to be addressed. I have also pointed out that the objectives of the research should be clarified in the introduction. In addition, there is additional detail needed for the methodology to ensure understanding of the interpretations as well as reproducibility of the results. All of these have been pointed out in the following:

1. There are a number of small typos. Please refer to annotated pdf for these.

We have addressed the typos highlighted in the pdf and would like to thank Reviewer 1 for pointing them out.

2. Line 113: specify that the samples were deformed axially.

The text has been modified accordingly.

3. Lines 113-114: the statement "corresponding to conditions of the brittle to ductile transition in granite" is ambiguous, since it refers to all statements prior to it. For example, the strain rate is not related to the conditions of the brittle to ductile transition.

The rheological behavior of a rock sample results from the interaction between pressure, temperature, stress and strain rate. At a given temperature and pressure, a rock can exhibit either localized or ductile behavior depending on the strain rate. It has been shown that higher strain rates cause sample embrittlement.This is also why rocks must be subjected to higher temperatures in the laboratory compared to field conditions to account for the difference between tectonic ($10^{-12/14}$) and experimental strain rates ($10^{-4/7}$). However, we acknowledge that our original statement was ambiguous and have reworded it to read: "... corresponding to conditions under which granite displays localized to semi-ductile behavior in the laboratory ...".

4. Figure 1, caption: specify that the thick lines in panel D are moving averages (I assume) of the thinner lines. Specify how many points are averaged (5, 15, etc.?)

Reviewer 1 is correct; the thicker lines are moving averages of 8 data points. This has now been explicitly specified in the caption of Figure 1.

5. Lines 119-122: the description of the stress-strain curves is not well linked to the figures. First, the yield point is not shown on the figures, which would help with the link. Second, normally I would take the yield point at the point of deviation from the linear-elastic deformation, and beyond this point I would not say that stress has "…a rapid rise to a maximum stress…". This sentence should be reworded.

We agree with Reviewer 1 and have simplified the description to focus on the important aspects. Since we do not use the yield point in the rest of the study, we have omitted it to avoid confusion. The revised sentence now reads: "Within the temperature range of 200 to 600°C, Lanhélin granite exhibits a mechanical behavior typical of brittle localized deformation, with stress increasing with strain until reaching a maximum value (Q_{max}) before failure."

6. Line 125: for the statement "… a more compliant…" than what? than for the samples at 600 and 800 degrees? Try to rephrase this statement without the comparative term, otherwise, specify what is being compared.

We have now specified that this statement is in comparison to the sample deformed at 600˚C. This comparison is crucial, which is why we decided to keep it in the manuscript.

7. Lines 124-128: the deformation of samples at 200, 400 and 800 degrees are explicitly described, but 600 is not. Please add a description of the sample at 600 degrees.

The description of the behavior of the sample deformed at 600˚C is similar to that of the samples deformed at 200 and 400˚C. Consequently, their descriptions have been grouped together, with the only difference being the initial elastic response of the apparatus. We have clarified this in the revised paragraph.

8. Lines 140-141: the statement "All brittle samples…" would include the sample at 600 degrees, which was classified as brittle in line 120. Please modify this statement, as well as the statement in lines 141-142, which uses the plural "specimens" implying that the 600 degree sample is ductile, in contradiction to line 120.

We agree that this paragraph was generally confusing, so we have completely rewritten it to better emphasize the difference in permeability evolution.

9. Line 152: the term "semi-ductile" is used here, which is inconsistent with the use of the term "ductile" elsewhere (note that "semi-ductile" is also used in line 132). Please select the preferred term and use it consistently.

This has been corrected. Semi-ductile is now consistently used throughout the manuscript to describe the behavior of the sample deformed at 800˚C.

10. Lines 167-168: it is not evident from Figure 2D what the grain crushing is. Is it the cracks surrounding grains and subgrains that can be seen in the blue box labelled 6?

We replaced "grain crushing" with "cataclasis" to avoid any confusion. This process of cataclasis is localized within the deformation band. and this has now been explicitly mentioned in the text.

11. Figure 2: the panels are labelled with lowercase letters, whereas in the text these are given as uppercase letters. The caption does not describe what the blue boxes are; this is especially important for boxes 1, 3, 4 and 6, which appear in two different planes on the vertical and radial sections. The caption does not specify that the left and right panels are radial sections, while the middle panel is the vertical section; regardless of the red and orange annotations, the caption should also contain this information so the reader is sure of what they are looking at.

We agree with Reviewer 1. We greatly expanded the caption of Figure 2 in order to explain all its elements. We also changed the labels of the panels to capital letters to match the references in text. The blue boxes indicate the sub-volumes where permeability has been computed, all of which appear on the vertical plane of the sample. Additionally, some volumes appear on the radial plane as well.

12. Lines 170-171: it is not clear what is meant by "…curved around a trend plane…"

For clarity, we reworded this sentence.

13. Lines 174-176: without annotations on Figure 2, it is hard for the reader to know what is being referred to here. Making use of the blue box might help. Or does this refer to the ruptures sleeve in the lower left of the left panel of Figure 2D? Or is it the upper right of the middle panel of Figure 2D?

We now explicitly direct the reader to the left side of the left panel in Figure 2D, where the ruptured jacket is visible. We chose not to reference the blue boxes, as they indicate the sub-volumes used to compute permeability later in the manuscript, and we believe this might cause additional confusion.

14. Lines 177-185: I would like to see some quantification of the cracks to complement the qualitative descriptions. This could be done using image analysis programs such as ImageJ (Fiji). After reading the supplementary material, I see that ImageJ has already been used in this project, therefore please use this to provide quantification of the changes in the fracturing.

We agree with Reviewer 1. To supplement our discussion, we carried-out additional sophisticated image analysis to quantify crack length and aperture. A summary of this analysis is presented in Table 1 and discussed at length in Section 3.2.

15. Lines 192-195: is this a general statement or does it refer only to the semi-ductile sample(s)? The first clause of this statement should be first highlighted in Section 2.1, possibly inserted at line 119. Then the second clause, starting at "suggesting" could be here. The use of the word "does" in line 193 makes it seem like the word "not" is missing. If this statement truly does mean that the initial permeabilities are similar, then please remove the word "does". What is meant by similar here? The initial permeabilities range from 2x10-19 to 6x10-19. Do you consider these as similar or dissimilar? I do not see how the fact that the initial permeabilities being similar suggests that confining pressure suppresses thermal cracking. I am missing the causality. Please expand, perhaps by using references that have shown thermally-induced cracking at low or 0 confining pressure (e.g. Mordensky et al., 2019).

Following Reviewer 1's comment, we made adjustments to the statement. We moved the first half of this statement to Section 2.1. It now reads: "The initial permeability of all samples (i.e., after pressurization and thermal loading, but before axial deformation) remains between 2 and 6×10^{-19}. There is no trend between initial permeability and applied temperature (Figure 1 D.).

We reworded the second part of this statement. In fact, our point was not that the similarity in values was hinting towards an inhibition of thermal cracking under pressure, but rather that a lack of correlation between initial permeability and applied temperature did. In fact, at room or low confining pressure, the permeability of granite is expected to increase with increased applied temperature due to thermal cracking. But in our case, we do not see such an effect, hinting at an inhibition of thermal cracking at 100 MPa confining pressure. This is now explained at length in the discussion and supported by citations, including that suggested by Reviewer 1."

16. Lines 198-200: what are you referring to by "this transition"? the brittle to ductile one? A transition related to the deformation? Reword this sentence, because as it is now, the "this" refers to "deformation", which I don't think is your intention.

The sentence was reworded and now reads: "When compared to the rock samples' initial permeability, the permeability data clearly showcases the impact of the transition from localized to semi-ductile behavior on the deforming specimens' hydraulic properties."

17. Lines 200-203: this again highlights the need to clarify which samples are considered brittle and which are considered (semi-)ductile. You have made a separate between the samples at 200 and 400 from those at 600 and 800, but the mechanical distinction is not clear from the stress strain plots. This indeed is an interesting point worth exploring: the mechanical behaviour would group the samples at 200, 400 and 600 degrees together, but the permeability would group the samples at 200 and 400 degrees separately from those at 600 and 800 degrees. I would like to see a discussion of this.

We reworded the description of the results and no longer group the samples; hence we believe this comment is no longer germane.

18. Line 203: the way this statement is currently worded, it implies that permeability at 800 degrees soars 30-fold after macroscopic failure. However, the permeability increases consistently (albeit in steps) over the duration of the test, and since there is no failure, this should be stated in this sentence. I would replace the word “soar” with “increase”.

We no longer mention failure as it was imprecise. We also replaced soar with “continuously increase” to improve clarity.

19. Line 207: this is the first time the word “fault” has been used. Are you referring to the deformation bands? Either define what the fault is in the context of this experiment, or use consistent terminology if this is referring to the DBs.

We thank Reviewer 1 for pointing this mistake out. We now consistently use the term deformation band.

20. Line 212: what are the global coordinates? How do they relate to the directions of loading (I assume z is axial, therefore based on a radial image?) and the dip directions of the DB? Is there nothing to say about similarity/difference of the permeability in the x, y and z directions? Why are they so consistent for all but one, or maybe 2, sub-volume(s) (4 and 5)? Note that Callow et al (2020) and Pengg et al (2014) were both on porous media, which should have evenly distributed pores, whereas this exercise was done on localized fractures, does this affect the consistency of the results in the x, y and z directions? What do the differences tell us about flow localization in the 800 degree sample compared to the 200 degree sample?

Global coordinates have been added to Figure 2. We acknowledge the Reviewer's point that permeability might differ in all three modeling directions, though our sub-volumes do not seem to fully capture this variation. This could be due to the sub-volumes' size. However, as Reviewer 1 noted, some variations exist, likely due to localized flow caused by sample-specific and very local microstructural features. To properly address the question of flow localization, we would require sample-scale flow modeling, which is currently impossible due to the computational intensity of the models; current calculations for one sub-volume taking several days. Therefore, acknowledging Reviewer 1’s opinion that permeability anisotropy might induce confusion, we have decided to show permeability only in the z direction (the direction recorded in the deformation rig) to support our discussion. We have not removed permeability values for the other directions altogether, but moved them to the supplementary material in Table S2.

As Reviewer 1 mentioned in their previous comment, other crack quantification techniques could help support the qualitative description of the microstructure. Hence, we conducted further image analysis (as mentioned in our previous reply), specifically calculating the total crack length and average crack aperture in the sub-volumes. Our rationale is that these two scalars are measured (as opposed to the permeability which is modelled) and indicative of the intensity of the damage while simultaneously being less sensitive to potential effects of the reduced sampling volume of the 3D sub-volumes. Combined with the permeability computed in the z direction in Table 1, we believe it now gives the reader a fuller picture of the microstructure.

21. Line 232: provide a sentence stating that the computed permeability for the 200 and 800 degrees specimens are compared in Table 2 to highlight the differences between the lowest and highest temperatures, or some other segway to explain why only two specimens are given, while this exercise was completed for all four.

We included a clarification in the manuscript, specifying that the calculation was performed solely for the two end-member cases. Due to the computational complexity of the modeling, it required significant cluster time, leading us to restrict our analysis to the samples deformed at 200 and 800 degrees.

22. Lines 234-236: since permeability is given in m2 in the figures and elsewhere, and is most commonly given this way, please do not give it in µm2. Also, sometimes the words "sub-volume containing the" are used and sometimes not. I would suggest simplifying all sub-volumes to "DB", "damage zone" and "bulk" and using this consistently throughout.

We homogenized our units to m^2 and corrected the text to use simpler references to "DB", "damage zone" and "bulk".

23. Table 1: please refer to this table in the text, perhaps at the end of line 232. Please give the permeability in m2. The caption says "marks in Figure 2". Specify that these are the blue squares.

We added a reference to Table 2, corrected the units and the Table caption as suggested.

24. Line 238: replace "brittle" with "200 ◦C specimen". Replace "permeability of the damage zone remains comparable to its brittle counterpart" with "permeability of the damage zones of both samples are similar".

We changed the text as suggested.

25. Lines 239-242: rewrite to: "On the other hand, the permeability in the bulk in the 800 ◦C specimen comparable to that of its DB and its damage zone, and is an order of magnitude greater than the permeability in the bulk in the 200 ◦C specimen."

We modified the text accordingly.

26. Lines 255-257: the computed permeability only shows a little over one order of magnitude difference between the DB and bulk, hardly enough to say that one is permeable and the other is impermeable. I would use different terms here, perhaps "less permeable" for the bulk would be sufficient.

We agree with the reviewer comment. We changed the text for "less permeable".

27. Line 258: the word "wide" to describe the DB in the 800 ◦C specimen is not substantiated. In Figure 2 the SB in the 200 ◦C specimen is similar width to the DB in the 800 ◦C specimen.

Here, we must kindly disagree with Reviewer 1. The DB in the semi-ductile sample is substantially wider than the one in the localized sample. In fact, in Figure 2, blue box 4, the DB takes almost the entire width of the box which is 3.9 mm in size. On the other hand, The DB in the 200°C appears to be less than 1 mm wide across the whole sample. These quantifications were now added explicitly to the manuscript.

28. Lines 258-260: the DB for the 800 ◦C specimen has a higher permeability than sub-volumes 1, 2 and 6, so the statement "as permeable as the brittle, sharp fracture" should be "more permeable than the DB in the 200 ◦C specimen".

We corrected this statement.

29. Line 264: but the permeability of the damage zone for the 800 ◦C specimen, sub-volume 5, is much lower than the other sub-volumes, so the permeability is clearly quite heterogenous, which is not what is implied here.

Permeability is indeed slightly lower in subvolume 5 and appears anisotropic, with the x-direction being less permeable than other directions. However, we would like to highlight the magnitude of this difference. While the difference in permeability between the bulk of the two specimens is an order of magnitude, the variation here is minimal and falls within the margin of error (20%, see Supplementary S4). Our modelling approach is imperfect and could therefore lead to such slight discrepancies.
As we mentioned in our previous reply, we decided to move the permeability values in the x and y directions to supplementary material so as to not create potential confusion. We retain the value in the vertical direction as it is the one which we recorded in the deformation apparatus. We complemented those vertical permeability data with additional characterization of the crack network (length and aperture) so as to illustrate the different geometries of the crack networks (please refer to our previous reply).

30. Lines 298: be careful using the word "impermeable" in such a general statement. The bulk rock referred to could be anything, including a sandstone. Either specify that this statement refers to low permeability rocks, like intrusives or metamorphics, or use "less permeable" to describe the bulk.

We agree that this statement was imprecise, we opted for "less permeable".

31. Line 308: I would remove the word "preferential", since the strain is still localized in the DB, it's just that some of it is also occurring in the bulk.

We removed the word "preferential".

32. Lines 328-331: this statement should come at the beginning of the manuscript, and form one of the key motivations of this work. Throughout the manuscript I have been asking myself: so what? How could these findings be applied in developing a supercritical geothermal field. Indeed, the text mentions that more work is needed to link these experiments with that question. Which leaves us with: what is the impact of this work? It would be better for this work to be framed not in the sense that it would be great to generate permeability at depths and temperatures corresponding to supercritical fluids, but rather to challenge this cutoff limit. Then the reader has an answer to the "so what?" questions from the beginning and can frame this research in that sense. I would suggest putting this somewhere after line 64. Then in the discussion, you could make must stronger statements about showing that permeability can indeed be developed at the BDT, and actually, that permeability can be really good!

We agree with Reviewer 1. We have reworked the introduction of the manuscript in order to reframe the problematic we are raising. Line 77, it now reads "For these reasons, the brittle-to-ductile transition is widely considered a critical threshold for hydrothermal circulation in the crust (Juppe et al., 2004, Violay et al., 2015, 2017) which not only bears implications for the exploitation of geothermal resources, but also for all crustal processes involving the circulation of fluids. We also added an extra paragraph at the end of the introduction which reads: "We demonstrate that permeability can be created in deforming semi-ductile rocks and that the resulting diffuse strain can lead to greater equivalent permeability than in brittle, localized rocks. Most importantly, we challenge the long established belief that the BDT marks a cutoff for the circulation of fluids in the crust."

33. Lines 333-334: this suggests that all experiments were conducted under semi-ductile conditions, which is contradictory to how some experiments are considered representative of brittle conditions.

We changed this statement to better reflect the conditions of our experiments. It now reads: "We carried out a comprehensive study of permeability in deforming samples of Lanhélin granite under conditions spanning brittle to semi-ductile conditions".

34. Line 339: again, be careful of the word "impermeable". Indeed, to anyone looking to extract fluids from these rocks would call them impermeable, even once fractured, since they are well below the exploitable permeability threshold. It would be better to say something like "less permeable". Also, you say that this fracture controls permeability, but since in these experiments the fracture and bulk are in

series, then the bulk controls permeability, not the fracture. Of course, in another setting, flow parallel to this fracture would be significantly higher, as could be calculated using the cubic law rather than Darcy's law.

We corrected this sentence and removed the word "impermeable" as well as any mention of control. It now reads: "Indeed, we showed that, in the brittle regime, strain is mainly accommodated on a sharp fracture which in turns concentrates permeability in an otherwise less permeable bulk rock."
Furthermore, it's important to note that while we compute the permeability of a sample here, the effective permeability of the rock mass will heavily rely on the number of fractures present, rather than solely on the permeability of individual fractures and the bulk permeability.

35. Line 340: again, remove "preferential"

We corrected this sentence.

36. Lines 341-342: again, the word "permeable" here follows the same rules as for the comment above. None of these permeabilities are "permeable" in the fluid extraction sense, and conversely, even something with permeability of 10-22 is permeable, since fluid can flow through it. It would be better to say something like "similar permeability" or "only slightly lower permeability".

We corrected this statement. It now reads: "Conversely, we showed that, in the semi-ductile regime, the partitioning of strain towards the bulk lead to the formation of a more permeable deformation band characterized by highly damaged rock embedded in a equally permeable damaged bulk."

37. Line 346: it would be worth noting, either here, or even at the beginning of the conclusions, that you demonstrated that in fact, rocks in the semi-ductile regime can have deformation-induced increased permeability.

We added a statement to that avail line 346.

38. I am not sure if seismicity should be highlighted here. This is not really a conclusion of the work, it's really only a benefit of targeting more ductile rocks for geothermal exploitation.

We agree with Reviewer 1 here and we removed any mention of seismicity in semi-ductile reservoirs.

39.
For the supplementary material:
1. Lines 35-39: please describe how the confining pressure, pore pressure and axial load (stress) were applied. Were they applied sequentially, as is inferred from this text, or were they applied simultaneously, eg. while maintaining some ratio?

We greatly expanded the description of the sample loading procedure. The text now explicitly mentions the order of application of the stresses, specifically, first the confinement pressure, followed by the pore pressure, the thermal load, and finally, the deviatoric load.

2. Line 86: what is the meaning of the word "horizontal" here? The figure shows radial images, which for a specimen standing on its end, would also be a horizontal slice. But are all slices horizontal? If so, then what is the meaning of x, y and z directions for the permeability? Surely for this you needed three orthogonal slices? The orientation of the slices needs to be explained further, as also pointed out for the main text.

We apologize for the confusion. Here, we do not refer to "slices" as shown in Figure 2 or Figure S1, which are 2D radial images of the sample. Instead, we are referring to 3D radial layers. These layers result from dividing the scanning of the sample into six stages along its vertical axis (due to the maximum vertical aperture of the X-ray beam). As described in supporting text S2, during each stage, the sample was rotated 360 degrees before the platen supporting the sample was raised, and the process was repeated. To avoid further confusion, we have completely removed the term "slice." Additionally, we have clarified the x, y, and z coordinates in the main text and on Figure 2, as mentioned in our previous reply.

References:

Mordensky, S. P. et al. Increasing the Permeability of Hydrothermally Altered Andesite by Transitory Heating. Geochem Geophys Geosystems 20, 5251–5269 (2019).

This reference is now included in the manuscript

Reviewer #2 (Remarks to the Author):

The article is very well written. The applied laboratory methods are on world-leading level and the research itself is very interesting. The reviewer have no doubts on the results itself and they should be published in this high-ranked journal.

We would like to extend our gratitude to Reviewer 2 for their words of encouragement. It was greatly appreciated.

However, from my point of view one very relevant point is not discussed in the manuscript. Figure 3 is a good example. The results the authors get in the laboratory (3B) are transferred without any comment/argument/elaboration to interpret a behavior at the field scale. There is a huge uncertainty with this transformation from one scale (cm-scale) to the other (km-scale). This so-called "upscaling" is here very crucial. I strongly recommend that the authors implement some extra-lines of text to tackle/discuss this issue.

We acknowledge Reviewer 2's point that our approach could have been perceived as lackluster. In order to address this problem, we have now added an entire paragraph (lines 335-351) discussing the upscaling of our results. By the means of the equivalent permeability presented in Heap et al., 2019, we now estimate the effect of semi-ductile rheology on the equivalent permeability of a reservoir.

Reviewer #3 (Remarks to the Author):

The paper proposes a revolutionary idea: deformation in a distinct fracture results in a less permeable structure than semi-brittle deformation leading to a more distributed zone of small fractures and deformed rock. This deviates from a long-held believe that more ductile deformation would result in lower permeable structures, such that the brittle-ductile transition would also be a boundary between permeable, potentially productive geothermal reservoirs with distinct fractures as fluid pathways and less permeable structures limiting exploitation of geothermal fluids. The study presented here is thus remarkable and challenges our traditional thinking.I personally think that the relevance for our understanding of rock deformation and its effect on permeability is greater than for geothermal energy, as the exploitation of supercritical geothermal fluids faces many other challenges, not least their rare occurrence at drillable depths. Thus the relevance for geothermal energy is somewhat overstated in the manuscript. I don't think this is needed. The experimental and analytical techniques are innovative and sound. These are great experiments and really exciting results that definitely deserve publication in Nature Communications.

We express our gratitude to Reviewer 3 for their positive feedback and are thrilled to read of their recognition of the relevance and impact of our work within the rock deformation community. However, we maintain the belief that it is important to contextualize our findings within the framework of supercritical geothermal reservoirs. Over recent years, particularly following the discovery of supercritical resources in Iceland, supercritical geothermal energy has emerged as a frontier for the industry. The growing interest in fluid circulation at the brittle-ductile transition is evidenced by significant publications, such as those by Watanabe et al. (2017, Nature Geoscience), and the proliferation of supercritical projects (e.g., Japan Beyond Brittle, Taupo Volcanic Zone, Newberry).

Additionally, we agree with Reviewer 3 that our study does not address all the challenges that supercritical geothermal energy will face in the future, and it is important to emphasize this point. Consequently, we have revised the last paragraph of section 3.4 to read: "Overall, the exploitation of supercritical fluids at depth still faces many challenges. In this context, our study provides only an initial perspective on their feasibility and questions the previously prevailing notion that the BDT represents a cutoff limit for hydrothermal circulation in the crust."

The critical point to be discussed, however, is the limited number of experiments. With one rock sample for each temperature, potential sample-to-sample variability and ranges of results are not accounted for. Repeated experiments at identical conditions are needed to validate the results and corroborate the claims made here, especially when quantitative results are considered.

We acknowledge Reviewer 2's concern about the relatively limited number of experiments presented in our study. However, as noted by the other reviewers, these experiments are exceptionally challenging to conduct.

Under different circumstances, we would have been willing to re-run experiments. Unfortunately, TARGET suffered a critical breakdown a few weeks before receiving the present feedback. Specifically, the intensifier regulating the confining pressure jammed, and the mobile piston regulating the volume of the intensifier welded in place under extreme stresses. After two months working around the clock to disassemble the intensifier, it appears irreparably damaged (see attached picture). This has been a significant setback for our lab, as procuring a new intensifier, which costs hundreds of thousands of dollars and takes months to be delivered, is now necessary.

Nevertheless, we believe our dataset is robust, exhibiting a clear permeability trend with temperature over four experiments. Additionally, we have recently published a study demonstrating the robustness of both the mechanical and permeability data obtained from our apparatus by benchmarking it against data from other research groups (see Figures 5 and 8 in Meyer at al., 2023).

Under these circumstances, and given the solid trend and demonstrated reproducibility of our machine, we would greatly appreciate if Reviewer 3 would reconsider their opinion.

[figure redacted]

Figure 1: Picture taken of the unmounted intensifier. The upper part of the piston can be seen on the left while the lower part is jammed into the pressure chamber (blue).

The discussion of the permeability distribution in the sample and the way it is measured experimentally shows a critical issue, that is only partially resolved by the permeability analysis from digital images. The permeability in the samples is really heterogeneous. If the fractured zone in the colder samples does not connect the two ends of the sample, the quantification of permeability differences between the samples is not directly measurable, as mentioned in your discussion. This limitation needs a bit more critical evaluation, including the limitations of deriving permeabilities from image analysis (not just the effect of pressure).

Following Reviewer 3's advice, we greatly expanded our discussion of the limits of the two methods used and how we addressed them. It now reads: "In summary, conducting *in-situ* permeability measurements provided us with a unique scalar representing the apparent, unidirectional permeability of the sample as a whole. This permeability value can be seen as a lower bound value for permeability in granite under conditions representative of the BDT, which does not fully capture the possible heterogeneity (e.g., permeability in a fracture) or anisotropy (e.g., preferential crack orientation) in the sample. Conversely, *post-mortem* microstructural analysis enabled us to grasp the spatial complexity of permeability within the samples. It could be leveraged to calculate the permeability within particular sub-volumes of the samples and appreciate potential differences and anisotropy. However, this method can only be applied on *post-mortem* samples, and as such, under pressure and temperature conditions that are not representative of the subsurface. Overall, it was only through the combined application of both methods that we could attain a comprehensive understanding of permeability, a multifaceted physical property more accurately characterized as a tensor rather than a scalar."

Nonetheless, the general observation of increased overall permeability in apparently less fractured rock is still an amazing implication that just needs a bit stricter discussion of the assumptions and limitations of the applied methodologies. It definitely deserves to be published soon.

We thank Reviewer 3 for their positive opinion of our work.

References:

Heap, M. J., Troll, V. R., Kushnir, A. R. L., Gilg, H. A., Collinson, A. S. D., Deegan, F. M., Darmawan, H., Seraphine, N., Neuberg, J., and Walter, T. R. (2019). Hydrothermal alteration of andesitic lava domes can lead to explosive volcanic behaviour. Nature Communications, 10(1):5063.

Watanabe, N., Numakura, T., Sakaguchi, K., Saishu, H., Okamoto, A., Ingebritsen, S. E., and Tsuchiya, N. (2017b). Potentially exploitable supercritical geothermal resources in the ductile crust. Nature Geoscience, 10(2):140–144.

Meyer, G. G., Acosta, M., Leclere, H., Morier, L., Teuscher, M., Garrison, G., Schubnel, A., and Violay, M. (2023). A new high-pressure high temperature deformation apparatus to study the brittle to ductile transition in rocks. Review of Scientific Instruments, 94(4):043903.

REVIEWERS' COMMENTS

Reviewer #1 (Remarks to the Author):

I am happy with the revised manuscript. The authors did a great job of addressing my comments and, in my opinion, it is ready to be accepted as it is.

Marlene Villeneuve

Reviewer #2 (Remarks to the Author):

Dear Authors, thanks for your revised manuscript. You have addressed my main point.

From my point of view your manuscript is now ready to be published.

Reviewer #3 (Remarks to the Author):

I am happy with the thorough and serious revision the authors have performed. I also accept the rebuttal to my own comments, even though some aspects can still be debated. But this is more at a level of different opinions rather than erroneous arguments.
I therefore wholeheartedly recommend the revised manuscript for publication.