Review History for "An open source FEM code for solving coupled thermo-poroelastoplastic processes"

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Summary

All reviewers considered the work to be of high value for the community. During the review process an implementation issue was revealed that has now been corrected.

Review Round 1

Reviewer 1: Ayman Abed

General comments

This paper presents a framework to solve coupled thermo-hydro-mechanical problems using the finite element method. The author(s) take advantage of the powerful FEniCSx platform to solve the involved balance equations and made the developed Python script(s) used to communicate with FEniCSx freely available for users. The authors targeted applications related to subsurface energy production and storage operations, but the framework can be modified to basically covers other types of coupled geomechanical problems.

The paper is clear, well-written, and structured. For the scientific content, I found the contribution very interesting for the researchers working on coupled processes and numerical implementation and thus relevant to the readers of Open Geomechanics. I recommend the publication of this paper with minor revisions.

In what follows, I provide the Authors with minor comments to improve the completeness of their work.

Specific Comments

1. When I tried to run the provided script, I initially got an error in the definition of Macauly brackets which states that module ufl has no attribute 'Max'. I changed 'Max' to 'max_value' then it worked.

```
def macaulay_bracket(self,f):

    return ufl.max_value(f,0.0)/f
```

2. In the caption of Figure 3 it is stated that 'Rock dilation at yield reduces pore pressure (and, thus, effective radial stress). I think it should be 'Rock dilation at yield reduces pore pressure (and, thus, effective radial stress increases).

3. Could you please elaborate more about the calculated stress path in Figure 3 for undrained triaxial compression and why we have a linear reduction in effective mean pressure during elasticity? Normally we do not expect such behaviour under constant volume (undrained conditions) at least for the linear elastic-perfectly plastic type of models.

4. Were the 3D meshes, used in the full-scale simulations, subjected to a mesh-convergence (H-convergence test) study? How would the refinement affect your conclusions?

5. How did you model the horizontal wellbores?

6. In general for Rock failure predictions one is advised to use Hoek–Brown failure criterion or even the more common Mohr-Coulomb failure criterion. Is there any reason for choosing Drucker-Prager yield criterion?

Reviewer 2: Thomas Poulet

General comments

This paper presents a new THM simulator using the Finite Element Method to solve the coupled problem with a monolithic solution strategy using Fenicsx. The implementation is checked against some displacement- and temperature-controlled benchmarks and two applications are presented in the contexts of geothermal energy and carbon geological storage.

The manuscript is well structured and easy to read. It explains succinctly the main aspects of the formulation and briefly the numerical implementation. It correctly presents some benchmarking tests against analytical solutions and discusses a couple of relevant applications. Overall, I found the approach interesting but recommend some revisions, mainly to transform the pitch into a more convincing argument for this work.

Major comments

- The authors need to better present and justify the uniqueness of their approach. The fact that most codes are not adaptable enough or tightly-coupled enough is unfortunately not sufficient to justify this new simulator if there exists even one code that already meets those criteria. Making the cut within a short list of codes is not enough by itself to warrant publication so the need for this specific new code needs to be made clearer. The wording should then be more specific and not state for instance l.63-65 that "many" other codes are limiting or "may not" consider elastoplasticity rigorously. The comparisons need to be more specific and cover some of the codes the authors might have in mind when they avoid stating that "all codes" are missing the features targeted here. This might include the PorousFlow [Wilkins et al., 2020] module of the MOOSE platform [Lindsay et al., 2022].

- With standard equations for the momentum, mass and energy balance equations, and with applications retrieving expected results, the novelty mainly comes from the numerical approach, which then deserves a few more explanations and justifications. For instance, the choice of a mixed formulation for the pressure and Darcy velocity should be explained (l.199), along with the way in which pore pressure boundary conditions are imposed weakly (l.210), or why an Euler implicit time integrator is used (l.213), since it is not unconditionally stable for such a non-linear system. Why not an explicit one, or a higher order implicit one?

- For the benchmarks, the mesh size and the numbers of degrees of freedom used should be mentioned, with a mesh sensitivity analysis to illustrate the convergence, rather than simply stating (l.280) that "finer load and mesh discretization would decrease this error".

- Comparing against analytical solutions is great but errors around 2% (l.279) are way too large to convince the reader of the code performance and the word "accurately", employed in many places, is not really justifies.

- A monolithic approach with so many variables might be particularly expensive, so this should be mentioned explicitly and discussed as part of the justification for this publication. Can the code reach errors that are orders of magnitudes lower against analytical solutions within reasonable computational time? How does that compare to existing codes (whether they're more adaptable or not)?

- How is the monolithic tight-coupling improving accuracy and stability over other codes (if that's one of its purposes)? Can the code provide smoother and less unstable results for on figure 7 (and with which computational resources and time)?

Minor comments

- The governing equations don't account for gravity, yet the gravitational body force appears later.
- Eq 12, the ratio should be inside the gradient. With the porosity strain taken into account, why isn't the permeability varying as well?
- Eq 14, the boundary conditions don't belong in the functional space definitions. The functions are constrained on the boundaries (e.g. on), not the test functions (e.g. on). The proper boundary conditions should appear with Eqs. 15-19.
- Eqs.15-19, ds and dx are not defined (noting that the symbol s is already used to denote entropy in eq.3...)
- Fig. 2a: why is there a kink at 5% axial stress in the linear elasticity part?
- The last paragraph of the introduction presents briefly all sections except section 3 that's not mentioned.
- Mentions of "tight-coupling" might be preferred to "full" coupling (as one can always introduce more couplings, even though I am myself guilty of having employed that term in the past...).
- The Einstein notation should be mentioned for clarity.

I found the study really interesting and hope that the main points mentioned can be addressed to increase this impact of this work.

References

Lindsay et al. 2022
Wilkins et al., 2020

Reviewer 3: Jean-Michel Pereira

General comments

The manuscript aims at presenting the main equations and a few validation cases of a new FEM code for THM reservoir simulations accounting for elastoplastic behaviour of the geomaterials. The tool builds on an open-source FEM code and implements specific functionalities for targeted applications. The examples provide insight into the governing phenomena at play in geothermal applications. Overall, the tool and its presentation are valuable for the readership in the sense that it is an open-source fully coupled FEM solver allowing future users to implement their own (and specific) elastoplastic models. However, the manuscript cannot be accepted in its present state. The authors are invited to correct or clarify some issues, as detailed hereafter.

Major comments

Constitutive equations

- Eqs. (1-3): The authors acknowledge that these equations neglect heat generated by high strain rate. This is fine. However, the occurrence of plastic strain also generate heat and increase the entropy of the system. Eq. (3) should include that component associated with plastic dissipation. More details and clarifications are expected.

- Eqs. (1-3): There is some inconsistency here. The authors might consider working (and thus writing these equations) with increments (in the sense of change, but not necessarily small change) for stresses, pore pressure and temperature to deal with non-zero initial conditions. For the thermal problem, it is clear that the coupling terms involving temperature are meant for temperature changes. For stresses and pore pressures, a choice has to be made but in any case, the authors should clarify what is the stress and pressure state corresponding to the non-deformed configuration of the domain (zero strain).

Drained vs undrained conditions

- l.114: The authors might consider explaining this thermal pressurisation using undrained conditions as a limit case. I understand that the authors want to stick to realistic conditions, using high and low permeabilities to reach those drained/undrained limit cases but working out the constitutive equations in those (ideal) cases is interesting for illustration purposes. As an illustration, the authors are invited to demonstrate how the equation (4) is obtained using the constitutive equations and restraints in terms of volume or fluid mass changes.
- l.282: If the authors refer to the total stress path, this is obvious since that latter is imposed. If the authors refer to the effective stress path, then they should make it clear, adding that the effective stress path is not affected because the pore pressure remains constant.
- The authors are invited to make it clear from the beginning how these testing conditions are simulated (through the use of large vs small values of permeability). By the way, finding threshold values of permeability depends on the loading rate. For elementary tests (such as triaxial tests), imposing a constant pore pressure throughout the domain or closing (no-flow) the boundaries are other means to enforce these conditions. These different points and the effectiveness of the use of specific permeability values in simulating drained or undrained conditions should be discussed in the manuscript (this point concerns all cases where drained and undrained conditions are considered). Actually, l.428 partially introduces this effectiveness in implementing undrained conditions. This point should be further elaborated.

Implementation

- l.136+: The text introducing the definition of M_ϕ is misleading. There is no unique value to match the Mohr-Coulomb criterion with a Drucker-Prager surface. This point should be acknowledged.
- l.176-189: These descriptions/definitions should be reworked. For instance: the first term is a volumetric strain, not a porosity (change); the second term (change in total stress) does not appear in that form in eq. (11); the third term does not appear in the same form in eq. (11) and this third term should have the dimension of a stress for consistency with eq. (11) (similarly to the second term); the part should be a part_i in the fifth term; why not using the notation for the elastoplastic (tangent) stiffness matrix instead of the notation for the yield function for the sixth term?; the seventh term might be represented by the plastic correction of the stress state to be more representative of what is usually done in practice.
- Eq. (14): Why not adding a last equation for the heat flux? The boundary condition for the equation for q_i^f is not of Dirichlet type but Neumann's. It is suggested to also add Neumann BCs (and corresponding equations if necessary) for the three subproblems (i.e. T-H-M subproblems).
- Algo 1: This algorithm mixes local (steps 8-9) and global (all the other steps) resolutions. Some clarifications are required.
- l.230: Body forces appear here while they are not considered in eq. (11). This should be harmonised.

Verification

- l.248: The authors are invited to provide more details about the influence of mesh refinement (e.g. mesh convergence study) and the influence of (pseudo-)time steps. Actually, the authors mention a possible influence (l.280-281) but this should be clarified.
- Regarding the error attributed to space and time discretisations (l.280-281), it might be interesting to check the influence of the plastic correction (and its associated tolerance, substepping procedure if any, etc.). Indeed, from Fig. 2c, it appears that the stress point lies slightly outside the failure surface, which might explain the reported error.

- Fig 3: The total stress path should be added.
- Fig 3: Add the case $\psi = 0^o$ to Fig. 3c (or in an additional subfigure).
- Fig 3c: Discuss the slope of the effective stress path (Δq as a function of $\Delta p'$) in relation to the Skempton and Biot coefficients. It is surprising to see a negative slope.
- Fig 4b: Why not showing the $+\Delta t$ case?
- l.419: How far is "far"? From Fig. 6a, it seems that the temperature change is not zero outside the vicinity of the wells (the dominant color is not dark red). By the way, it is weird to have such a homogeneous temperature change (even if it is not so large). Plot the temperature profile along a line in the plane of Fig. 6, including the wellbores but perpendicular to these latter.
- l.448-450: Add a figure showing the regions where shear failure occurs. Actually, Fig. 8 might be intended to show those regions but plotting the values of f and not of Δf would make this evaluation more straightforward. Indeed, the regions where f = 0 are the important ones.
- l.451-452 and fig 7: Add temperature to this plot.
- l.486 (and conclusions, l.591): The authors do not demonstrate that the model is accurate. Actually, the fact that the stress paths are (slightly) crossing the yield surface shows that some (small) error is made. See previous comments on discretisation issues.
- l.562-565, "lateral constraints": Does this mean that the simulations are affected by border effects? Did the authors check the influence of the size of the modelled domain? Border effects would partly invalidate the discussion and conclusions drawn from the current study.

Minor comments

- l.97: Add that the equations concern saturated porous solids.
- l.98: Rework this part dedicated to the definitions of various quantities (generally, the definitions correspond to specific evolutions, which is not always made clear, see the comment below (cf. l.104) for instance).
- l.103: Define precisely what is the "porosity strain".
- l.104: Add that the definition of ζ assumes isothermal evolutions (together with p=0).
- l.148 and eq. (8): Consider noting the plastic multiplier $d\lambda$ instead of λ, for consistency purposes.
- l.154: "poroelastic, thermoelastic" should rather read "poroplastic, thermoplastic", right? The section is for plasticity.
- Eqs. (9) and (10): Double check the indices (use distinct letters for summation indices).
- Fig 1 and l.169: The introduction of a plastic behaviour does not correspond to an additional coupling, since this is part of the mechanic component of the model. The figure might be reworked.
- l.173: Plastic strain might also arise from mechanical loads. This might be mentioned, for the sake of generality.
- l.216-217: It is weird to read here that the equations are non-linear while eqs (11-13) where introduced as linear ones (cf. l.162).
- Algorithm 1, step 6: Δi should read Δx_i
- Table 1: "Solid" should read "Porous solid" in the definition of β_d.
- Figures 2-4: These figures include the isotropic compression. It should be made explicit by adding text (labels) to the figures, for instance. Another possibility would be to reset strains after the isotropic compression and then remove the first stage from the plots. It should also be made clear that the load factor concerns only the final stage (triaxial compression in the case of figure 2, for instance).
- l.295: "decrease" should read "increase", right?

- l.303-307: The occurrence of negative pore pressure might not be an issue if a back pressure was considered (in the "real" experiment).
- Fig. 6: Point (4) in the caption should relate to what can be seen in the figure. This is not the case here.
- l.474-475: Compression is not isotropic. The authors are invited to rewrite this sentence and be more precise here.
- l.494-499: The authors might consider citing Lewis & Schrefler's reference book for the multiphase case.
- l.503: It is a bit surprising to see two distinct values for the geothermal gradient while the case studies do not target a specific site.
- l.510-513: Does this mean that the source term is present at all nodes but deactivated by the Dirac function? If yes, what not handling specifically the nodes concerned with the boundary conditions.
- l.519: "high horizontal"? A word is missing.
- l.521: What is "sufficient time"? Sufficient to what respect?
- l.523: Shouldn't the slope be larger than M_ϕ for the stress path to reach yield?
- l.523, "ensuring yield": The authors are invited to explain why yield has to be "ensured".
- Fig.9: "poroelastic" misspelled.
- Figs. 10 and 11: Labelling the case B with HM instead of THM is misleading since it lets think that the simulation is performed in isothermal conditions. It still corresponds to a THM simulation, but at a location where thermal effects are not significant.
- References: Double check all references (Allis et al. is incomplete, NRC is not an author, format of Ugueto et al is not correct...).

Author Response

We have addressed all comments from the reviewers and our responses are summarized below. The suggestions are very constructive and we thank the reviewers for helping us improve the paper. Our responses are in blue. Line numbers refer to the revised version with tracked changes (and by compiling a pdf copy of the source code), in which we highlighted in blue all changes. Furthermore, the reviewers pointed out discretization/formulation issues causing larger than desired numerical errors. We have taken our time to address this major comment and the revised manuscript reflects new simulations with the corrected formulation (the formulation issue was incorrectly placing a + sign in volumetric strain rate coupling but has now been corrected). All figures, results, and discussion have been updated accordingly.

Response to Reviewer 1

When I tried to run the provided script, I initially got an error in the definition of Macauly brackets which states that module ufl has no attribute 'Max'. I changed 'Max' to 'max_value' then it worked.

The error is related to different versions of Fenicsx. The numerical code has been updated to work with the most current release as well as older versions. The following has been added to the paper:

l.321-l.323: "The numerical solution was verified to work with Fenicsx version 0.6.0. Additional changes may be required for future releases."

In the caption of Figure 3 it is stated that 'Rock dilation at yield reduces pore pressure (and, thus, effective radial stress). I think it should be 'Rock dilation at yield reduces pore pressure (and, thus, effective radial stress increases).

The reviewer is correct. We have made the change as the reviewer suggested. Note that Fig. 3 is now Fig. 4 in the revised version and is between l.585 and l.586.

Could you please elaborate more about the calculated stress path in Figure 3 for undrained triaxial compression and why we have a linear reduction in effective mean pressure during elasticity? Normally we do not expect such behavior under constant volume (undrained conditions) at least for the linear elastic-perfectly plastic type of models.

This stress path resulted from an error in the mass balance formulation: $\alpha \frac{\partial \epsilon_v}{\partial t}$ term originally had a plus sign but it has since been changed to a negative sign. All simulations were re-run with the correct formulation and all figures were updated accordingly.

Fig. 3 (Fig. 4 in revised version) now shows near constant volume behavior during undrained elastic compression (with $K > K_f$).

Was the 3D meshing, used in the full-scale simulations, subjected to a mesh-convergence (H-convergence test) study? How would the refinement affect your conclusions?

We have included a mesh convergence study for the triaxial simulations. Subsection 4.1 has been added to the paper and it begins on l.438.

The full-scale models were not subjected to an h-convergence test but we have (1) refined the mesh size as the reviewer suggested and (2) added the following to the paper to clarify the mesh and intended purpose of these examples:

l.598-l.610: "Both simulations are modeled in three-dimensions with local mesh refinement near the wellbores (mesh size of 10 m for the closed-loop wellbore and mesh size of 4 m for the compartmentalized reservoir). Total degrees of freedom are 4.02E6 for the closed-loop model and 1.39E6 for the compartmentalized reservoir model. The full-scale simulations are intended to (1) provide an application of the numerical code at field scale and (2) compare the numerical results to analytical solutions for further verification. The simulations were not subject to a mesh convergence study as numerical results compare well with analytical solutions and expected results."

How did you model the horizontal wellbores? Appendix B has been added to explain the discretization for the wellbores. This appendix begins in l.982.

In general for Rock failure predictions one is advised to use Hoek–Brown failure criterion or even the more common Mohr-Coulomb failure criterion. Is there any reason for choosing Drucker-Prager yield criterion?

Drucker-Prager is chosen mostly for ease of numerical implementation. The following has been added to the paper:

l.235-l.240: "The Drucker-Prager failure criterion is chosen for ease of numerical implementation as it has a smooth yield surface, avoiding discontinuous derivatives of the yield function. Future versions of the code, however, are planned to include other failure criterion for rocks, e.g., Mohr-Coulomb, Mogi, Lade, etc."

Response to Reviewer 2

The authors need to better present and justify the uniqueness of their approach. The fact that most codes are not adaptable enough or tightly-coupled enough is unfortunately not sufficient to justify this new simulator if there exists even one code that already meets those criteria. Making the cut within a short list of codes is not enough by itself to warrant publication so the need for this specific new code needs to be made clearer. The wording should then be more specific and not state for instance l.63-65 that "many" other codes are limiting or "may not" consider elastoplasticity rigorously. The comparisons need to be more specific and cover some of the codes the authors might have in mind when they avoid stating that "all codes" are missing the features targeted here. This might include the PorousFlow [Wilkins et al., 2020] module of the MOOSE platform [Lindsay et al., 2022].

We agree that a clear justification for a new code is needed as well as a better explanation of the uniqueness. We have made the following corrections/additions to the paper:

1. Added Table 1 (l.120) for direct comparison between our code and others.
2. Added section 2.1: "Modeling Approach" (l.105) to describe the objectives of the numerical model and our modeling approach.
3. Removed "may not consider rock mass elastoplasticity rigorously".

4. Revised the paragraph between l.58-l.80 to more specific wording, as the reviewer suggested.

With standard equations for the momentum, mass and energy balance equations, and with applications retrieving expected results, the novelty mainly comes from the numerical approach, which then deserves a few more explanations and justifications. For instance, the choice of a mixed formulation for the pressure and Darcy velocity should be explained (l.199), along with the way in which pore pressure boundary conditions are imposed weakly (l.210), or why an Euler implicit time integrator is used (l.213), since it is not unconditionally stable for such a non-linear system. Why not an explicit one, or a higher order implicit one?

We also agree with the reviewer that further explanation of the numerical approach is needed. The following has been added to the paper:

1. New paragraph between l.324-l.346 to further explain use of mixed formulation of fluid flow and SUPG stabilization.
2. Explanation of implicit Euler time integrator between l.355-l.360.
3. Discussion on implementation of BCs in the mixed formulation between l.389-l.395.

For the benchmarks, the mesh size and the numbers of degrees of freedom used should be mentioned, with a mesh sensitivity analysis to illustrate the convergence, rather than simply stating (l.280) that "finer load and mesh discretization would decrease this error".

We have made these corrections, as the review recommended:

1. Added a mesh convergence study in section 4.1 beginning on l.438.
2. Removed the original l.280
3. Added l.407-l.410 to clearly state the mesh size and degrees of freedom.

Comparing against analytical solutions is great but errors around 2% (l.279) are way too large to convince the reader of the code performance and the word "accurately", employed in many places, is not really justifies.

This was also pointed out by other reviewers, and has been corrected. The large numerical error came from a mistake in the mass balance formulation: the $\alpha \frac{\partial \epsilon_v}{\partial t}$ originally had a plus sign but it has since been changed to a negative sign. All simulations were re-run with the correct formulation and all figures were updated accordingly.

Numerical errors are now in the range of 10^{-2}–10^{-3}. See updated Figs. 2-5 for the correction.

A monolithic approach with so many variables might be particularly expensive, so this should be mentioned explicitly and discussed as part of the justification for this publication. Can the code reach errors that are orders of magnitudes lower against analytical solutions within reasonable computational time? How does that compare to existing codes (whether they're more adaptable or not)?

We have added a discussion on our modeling approach, as the reviewer recommended. The following additions have been made:

1. Added Fig. 2 (l.485-l.486) to show the numerical error and CPU time.
2. Discussion of advantages/disadvantages of the modeling approach in the paragraph between l.324-l.346.

How is the monolithic tight-coupling improving accuracy and stability over other codes (if that's one of its purposes)? Can the code provide smoother and less unstable results for on figure 7 (and with which computational resources and time)?

We have stated the purpose/objectives of the code in l.106-l.114. Also, regarding the smoother results for the full-scale example simulations, we have (1) re-run the simulations with a finer mesh and (2) added the following to the paper for clarification their intended purpose:

l.598-l.610: “Both simulations are modeled in three-dimensions with local mesh refinement near the wellbores (mesh size of 10 m for the closed-loop wellbore and mesh size of 4 m for the compartmentalized reservoir). Total degrees of freedom are 4.02E6 for the closed-loop model and 1.39E6 for the compartmentalized reservoir model. The full-scale simulations are intended to (1) provide an application of the numerical code at field scale and (2) compare the numerical results to analytical solutions for further verification. The simulations were not subject to a mesh convergence study as numerical results compare well with analytical solutions and expected results.”

All figures have been updated with results from simulation with a finer mesh as stated above.

The governing equations don't account for gravity, yet the gravitational body force appears later.

Thank you for pointing out this mistake. We have updated the governing equations, as the reviewer suggested. See Eq. 12-14 beginning on l.277.

Eq 12, the ratio should be inside the gradient. With the porosity strain taken into account, why isn't the permeability varying as well?

We assume that permeability remains constant due to small changes of initial porosity. We have added the following to the paper to clarify this:

l.282-l.286: “The mass balance equation assumes permeability and pore fluid viscosity are constants and, thus, they are taken out of the divergence operator.”

Eq 14, the boundary conditions don't belong in the functional space definitions. The functions are constrained on the boundaries (e.g. on), not the test functions (e.g. on).

We have clarified in l.346-l.347 that the functional spaces in Eq. 14 (now Eq. 15 in revision) relate to the test space and not the trial space – where functions vanish on the boundaries.

The proper boundary conditions should appear with Eqs. 15-19 and ds and dx are not defined (noting that the symbol s is already used to denote entropy in eq.3...)

Also, we have updated Eq. 15-19 (now Eq. 16-20 in the revision) beginning on l.375 to include all weakly imposed BCs.

Fig. 2a: why is there a kink at 5% axial stress in the linear elasticity part?

The kink resulted from showing the stress/strain during initial compression to prescribed effective mean stress, i.e., before the “experiment” begins. We have updated the Figs. to show deviatoric stress/strain rather than axial stress/strain. The kink is now gone.

The last paragraph of the introduction presents briefly all sections except section 3 that's not mentioned.

Thank you for pointing out this mistake. We updated the last paragraph in the introduction to include section 3. See l.l94.

Mentions of “tight-coupling” might be preferred to “full” coupling (as one can always introduce more couplings, even though I am myself guilty of having employed that term in the past...).

The use of “full-coupling” has been clarified in l.87-l.89 with additional reference to those authors who prefer “tight-coupling”. For example, Lindsay et al. (2022), Wilkins et al. (2021)

The Einstein notation should be mentioned for clarity.

We have updated the mathematical formulation to the more popular index notation with spatial derivatives shown as $\frac{\partial(.)}{\partial x_i}$ and $\frac{\partial(.)}{\partial t}$. The reference to Cheng (2016) in l.152 details the index notation.

Response to Reviewer 3

Eqs. (1-3): The authors acknowledge that these equations neglect heat generated by high strain rate. This is fine. However, the occurrence of plastic strain also generate heat and increase the entropy of the system. Eq. (3) should include that

component associated with plastic dissipation. More details and clarifications are expected.

We have added a discussion on plastic dissipation and explain why we neglect this additional coupling (we are not considering rapid failure of large faults where this coupling is often employed).

This discussion is between l.171-l.187.

Eqs. (1-3): There is some inconsistency here. The authors might consider working (and thus writing these equations) with increments (in the sense of change, but not necessarily small change) for stresses, pore pressure and temperature to deal with non-zero initial conditions. For the thermal problem, it is clear that the coupling terms involving temperature are meant for temperature changes. For stresses and pore pressures, a choice has to be made but in any case, the authors should clarify what is the stress and pressure state corresponding to the non-deformed configuration of the domain (zero strain).

We have changed Eq. 1-3 to work with increments, as the reviewer suggested.

See the updated Eq. 1-3 beginning on l.152.

l.114: The authors might consider explaining this thermal pressurisation using undrained conditions as a limit case. I understand that the authors want to stick to realistic conditions, using high and low permeabilities to reach those drained/undrained limit cases but working out the constitutive equations in those (ideal) cases is interesting for illustration purposes. As an illustration, the authors are invited to demonstrate how the equation (4) is obtained using the constitutive equations and restraints in terms of volume or fluid mass changes.

We agree with the reviewer that a discussion on undrained response to cooling is needed. We have added an additional paragraph between l.201-l.214 to address this comment.

l.282: If the authors refer to the total stress path, this is obvious since that latter is imposed. If the authors refer to the effective stress path, then they should make it clear, adding that the effective stress path is not affected because the pore pressure remains constant.

We have clarified in l.499-l.505 that the stress path is in terms of effective stress and that pore pressure remains constant.

The authors are invited to make it clear from the beginning how these testing conditions are simulated (through the use of large vs small values of permeability). By the way, finding threshold values of permeability depends on the loading rate. For elementary tests (such as triaxial tests), imposing a constant pore pressure throughout the domain or closing (no-flow) the boundaries are other means to enforce these conditions. These different points and the effectiveness of the use of specific permeability values in simulating drained or undrained conditions should be discussed in the manuscript (this point concerns all cases where drained and undrained conditions are considered). Actually, l.428 partially introduces this effectiveness in implementing undrained conditions. This point should be further elaborated.

We have added the following to the paper to clarify the drained/undrained testing conditions:

1. l.427-l.433: discussion for threshold permeability values compared to the axial loading rate.
2. l.403-l.405: statement that undrained conditions are by decreasing permeability under a constant load rate.
3. l.958-l.963: discussion for threshold permeability values compared to the axial loading rate (this is a new section).

l.136+: The text introducing the definition of M_ϕ is misleading. There is no unique value to match the Mohr-Coulomb criterion with a Drucker-Prager surface. This point should be acknowledged.

We agree with the reviews comment that the definition is misleading. We have added the following to the paper to clarify the meaning: l.226-l.228: "... i.e., the Drucker-Prager cone is circumscribed by the Mohr-Coulomb surface)."

l.176-189: These descriptions/definitions should be reworked. For instance: the first term is a volumetric strain, not a porosity (change); the second term (change in total stress) does not appear in that form in eq. (11); the third term does not appear in the same form in eq. (11) and this third term should have the dimension of a stress for consistency with eq. (11) (similarly to the second term); the part should be a part_i in the fifth term; why not using the notation for the

elastoplastic (tangent) stiffness matrix instead of the notation for the yield function for the sixth term?; the seventh term might be represented by the plastic correction of the stress state to be more representative of what is usually done in practice.

We have reworked these descriptions and the associated figure, as the reviewer recommended. The changes are made between l.296-l.312:

1. Fig. 1 combines the mechanical field into one box.
2. The mathematical terms are consistent with Eq. 12-14
3. Definition of volumetric strain has been reworked.
4. Changed yield function to elastoplastic tangent stiffness matrix.
5. Changed plastic strain to the plastic stress correction.

However, regarding the dimension of stress, we have chosen to keep the description terms consistent with that in Eq. 12-14 rather than keep the units the same.

Eq. (14): Why not adding a last equation for the heat flux? The boundary condition for the equation for q_i^f is not of Dirichlet type but Neumann's. It is suggested to also add Neumann BCs (and corresponding equations if necessary) for the three subproblems (i.e. T-H-M subproblems).

We have added the Neumann BCs including (1) body fore, (2) fluid source/sink term, and (3) thermal source/sink term in Eq. 12-14. See l.277-l.278 for this change.

Algorithm 1: This algorithm mixes local (steps 8-9) and global (all the other steps) resolutions. Some clarifications are required.

We have clarified in steps 8-9 that the plastic consistency check is done on a global level (volume integration) and have updated Algorithm 1 accordingly. See l.371-l.372 for the change.

l.230: Body forces appear here while they are not considered in eq. (11). This should be harmonised.

We have added the Neumann BCs including (1) body fore, (2) fluid source/sink term, and (3) thermal source/sink term in Eq. 12-14. See l.277-l.278 for this change. This is now harmonized with the variation form Eq. 16-20.

l.248: The authors are invited to provide more details about the influence of mesh refinement (e.g. mesh convergence study) and the influence of (pseudo-)time steps. Actually, the authors mention a possible influence (l.280-281) but this should be clarified.

Regarding the error attributed to space and time discretisations (l.280-281), it might be interesting to check the influence of the plastic correction (and its associated tolerance, substepping procedure if any, etc.). Indeed, from Fig. 2c, it appears that the stress point lies slightly outside the failure surface, which might explain the reported error.

This was also pointed out by other reviewers, and has been corrected. We have provided a mesh convergence study in section 4.1 beginning on l.438. A brief explanation of the original error:

The large numerical error came from a mistake in the mass balance formulation: the $\alpha \frac{\partial \epsilon_v}{\partial t}$ term originally had a plus sign but it has since been changed to a negative sign. All simulations were re-run with the correct formulation and all figures were updated accordingly.

Numerical errors are now in the range of 10^{-2}–10^{-3}. See updated Figs. 2-5 for the correction.

Fig 3: The total stress path should be added.

Regarding the total stress path in Fig. 3 (now Fig. 4 in the revision): we have compared our simulations results to analytical solutions for poroelastic undrained response and results are available in Fig. 2. See also Eq. 22 for the analytical solution.

Fig 3: Add the case $\psi = 0^o$ to Fig. 3c (or in an additional subfigure).

We have updated Fig 3. (now Fig. 4 in the revision) to clearly show both case of $\psi = 7.5^o$ and $\psi = 0^o$, as the reviewer recommended.

Fig 3c: Discuss the slope of the effective stress path (Δq as a function of $\Delta p'$) in relation to the Skempton and Biot coefficients. It is surprising to see a negative slope.

We have also added a discussion for Skempton's coefficients influence on the stress path between l.538-l.546. Note that the negative slope resulted from an error in the code (as previously mentioned). This has since been fixed and simulations were re-run.

l.419: How far is "far"? From Fig. 6a, it seems that the temperature change is not zero outside the vicinity of the wells (the dominant color is not dark red). By the way, it is weird to have such a homogeneous temperature change (even if it is not so large). Plot the temperature profile along a line in the plane of Fig. 6, including the wellbores but perpendicular to these latter.

We have corrected the color bar of Fig. 6a (now Fig. 7a in the revision) to show the predominant color as dark red (the scale was originally incorrect and we thank the reviewer for pointing this out).

Also, regarding the temperature change along (and across) the length of the wellbores, this is not easily visible because the scale of the color bar is significantly larger than the temperature change along the well length. We have updated the Fig. 7a to show the temperature difference with text as this is rather difficult to show by manipulating the color bar scale.

l.448-450: Add a figure showing the regions where shear failure occurs. Actually, Fig. 8 might be intended to show those regions but plotting the values of f and not of Δf would make this evaluation more straightforward. Indeed, the regions where f => 0 are the important ones.

We have updated Fig. 8 (now Fig. 9 in the revision) to show regions where f = 0, as the reviewer recommended.

l.451-452 and fig 7: Add temperature to this plot.

Temperature has been added to Fig. 7 (Fig. 8 in revised version).

l.486 (and conclusions, l.591): The authors do not demonstrate that the model is accurate. Actually, the fact that the stress paths are (slightly) crossing the yield surface shows that some (small) error is made. See previous comments on discretisation issues.

As previously mentioned, we have corrected the discretization issues for all simulations and the new figures reflect this.

l.562-565, "lateral constraints": Does this mean that the simulations are affected by border effects? Did the authors check the influence of the size of the modelled domain? Border effects would partly invalidate the discussion and conclusions drawn from the current study.

Here, we are referring to the fact that lowering the temperature of the subsurface primarily alters the horizontal stresses as a result of adjacent rock not allowing free expansion/contraction in horizontal directions. We are not referring to boundary effects. The following has been added to the paper to clarify this:

l.828: "... (a result of adjacent rock causing a virtual lateral constraint)."

l.97: Add that the equations concern saturated porous solids.

We have made this correction. See l.151 for this change.

l.98: Rework this part dedicated to the definitions of various quantities (generally, the definitions correspond to specific evolutions, which is not always made clear, see the comment below (cf. l.104) for instance).

We have reworked these definitions and provided examples of specific evolution cases. The changes occur between l.158-l.166.

l.103: Define precisely what is the "porosity strain".

We have made this correction in l.165-l.166.

l.104: Add that the definition of ζ assumes isothermal evolutions (together with p=0).

We have made this correction in l.161.

l.148 and eq. (8): Consider noting the plastic multiplier $d\lambda$ instead of λ, for consistency purposes.

We have (1) changed λ to $d\lambda$ everywhere in the paper and (2) changed “elastic” to “plastic” in l.250.

l.154: "poroelastic, thermoelastic" should rather read "poroplastic, thermoplastic", right? The section is for plasticity.

;

Eqs. (9) and (10): Double check the indices (use distinct letters for summation indices).

This was also a comment/suggestion from other reviewers. We have updated the mathematical formulation to the more popular (and more easily readable) index notation everywhere in the paper. The summation indices have been checked for consistency.

Fig 1 and l.169: The introduction of a plastic behaviour does not correspond to an additional coupling, since this is part of the mechanic component of the model. The figure might be reworked.

We have revised and updated Fig. 1 by combining the mechanical field into one box. See l.286-l.287 for this change.

l.173: Plastic strain might also arise from mechanical loads. This might be mentioned, for the sake of generality.

This has been updated to reflect thermal, hydraulic, and mechanical loads may cause shear failure. See l.293 for the change.

l.216-217: It is weird to read here that the equations are non-linear while eqs (11-13) where introduced as linear ones (cf. l.162).

This was a mistake in the original draft. The word “linear” has been removed from the revised version. This is reflected in l.277-l.278.

Algorithm 1, step 6: Δi should read Δx_i

This has been corrected.

Table 1: "Solid" should read "Porous solid" in the definition of β_d.

This has been corrected.

Figures 2-4: These figures include the isotropic compression. It should be made explicit by adding text (labels) to the figures, for instance. Another possibility would be to reset strains after the isotropic compression and then remove the first stage from the plots. It should also be made clear that the load factor concerns only the final stage (triaxial compression in the case of figure 2, for instance).

We have decided to show only the deviatoric stress path in all figures to “hide” the isotropic compression initialization stage. This is reflected in Figs. 3-5. Furthermore, we have changed the load factor to clearly state the loading rate and time (more precise to improve clarity). Figs. 3-5 now show also time as opposed to a load factor.

l.295: "decrease" should read "increase", right?

Correct, we have updated this in the revision.

l.303-307: The occurrence of negative pore pressure might not be an issue if a back pressure was considered (in the "real" experiment).

We have changed this sentence to read: l.521-l.528: “Note that the pore pressure eventually becomes negative at high enough deviatoric strain, a non-physical event that could be prevented in the real experiment by water cavitation or initial pore pressure larger than the change caused by plastic dilation, but displays the ability of the numerical code to simulate such coupled poroelastic behavior.”

Fig. 6: Point (4) in the caption should relate to what can be seen in the figure. This is not the case here.

We have removed point (4) in the Fig. 6 (now Fig. 7 in the revision) caption.

l.474-475: Compression is not isotropic. The authors are invited to rewrite this sentence and be more precise here.

We have changed this sentence to:

l.722-l.728: “Locations adjacent to the wellbores (point A on Figure 9a) yield within 10 years of heat drainage while locations between wellbores (point B on Figure 9a) stay in the elastic regime for more than 30-years because the initial state of stress favors shear failure by nearly isotropic unloading.”

l.494-499: The authors might consider citing Lewis & Schrefler’s reference book for the multiphase case.

We have added the citation in l.753.

l.503: It is a bit surprising to see two distinct values for the geothermal gradient while the case studies do not target a specific site.

We have opted to re-run the simulations with the same geothermal gradient of 30^o/km. This is now consistent between the two case studies.

l.510-513: Does this mean that the source term is present at all nodes but deactivated by the Dirac function? If yes, what not handling specifically the nodes concerned with the boundary conditions.

Yes, this is correct about the Dirac Delta function – it is only active at a few nodes but present everywhere. We have added the following in section 3 to further explain BCs in the mixed form of fluid flow:

l.389-l.395: “In the mixed form for fluid flow, Dirichlet pressure boundary conditions are weakly imposed as natural conditions in Eq. 19 while Neumann boundary conditions (flux) are essential. This is opposite to the pressure only formulation where Dirichlet boundary conditions are essential and Neumann boundary conditions are natural [Pan and Rui, 2012].”

Also note that this is the preferred way of handling such BCs in Fenicsx.

l.519: "high horizontal"? A word is missing.

We have corrected this to “initially large horizontal effective stress without tectonic stresses” in l.780-l.781.

l.521: What is "sufficient time"? Sufficient to what respect?

We have corrected this to “within the 6-month injection period” in l783-l784.

l.523: Shouldn’t the slope be larger than M_ϕ for the stress path to reach yield?

The slope should be less than since this is unloading by increasing pore pressure and decreasing effective stress, in other words isotopic unloading favors shear failure.

l.523, "ensuring yield": The authors are invited to explain why yield has to be "ensured".

We have changed “ensuring” to “causing” and provided explanation that it is based off analytical solutions for depletion stress path. See the revised sentence beginning in l.779.

Fig.9: "poroelastic" misspelled.

This has been corrected.

Figs. 10 and 11: Labelling the case B with HM instead of THM is misleading since it lets think that the simulation is performed in isothermal conditions. It still corresponds to a THM simulation, but at a location where thermal effects are not significant.

We have updated Figs. 10 and 11 (Figs. 11 and 12 in the revision) without the HM and THM labels, as the reviewer suggested. We, instead, show temperature change to get the message across.

References: Double check all references (Allis et al. is incomplete, NRC is not an author, format of Ugueto et al is not cor-

rect...).

We have double checked all references and corrected many that were not formatted correctly.

Review Round 2

Reviewer 1 (Anonymous)

The authors have sufficiently addressed my comments one by one and I am satisfied with their responses. I therefore recommend that the manuscript be published in Open Geomechanics in its current form.

Reviewer 2

The second reviewer had no opportunity to perform the review, hence the editor reviewed the revised manuscript for the comments.

Reviewer 3 (Anonymous)

The authors have thoroughly revised their manuscript, clarifying or correcting the issues raised in my previous report. I'm happy to recommend the publication of this excellent work that will certainly benefit the community.

Two typos spotted during this round of review:

- Tab. 1, caption: "thermo-poroelastic" to "thermo-poroelasticity"
- l.354+: "solves" what? Words are missing there

Editor: Corrected during the copy editing process.