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EST-D-24-03216R2
"Effects of Fault, Fluid and Operational Properties on Induced Seismicity along a Reservoir Fault During Hydrogen Storage in a Depleted Gas Reservoir"
Revision 2

Renato Poli (Reviewer 3)

Reviewer Recommendation Term:	Reject
Overall Reviewer Manuscript Rating:	50

Transfer Authorization	**Response**
My reviewer report is complete.	Yes
I am ready to transfer my reviewer report to the Editors.	Yes

Custom Review Question(s):	**Response**
I confirm that there is no conflict of interest with any of the co-authors in this paper.	Yes

Comments to Editor:

Question 1: The subject addressed in this article is worthy of investigation
Subject addressed in this article is worthy of investigation
1) Strongly disagree
2) Disagree
3) Neutral
4) Agree
5) Strongly agree
Ans: 5) Strongly agree

Question 2: The information presented is new
1) Strongly disagree
2) Disagree
3) Neutral
4) Agree
5) Strongly agree
Ans: 2) Disagree

Question 3: The conclusions are supported by the data
1) Strongly disagree
2) Disagree
3) Neutral
4) Agree
5) Strongly agree
Ans: 1) Strongly disagree

Question 4: The manuscript is appropriate for the journal
1) Strongly disagree
2) Disagree
3) Neutral
4) Agree
5) Strongly agree
Ans: 2) Disagree

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Question 5: Organization of the manuscript is appropriate
1) Strongly disagree
2) Disagree

3) Neutral
4) Agree
5) Strongly agree
Ans: 4) Agree

Question 6: Figures, tables and supplementary data are appropriate
1) Strongly disagree
2) Disagree
3) Neutral
4) Agree
5) Strongly agree
Ans: 3) Neutral

Comments to Author:

EFFECTS OF FAULT, FLUID AND OPERATIONAL PROPERTIES ON INDUCED SEISMICITY ALONG RESERVOIR FAULT DURING DEPLETED GAS RESERVOIR HYDROGEN STORAGE

The paper uses a numerical framework to investigate the impact of operational and physical parameters on fault slip and seismicity. The study's object is a depleted gas reservoir under hydrogen storage. The overall language of the paper is adequate. It could benefit from a more concise text, especially in the Introduction. The results section show a large set of figures and descriptive text. The authors could save space and verbosity with tornado plots or summary tables, individualizing the effect of each parameter.

The Introduction describes motivations and reviews the state of the art, followed by a deeper literature review of each parameter under consideration. Next, the authors present the mathematical background for the simulations and a description of the base case parameters. The results section discusses one parameter at a time, followed by discussion and a brief summary as conclusions.

The numerical model rely on strong simplifications and assumptions, and the numerical framework's accuracy is unclear. It is important to include remarks on missing physics like fracture tip behavior, plasticity envelopes, thermal stresses, the presence of natural joints, etc. The paper could benefit from some numerical validation work to reproduce expected behavior in some control cases. The parameterization of the reservoir rock is unrealistic, especially regarding the Biot coefficient and initial stresses. The results do not show the pressure, displacement, and stress fields, which might not be physical for the high injection rates investigated.

I listed a few remarks below.

ABSTRACT
- The abstract extensively uses the passive voice, which differs from the text's body.

1. INTRODUCTION
- Parts of the Introduction may fit better in the literature survey. The text is long and lacks focus.
- The last paragraph, starting on page 1 ("Salt caverns have ..."), could be split for clarity.
- Substantial advocacy in the Introduction favors hydrogen storage in depleted reservoirs, excessively focusing on the drawbacks of salt caverns and aquifers. However, the technologies are not mutually exclusive, and I suggest focusing on the niche and applicability of each, balancing the tradeoffs.
- Be more selective on references and the relevant contribution of each author. For example, page 2 cites Muhammed et al. (2022) at least seven times.
- Add more context to the last sentence in the third paragraph of page 2 ("Therefore, numerical simulation of geological ..."). It seems from the text that this is the only tool of analysis, which is unusual. Before any numerical simulation, a number of hypotheses and analytical work normally outline the risks and opportunities to be refined later using numerical models.
- The last paragraph of page 2 brings information that is irrelevant to the context. (Ex: "Geochemical research must examine ..."). Remove the text and keeping focus on the object in the study
- The first paragraph of page 3 should be rewritten for clarity. The second criterion is unclear ("Secondly, the injection can lead to stress changes well beyond..."). The idea of fault slip under depletion may be more related to differential depletion than compaction (each side of the fracture has a different depletion profile).
- Although fault slip triggers seismicity, this is not the only cause. The authors could discuss tensile or shear failure of the rock matrix. Be clear that only one issue is under investigation.
- Page 3, second paragraph: "However, relatively little is known about the factors ...". I am not convinced this is true, as faults and fractures have been extensively investigated.
- The higher the Biot coefficient, the less likely of a fault to slip, this is consensual. The base case (T[illegible] 1) uses a low Biot coefficient (\alpha=0.35), which is unlikely to be the case of a sandstone with porosity, \phi=15%. Such parameters induce the models to experience more seismicity.
- Although seismicity may (and will) occur, the paper's goal is to avoid it completely, which is impossible considering the ranges of pressure and temperature needed. One idea is to mention the criticality and criteria for designers assessing such an issue.

- When mentioning the numerical simulator ICGT, it is important to cite references and numerical validation procedures covering the current use of the simulator. (last paragraph of the Introduction)

2. LITERATURE REVIEW
- The section cites work on similar environments. It acknowledges the limitations of 2D assessment, which is correct. The reader could benefit from more critical points of view. Numbers from the work of (Deng, 2021), for example, are listed without enough context. Providing a qualitative view of their findings and translating to the current context is preferable.
- Friction coefficient: It is important to mention the physics that are missing in the models. Rocks can be approximated as linear poroelastic in a very narrow domain. Plasticity, compaction, and loss of integrity are normally strain release mechanisms concurrent with fault slip. The higher the fault friction coefficient, the more likely these other mechanisms will be triggered.
- Fault permeability: It would be interesting to investigate the permeability restrictions normal to fault plane, which will cause a pressure differential on both sides that might trigger the slip.
- Withdrawal rate, first paragraph: "The change in the effective horizontal...". The equations lack definitions of the symbols (\Delta P, for example) and context. For example, they are not valid after a fault slip event that will release energy nor for initialization purposes.
- Withdrawal rate, last paragraph: "the effects of production rate on fault slip..." The authors do not provide enough evidence for that statement, as each cited paper studies a different context.
- Fluid Viscosity: It is not clear if the model in use covers the physics of the "EHD fault lubrication regime".

3. METHODOLOGY
- Cite ICGT numerical validation work covering the current runs.
- The term "Darcy-type" flow is inadequate for the flow between parallel plates. Normally, the preferred terms are "cubic law", "Poiseuille law" or "Lubrication". Note that such laws are highly imprecise for natural faults and fractures. See: Viswanathan et al. (2022): From fluid flow to coupled processes in fractured rock (...)
- Equation (1) and (2): the initial states of stress and strain \sigma_0 and \epsilon_0 may be omitted. Otherwise, to keep consistency, they must be present in equation (2). The authors may consider to comment on that in the text.
- I could not understand the integrals in equation (4) and (6), as they normally emerge with a test function in the FEM context. Please revise or justify their derivation.
- The last paragraph of the section and the legend of Table 1 present the mathematical symbols to the reader. Consider using a table of symbols or introduce the symbols in the text as they appear.
- In equation (7), I could not understand the use of a position vector "x". Displacement "u" is standard for small strain poroelasticity.
- It is important to describe the fault model and its limitations. A reference in section 3.3 is needed to provide a minimum background to equation (8).
- Reorganize last few paragraph of section 3.3, equations (10) and (11).
- It seems that the double loop augmented lagrangian algorithm solves the contact nonlinearities. Include a brief discussion of the algorithm, show its limitations, and validation procedures. Comment on the quality assessment of the numerical approximation (overall equilibrium, mass balance, physical consistency).
- Provide references for section 3.4.

4. MODEL DESCRIPTION
- Figure 1 is too small (unreadable).
- Define the Pressure and Temperature for the fluids' injection rates and properties of Table 1 and 2.
- The authors should be more explicit about the boundary conditions and stress initialization.
- Isoparametric linear elements in FEM are limited for this kind of assessment, demanding too much refinement near the fracture and increasing the stiffness artificially. Consider using quadratic elements in future work.
- Table 1 must be mentioned in the text earlier than Table 2 and before it appears.
- Table 1: Biot coefficient is unrealistic (a sandstone with porosity around 15% would have \alpha>0.6). Vertical stress is unclear (0.9800517 MPa - is this correct?). Mention the vertical stress at the reservoir depth.
- Table 1 shows that the horizontal stresses are equal at initialization. This is a quite unique environment. As the Earth's crust is critically stressed, the reservoir and its surroundings will likely have critically stressed faults (in the imminence of slippage)—see Zoback's "Reservoir Geomechanics" book, chapter 11—Critically stressed faults and fluid flow. Critically stressed faults tend to me more sensitive to the present investigation.
- Table 2: The parameters are not independent from each other. This is a limitation worth mentioning.

5. RESULTS
- The sections are excessively descriptive and verbose. I suggest considering a couple of summary tables and illustrations to overview the most (and least) important factors - a tornado diagram, for example.
- The idea of "contact traction" is unclear. How relevant is the change in the direction of contact traction? Many figures show such change, but it is not clear what they communicate or how they help understand the phenomena.

- Some parameters assessed could be introduced after physical reasoning. (1) Why would the well perforation length be important? In which cases this might be critical? What scenario do the authors have in mind? A diagram could be useful for each case. (2) How can fluid density and buoyancy be necessary for the current scenario? What effective stress increment shows up at the bottom of the fracture due to buoyancy (order of magnitude)?
- The pressure variation and stress distribution are not clear. For the high injection rates used, the pressure, stresses, and displacements may become physically inconsistent.
- Does temperature impact fault slip? A section with limitations in this direction may help understand the domain and hypotheses of the study
- Some figures (e.g., Fig 11) show higher apertures near the corners and no aperture in the middle. Why? Moreover, the noise seen in the aperture plots suggests numerical issues.
- The text in Fig. 10 is too small, making it difficult to understand and interpret the plots.

6. DISCUSSION
- Emphasize the study's limitations, especially the lack of plasticity, compaction, and other strain release mechanisms. The investigated fault was not critically stressed.
- Discussion, paragraph 2: From the authors' argument, the rate of 20m3/s is extremely high. One should not expect one well to cope with such rate even in worst case scenarios. Other problems are likely to occur, even in this simplistic model (ex: well Bottom hole pressure and cap rock integrity)

7. CONCLUSIONS
- The conclusions cannot be generalized, as the case study is over-simplistic.
- Emphasize along the text the extent to which seismic activity is tolerable.

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