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Solid Earth
Discussions



# *Interactive comment on* "Induced seismicity in geologic carbon storage" *by* Víctor Vilarrasa et al.

**Anonymous Referee #2**

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Lacking in new data and contributions, and filled with citations and unsupported generic comments The authors attempt to mitigate undesirable induced seismicity by investigating different mechanisms leading to fracture/fault instability and performing numerical simulations. The authors mention that the main factors causing stress changes in the reservoir are injection-related pressure buildup, in-situ stress state, injected fluid's temperature gradient. The outline of the paper is communicated at the end of Section 1 in page 4. However, there is no clear section on what unique contributions this study is making to improve the state-of-the-art. A general theme of the manuscript is that too many generic, qualitative comments are made without new data or analysis to support those comments. There is an unreasonably large emphasis on citing and reviewing existing papers instead of showing new results. When the simulation results are shown, there are no clear quantitative details of the simulation model: model dimensions, meshing, initial and boundary conditions, well conditions, and hy-

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Discussion paper

draulic/mechanical properties. This suggests that the manuscript should be submitted as a review article, not Research Article. 1. Figure 1,2,3: They are extremely generic, redundant and partially inaccurate. For example, Figure 2 shows that the effect of temperature change is to only shift the Mohr Circle to left, which is highly imprecise and can be inaccurate depending on the rock type, injection layer geometry (total stress can change), and the magnitude and direction of temperature change. Figure 3 lumps all sedimentary rocks in the world as critically unstressed and assumes that they all fail under linear Mohr Coulomb condition. This is almost unscientific and completely unnecessary. 2. Figure 4: This shows results for a problem that is not even defined. What is the physical model setup, what are the initial and boundary conditions of the coupled flow-mechanics problem, what is the well rate and injection duration? Why do we accept this result as correct? 3. Figure 5: Same as before. Why is this an accepted solution? What is the problem setup? 4. Page 9: “progressively increasing the flow rate at the beginning of injection may avoid the initial peak in pressure buildup” This statement needs to be quantified: how much increase to avoid how much pressure buildup. Otherwise, the idea of “progressively increasing the rate” is a conjecture. 5. Page 1-15: There is too much literature review. Almost 906. Abstract: “We aim at understanding . . . and to develop methodologies . . . through dimensional and numerical analysis.” There is now dimensional analysis. In fact, the word “dimensional” appears only once in the abstract. Please remove it from the abstract. 7. Page 14-15: This combines citations with discussion of authors’ results. This is very confusing. It is better to move authors’ own work into a separate section and not mix with background literature survey. 8. Page 15 line 5: “As a result, the induced horizontal stresses in the in-plane direction are high where the storage formation is present on both sides of the fault, but it is low where the base rock is on the other side of the fault.” This is not a result in this manuscript. Either remove it or support it with actual simulation results. 9. Figure 7 and 8: Data used for the simulation must be provided otherwise it is not clear what to expect in the result. What is the contrast in elastic stiffness and hydraulic properties between the damage zone vs. reservoir vs. caprock. All modeling assumptions used during the simulation must be listed. 10. Page 17-18: This proposes a field test to macroscopically characterize hydraulic, thermal and geomechanical properties without mentioning any challenges related to applicability and operation. Otherwise such a field test will get classified as unrealistic and not useful for CO2 injection. 11. Page 21: “predictive models of induced seismicity that consider coupled THMS processes should be applied” This is much easier said than done. What are these models? The results in this manuscript do not show any coupling to seismicity, which requires solution of the elastodynamic problem in a n-dimensional domain with a (n-1) dimensional fault surface, not a n-dimensional fault zone. This manuscript presents neither an approach nor results from coupling of the four processes T, H, M, S. 12. Page 21: “The continuous characterization will permit updating the fault stability analysis by incorporating newly detected faults.” How will the new faults be detected? This is not trivial and not answered in this manuscript. So, please remove this. 13. Figure 6: Color scale can be improved. For example, it is different for the upper and lower figures, yet the maximum value is not visible in the upper figure.

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