Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-129-AC2, 2019

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

Víctor Vilarrasa et al.

victor.vilarrasa@idaea.csic.es

Received and published: 29 March 2019

RESPONSE TO THE INTERACTIVE COMMENT OF REFEREE #2

We discuss below the comments made by the reviewers and our responses. To facilitate reading, we indicate the referee's comments with C and our responses with Reply.

General Comments

C: 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

Printer-friendly version

Discussion paper

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 hydraulic/mechanical properties. This suggests that the manuscript should be submitted as a review article, not Research Article.

Reply: As we already explained in the response to the interactive comment of referee #1, this is a review article, because as awardee of the Outstanding Early Career Scientists Award for the Division on Energy, Resources and the Environment (ERE) of the EGU, I was invited to publish a paper in one of the EGU journal based on my lecture. Since I presented in my lecture the work that I have done in the last years and that contributed to receive the award, the article type should be changed from research article to review article. We apologize for this mistake when we submitted the manuscript.

Specific Comments

C: 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.

Reply: These three Figures are schematic to explain general aspects of induced seismicity. Regarding the shift of the Mohr circle due to temperature change, it is shifted to the left because cooling is expected to occur around CO2 injection wells, and thus, a total stress reduction will occur. We will add a minus in front of the delta T to indicate that cooling takes place. Additionally, the size of the Mohr circle changes because the changes in the total stresses may be different in the vertical and horizontal directions. Nonetheless, it may be difficult to observe that the two circles (the red and the blue ones) have different sizes, so we will modify the Figure to exaggerate this effect. As for Figure 3, we agree with the referee that not all sedimentary rocks are not critically stressed, as we already state in the figure caption and main text. We also agree with the referee that the failure envelope is not linear for rock. Indeed, we usually use non-linear shear strength in our studies. Since the Figure was schematic, we were just representing a linear failure surface, but we will modify it to show the non-linearity of shear strength. Additionally, we will modify this Figure to indicate that crystalline rock is more likely to be critically stressed than sedimentary rocks because of their higher stiffness, which makes them accumulate more stress. Additionally, to support this statement, we will add a Table showing the stress state at several CO_2 storage sites together with the mobilized friction coefficient. The mobilized friction coefficient ranges from 0.35 to 0.54, so in all cases is lower than 0.6, meaning that favourably oriented faults to undergo shear slip are not critically stressed. Of course, knowing the stress state at each site is crucial because the maximum sustainable injection pressure to avoid reactivating faults depends on the initial stress of state. Thus, the injection pressure at the site with a mobilized friction coefficient of 0.54 has to be lower than at the site with a mobilized friction coefficient of 0.35.

C: 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?

Reply: This Figure describes the pressure evolution in a 100-m thick reservoir in which 1 Mt of CO_2/yr are injected in an aquifer with permeability of 1e-13 m^2 and radius of 100 km. Since the pressure front does not reach the outer boundary during the injection

Printer-friendly version

Discussion paper

period shown in the figure, the nature of the boundary does not have any effect on the pressure evolution. The aquifer, which is placed at 1.5 km depth, initially presents hydrostatic pressure. Nevertheless, since we show the pressure changes, the absolute initial pressure is not relevant. We will provide these details on the characteristics of this particular model in the manuscript. Regardless of the particularities of this model, the intention is to describe in a general way CO2 injection pressure evolution, which is significantly different from that of water injection. As explained in the text, the characteristics of this pressure evolution, i.e., the initial sharp increase in CO2 pressure followed by a relatively constant injection pressure, have been observed in the field, in analytical and numerical solutions. Based on this evidence, it can be accepted as correct.

C: Figure 5: Same as before. Why is this an accepted solution? What is the problem setup?

Reply: The results shown in this Figure are from a fully coupled numerical code that solves non-isothermal two-phase flow in deformable porous media (CODE_BRIGHT), which has been benchmarked extensively and is well accepted within the scientific community. As for the problem setup, we will add more details. Nevertheless, the Figure was intended to support the explanations of the processes that occur during cold CO2 injection, without focusing on a specific case.

C: 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.

Reply: We will delete this sentence.

C: Page 1-15: There is too much literature review. Almost 90.

Reply: We deem this amount of references appropriate for a review article.

C: 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.

Reply: We will remove the word dimensional in the abstract.

C: 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.

Reply: In this section, we are providing explanations of the relevant aspects that control fault stability. Even though we have studied this problem extensively, other authors have made relevant contributions to the topic and we believe that it is important to include their contributions in this section as well.

C: 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.

Reply: This statement results from the observation of the changes in the horizontal stress in the in-plane direction shown in Figure 7. To clarify this point, we will add a reference to this Figure at the end of the sentence.

C: 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.

Reply: We will provide this information in the revised version of the manuscript.

C: Page 17-18: This proposes a field test to macroscopically characterize hydraulic, thermal and geomechanical properties without mentioning any challenges related to

Printer-friendly version

Discussion paper

applicability and operation. Otherwise such a field test will get classified as unrealistic and not useful for CO_2 injection.

Reply: We thank the referee for raising this point, which is certainly of interest and deserves discussion. There are a number of challenges related to this characterization test. To begin with, the drilling of a network of monitoring wells is not a common practice yet. Monitoring techniques also present challenges. Pressure is usually measured at the well-head, but calculating the bottom-hole pressure from the well-head pressure is not straightforward given the non-linearities of the injected fluid, especially for CO_2 injection. Unfortunately, pressure measurements in well different than the injection well are almost inexistent. Temperature measurements receive even less attention. As for deformation measurements, ground surface can be measured with InSAR data, but for characterization tests that last a few days, the deformation of the ground may not be detectable given the great depths of storage formations. Thus, deformation should be measured at depth within the boreholes. These measurements pose the question of whether the measured deformation refers to that of the rock or to that of the well. Since the casing of wells is stiffer than rock, the rock may deform more than the well and sliding could even occur between the rock and the cement surrounding the well casing. Fiber optic may solve part of these monitoring challenges, but the way how this monitoring should be performed is still not crystal clear for the moment. As far as microseismicity monitoring is concerned, arrays of geophones are certainly needed to be placed at depth. Otherwise, the signal-to-noise ratio is too high, which complicates detecting microseismic events. Additionally, multi-sensor arrays with a wide aperture coverage are necessary to accurately locate the events. We will include in the manuscript this discussion on the challenges of performing such characterization test.

C: 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.

Reply: This is a recommendation we made for future practices based on our previous experience. Given that we do not go into the details of the seismic part, we will replace THMS by THM, which is discussed in the manuscript.

C: 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.

Reply: The continuous characterization refers to the methodology explained in Figure 10. Thus, by applying this methodology, it is possible to detect previously unidentified low-permeable faults and incorporate them in the model of the injection site. We will mention Figure 10 at the end of this sentence to clarify how new faults can be detected.

C: 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.

Reply: We will improve the color scales of Figure 6 so that the maximum and minimum values are visible.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2018-129, 2019.