Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-129-AC1, 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: 12 February 2019

RESPONSE TO THE INTERACTIVE COMMENT OF REFEREE #1

We discuss below the comments made by referee#1 and our responses. To facilitate reading, we indicate the referee's comments with C and our responses with Reply.

General Comments

C: The authors present and review an overview of the issues surrounding induced seismicity in geologic carbon storage. Specifically the authors attempt to show the impacts of 1) stress state, 2) pressure evolution, 3) thermal effects, and 4) fault stability on the potential for induced seismicity. They then assess the characterisation required to analyse the above and propose a number of ways to minimise the risk of induced seismicity.

Reply: We would like to begin by thanking the referee for looking in detail to this manuscript, as shown by this concise summary.

C: Whilst each of the above are treated suitably I struggle to see the major advances in this paper (above that of the cited papers) as required for a research article. It almost has the feel of a review/commentary paper. This may be enhanced by the lack of clarity on what original research is presented here as opposed to previously published citations (of which more than 130 also makes this feel more like a review). I give examples below. Specifically, there is no introduction or methods that describe what numerical modelling is actually performed. If there are new results here, they need to be shown more clearly.

Reply: We understand the referee's concern and would like to explain the peculiarity of this manuscript. As awardee of the Division 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 journals based on my lecture. In the lecture, I presented the work that I have done in the last years and that contributed to receive the award. This is why the paper is a review and compilation of recent work.

Specific Comments

C: Page 6. Triggering mechanisms. Many alternative mechanisms (other than pore pressure increase) are presented for seismicity triggers but the paper then only goes on to explore a few of these explicitly. For example, heterogeneity and geochemical effects are not discussed further. Thermal effects are considered but no detailed assessment of rock properties and contrast of layers. No further discussion of stress redistribution or aseismic slip. Thus I am left feeling the conclusion that seismicity can be predicted, monitored and managed is undermined by not tackling these in detail.

Reply: The objective of providing a detailed list of triggering mechanisms other than pore pressure build-up was to clearly show that the widespread idea that induced seismicity is exclusively caused by pore pressure increase is not accurate and that other processes should be considered. In order to tackle all these mechanisms, we will add a new Section entitle “Other effects” (placed after the Section on “Non-isothermal effects”), so that we give more details and discuss all the mechanisms included in the list. As for the completeness of non-isothermal effects, we agree that the effect of the contrast of rock properties between layers is relevant. For example, if the rock layers have different thermal expansion coefficient, the shear stress increases in the contact between the two layers, which may result in damage to the lower portion of the caprock around injection wells. We will add a detailed explanation of this aspect in the manuscript.

C: Page 8. Stress state It is a very large assumption to say sedimentary rocks are not critically stressed. There are clearly many exampled (even cited in this paper, e.g. Blackpool) where sedimentary rocks are critically stressed. The last sentence of this section admits this but it does not appear valid to me to makes this strong assertion/ assumption, particularly as displayed in Figure 3.

Reply: The Section on “Stress state” starts by stating that sedimentary rocks are generally not critically stressed. Sedimentary rocks are more ductile or plastic (sometimes called soft rocks) as compared, for instance, to igneous and metamorphic crystalline rocks, which behave in a more fragile manner. As sedimentary rocks have lower stiffness compared to other rocks, stress state is generally more isotropic, i.e., subject to less deviatoric stresses. By this, we are not affirming that they are never critically stressed, but that, in general, this is the case, which is favourable for CO2 storage because injection is performed in sedimentary basins. To support this statement, we will provide a Table with the stress state of CO2 injection sites and the corresponding mobilized friction coefficient. We agree that if one does not read the caption, Figure 3 may give the impression that all sedimentary formations are not critically stressed. Thus, we will modify it and indicate that sedimentary rocks are generally not critically stressed.

C: Page 9 Pressure Buildup Evolution. This may be many, and even incorrect, but is

the term "pressure buildup" used correctly here? This phrase, to me, implies the early stage of injection, or the build-up to max sustained pressure. Here it used to describe what i would call ’pressure evolution’ over the whole project. In the discussion of Figure 4, is this new work? How was it modelled? What are the boundary conditions, scales etc etc? Labelling on the figure also needs improved.

Reply: The described pressure evolution occurs as long as the pressure perturbation front does not reach the boundaries of the aquifer. Figure 4 shows the pressure evolution for the first year of injection, but even if injection is maintained for decades e.g., 30 years), the injection pressure remains practically constant at the injection well. To avoid reaching the aquifer boundary during a 30-year injection, the radius of the model needs to be of some 100 km. In reality, we rarely found aquifers with extremely large size. If a boundary is reached by the pressure perturbation front, injection pressure will increase or decrease depending on whether the outer boundary is low-permeable or high-permeable, respectively. We will provide more details on the model and boundary conditions of the results presented in Figure 4.

C: Page 10. Here the authors state that pressure dissipation can be accommodated by brine leaking through a fault but not CO2. They need to be explicit as to why this is the case. e.g. in the last sentence of this section this should state there is high entry pressure ’to CO2’ specifically and that there is (presumably) a lower entry pressure for brine.

Reply: Since the caprock and faults are fully saturated with the resident brine, brine flow is a single phase problem, and thus, there is no entry pressure for brine flow. In addition, it should be taken into account that the pressurized area will become of several thousands of square kilometres in the long-term. Thus, even for the small flux of brine that will occur across the caprock, the total volume of displaced brine will be very large. We will explain in more detail this aspect.

C: Page 11. Non Isothermal Effects As with the pressure modelling, is this new work here? For example, on lines 6-10 of page 11 is this new work or results from Jeanne et al? lines 11-13. Is this a general comment or for a specific model/conditions? lines 16-18. Why? Is this because there is only cooling in the reservoir not caprock? line 19. Why especially in normal faulting regimes? line 21 - end of section. Is all this discussion (and figure 6) all based on modelling? As for above, what conditions, modelling approach etc etc if it is new.

Reply: As explained in the response to the General Comments, the content of this section is a compilation of recent work (both by the authors and by other contributors in the literature). All of these results are based on numerical modelling. We will explain in the text the relevant conditions to understand the modelling results. Lines 6-13 provide a general explanation of thermo-mechanical effects which have been observed in our simulations and also by other authors. The explanation to the statement made in lines 16-18 is explained in the next paragraph. As it can be seen in the simulation results shown in Figure 5, cooling occurs both in the reservoir and the lower portion of the caprock. Thus, thermal stresses occur in both formations within the cooled region. However, the reduction in the vertical stress within the reservoir generates an imbalance in stress equilibrium. Similarly to what occurs in tunnel excavations, there is a stress redistribution around the cooled region, which results in an increase in the horizontal stresses in the lower portion of the caprock. This increase improves caprock stability in normal faulting stress regimes, because the deviatoric stress is reduced. However, the deviatoric stress increases in the lower portion of the caprock in reverse faulting stress regimes as a result of this stress redistribution. We now provide a more detailed explanation of these processes and their implications.

C: Page 14 Fault Stability. line 6-8. Surely depends on the orientation of the strata (if in sed rocks) relative to the well, not that the well is horizontal? line 24-25. What does 'more deformable' mean? Is this a condition set in the model? page 15 line 8. Why is reservoir stiffer? Is this a condition of the model again?

Reply: What we mean by horizontal well is that it has a long open section, i.e., more

than 1 km, like the wells at In Salah, Algeria. If the storage formation has a slope of some degrees, a “horizontal” well should follow that inclination. Thus, we will change 'horizontal well' by 'parallel to the strata'. By 'more deformable' we mean that the Young's modulus is lower. The modelling results presented in this Section use properties measured in the laboratory. For the reservoir, we consider the properties of Berea sandstone and for the caprock and base rock we consider the properties of Opalinus clay. This is why the reservoir is stiffer than the base rock. We now provide more details on the model.

C: Characterization techniques pg 17 line 1. Stress orientations and magnitudes are pretty hard to measure from core. Can this be changed to 'most properties'. pg 18 line 2. Do we need to be careful here about formation/caprock damage here? How is this different/beneficial to say a XLOT in the caprock? pg 19 line 1. Heterogeneity is the crucial bit here. I'm not sure you can confidently infer the next section (and figure 10) when heterogeneity could easily give the same results.

Reply: In the lab measurements from cores, we were referring to the hydraulic, thermal and geomechanical properties of the rocks. Since the sentence in p. 17 line 1 may lead to confusion, we will rephrase it. As for the potential damage to the caprock, if microseismicity is induced in the caprock, shear slip of fractures may enhance permeability (by one to two orders of magnitude according to lab rock experiments), but most importantly, may reduce CO_2 entry pressure. Thus, it is preferable to limit microseismicity in the caprock. Nevertheless, the amount of assumable damage could vary site specifically. For example, the caprock thickness at In Salah, which was of several hundreds of metres, may allow to accept some damage to the lower portion (some meters) of the caprock because the overall caprock integrity will not be compromised. XLOT should be done in the caprock to estimate the stress state, but the maximum sustainable injection pressure will be always lower than the fracturing pressure.

C: Minimising Risk. pg 21. line 16 onwards. This section/bulletpoint seems a little out of place here. Sure, co-injection etc. could be used but there are other ways to manage pressure too (from straight water production and disposal to changing injection rate, WAG or not etc etc) and for a section entitled other storage concepts there are lots of other methods (basalt storage e.g.). The link to geothermal energy seems out of place/unnecessary.

Reply: With this bullet point we wanted to highlight that fluids, either brine or CO2, can be produced to lower pressure build-up. The intention was not to be an exhaustive review of all proposed methods. And we mention these two alternatives as examples.

C: Figures 4-8 in particular need more scale bars, description of colours used etc. Fig 5 in particular needs better labelling to show which Mohr diagram is for which layer.

Reply: We will add the scale bar to Figure 4. Figures 5-8 already include the scale bar and colour description. The location of the Mohr circles shown in Figure 6 is indicated in the insets of both Figures 6a and 6b, but more details on the exact location of the points will be stated in the caption.

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