Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-129-RC1, 2019

Solid Earth Discussions
Open Access
EGU

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

Anonymous Referee #1

Received and published: 5 February 2019

General Comments

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.

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

Printer-friendly version

Discussion paper

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.

Specific Comments

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.

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.

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.

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.

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.

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?

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.

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.

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.

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

Printer-friendly version

Discussion paper