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Interactive comment on "Unprecedented quiescence in resource development area allows detection of long-lived latent seismicity" *by* Rebecca O. Salvage and David W. Eaton

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All line numbers refer to line numbers in the updated "clean" manuscript i.e. that without the track changes.

1 General Comments

This paper documents the detection and analysis of earthquake activity within a normally-active region of hydraulic fracturing during the cessation of activity

due to lockdowns associated with the COVID-19 pandemic. This study has an interesting and unique position in being able to assess changes in earthquake rate due to a changein hydraulic fracturing activity, alongside changes in earthquake detectability due tothe reduction in background seismic noise. The authors find that seismicity during the lockdown does not display the high-rate, temporally clustered sequences otherwise observed associated with reservoir stimulation, however, they do observe consistent seismicity within previously stimulated regions.

The authors provide good analysis of possible causes of this non-stimulated seismicity, including discussion of triggering from large regional earthquakes, the impact of pore-pressure, and longer-lasting fluid diffusion and poroelastic effects. The authors finally interpret this "latent" seismicity as being due to an altered stress-state within the previously stimulated regions due to trapped fluids, and infer that these earthquakes are driven by aseismic slip. I would like to see more discussion of the interplay between the purported stress-changes, and the strength of the fractures within the reservoir: I wonder if the change in stress is actually the dominant effect, or instead a reduction in fault strength due to prior fracturing would dominate the failure criterion? I'm also curious about how the purported high-pressures are sustained alongside the inter-connectedness of the fracture network?

These are excellent points, and we have tried to address them in the discussion section of the manuscript (L 400-405; L 451-463). We agree that a change in stress or a reduction in fault strength may be responsible for generating this seismicity, which we did not really discuss before, and so we have added fault weakening into our discussion as a possible cause. We have also added more discussion around how aseismic slip may enable the generation of seismicity beyond the pressurized fluid *front (i.e. at large distances from the injection point), as has been hypothesised by a number of authors and have included a number of references (e.g. Eyre, 2019; Wei et al., 2015; Cappa et al., 2018). We think that far-field (tectonic) stresses may play a role in sustaining such slip, as well as direct interaction with a pressurized fault patch over shorter distances.*

Overall I think this is a good, well-written paper documenting an interesting case of reservoir stimulation shutdown. I think it might be relevant to point out the further uniqueness of this study in that most other shutdowns occur either after a large event, which would in itself alter the stress state, or when a reservoir is depleted. I have some specific comments below and some minor technical corrections to the manuscript.

Thank you for your comments. We have tried to emphasize the more unique aspect of this study throughout the text, and in particular in L 59-64 and L 394-399, as you suggest.

2 Specific Comments

1. **Is there any indication of long-lasting changes in the stress-field? e.g. do you observe changes in the stress-ratio or rotation in the principal stress axes associated with the initiation of reservoir stimulation, and is this sustained throughout the background seismicity? I imagine that the seismicity might be too sparse prior to the field becoming active to provide background state, but there may be stress-field data from borehole-breakouts prior to stimulation? I'm also curious about the likely magnitude to stress-variation due to hydraulic fracturing.**

   You are quite correct that the seismicity is too sparse prior to the KSMMA be-

   coming active to really detail this. Source characteristics of the events identified as "background" seismicity" are mostly not accounted for. We did find some information regarding borehole breakouts, and a recent study by Babaie-Mahani et al. (2020) which details recent (2018-2019) stresses (from focal mechanism analysis), however a comparison of before and after is not possible due to the limited data. We have added a number of paragraphs into the discussion for this (L 366-393), however at this time we are unable to categorically give an answer to this question as it represents a large area of research that is yet to be undertaken.

2. **Is there any other evidence of aseismic creep? I am not familiar with the paper by Eyre (2020), but I wonder if they found any characteristic temporal evolution of seismicity that they associated with aseismic slip? I'm also curious about why an aseismic driver is required? It is not generally assumed that background seismicity requires an aseismic driver, could this not just be the "new-normal" background seismicity after fractures were weakened due to hydraulic fracturing?**

   We have now included more references to aseismic slip in hydraulic fracturing environments, and tried to point out the characteristics that we observe in our seismicity that we believe are indicative of this mechanism (L 473-477). This includes the long-lived persistant nature of the seismicity (swarm-like activity rather than a typical mainshock-aftershock sequence), the lack of hypocenter migration (away from a point of injection), as well as dominance of low frequency energy in the waveforms.

   It is possible that the seismicity detected reflects a new normal in terms of background seismicity, with event counts now greater than they were prior to operations in this area, and we have added this comment in to the manuscript

(L 400-405). We were trying to suggest that the generation of this seismicity (whether it is latent and a direct consequence of the recent operations, or whether it is the new background rate) could be generated by aseismic slip (as a result of tectonic forces), or through other mechanisms such as fracture weakening. We have tried to make this more clear in the manuscript by softening our language to advocate that aseismic slip is a proposed mechanism for this seismicity generation, rather than the only mechanism.

3. **It would be great to have some well-stimulation data to confirm your suspicions in Line 125. I imagine that this is hard to come by, and if so, can you add a note around line 125 to say that well-data were not available.**

   As you note, this information is proprietary between the regulators and the individual companies operating in this area, and as such we cannot comment further on this as we do not have access to this information.

4. **Around line 267 the authors argue that upwards of 70% of the earthquakes "cannot be explained by this mechanism [natural seismicity]". The argument is based on Gutenberg-Richter scaling, which is quite a weak argument for such a strong statement. Furthermore, the statement of scaling is based on a b-value of 1, which the authors do not find for this region - I'm curious about the reason for using a b-value of 1? Finally: it is hard to make such a strong statement, given that natural earthquake (de-clustered) distributions appear to be almost Poisson and can have variations in rate in time. I suggest more circumspect language in this statement rather than "cannot be explained".**

   We agree that the language here needs to be softened, and we have tried to do so throughout the text in relation to this point. We used a b-value of 1 since

this is the expected b-value for natural seismicity in North America (Frohlich and Davis, 1993; Godano et al., 2014), and as we were trying to determine whether any events could be deemed "natural", we chose this. We have added this explanation into the text (L 321-323). It is common to have much higher b-values associated with hydraulic fracturing experiments (as we noted in the text and found in our study), but the aim of this part of the discussion was to determine whether any/some/all of the seismicity may be natural. We have tried to make this clearer.

5. **I am surprised that your magnitude of completeness appears to have gone up during the shutdown and I'm curious to hear why you think this is? It would also be useful to state what method was used to compute the magnitude of completeness.**

   We have added the method we used to calculate the magnitude of completeness (maximum curvature method). We originally included this comment for completeness, however, upon reflection we can see that it is confusing and we have therefore removed the sentence regarding the Mc for the entire catalogue from this paper. We believe that the differing Mc is an artefact due to differing methods used in the magnitude calculations between Salvage et al., 2021 and this paper, meaning they are not directly comparable.

3 Technical Comments

Below I have listed my technical comments starting with the relevant line number:

- **23: Change "on such" to "of such"**

  This was a typo and has been changed.

- **64: Change "Z-component" to "vertical-component"**

  We have changed all references to Z-component to now read the vertical component.

- **Lines 89-95: While the detection methods are not the key topic of this paper, and it only really matters that the detection method is consistent throughout the study, it would be good to have a little more detail and cite relevant papers - ideally citing another paper using the same methodology would be useful here.**

  We have added a couple of sentences relating to the methodology employed here, as well as reference to a paper that uses it (L 106-114).

- **134: What was the magnitude of completeness for these precursory events? Saying "A total" suggests absolute completeness.**

  This was not our intended meaning. We have changed the sentence to reflect that we simply wanted to state the number of events in the precursory sequence. Due to the small number of events in this sequence, the Mc was calculated to be $\sim$*1.1.*

- **175: Remove trailing "a" at the end of the line**

  This has been removed.

- **218: "Data is" should be changed to "Data are" - it also isn't clear which data are being referred to here.**

  We have changed this to be gramatically correct.

- **258: I suggest changing "non-existent" to "undetectable" given the limitations of the seismic networks available.**

  This has been changed.

- **314: "Stress field would likely diminish": I suggest rephrasing this, it is hard to imagine the entire stress field diminishing, but there certainly might be a change in orientation and magnitude of principal stress axes. This statement could also do with a citation.**

  We have changed this sentence to be more accurate (and realistic), and included a reference as you suggest (L 446-447).

- **320: I don't know the paper cited here, but aseismic slip has long been associated with seismicity, so I assume that this paper talks specifically about aseismic slip in hydraulic fracturing environments: It would be good to make that clear in this statement.**

  This has been changed.

- **365: Change "always" to "since Oil and Gas production started" or similar.**

  This has been changed.

- **Figure 2(b): The inset and key are not needed as far as I can tell because they repeat from 2(a) - happy to have them left in, but if you do, can you add in the white box on the inset map showing the main figure location I saw this in 2(a), but not 2(b).**

  Figure 2 has been updated to include all seismicity from 2020 (at the time of submission it only included seismicity to October). We have also increased the text size in the figure, and made sure the inset map includes the white box. We have left the legend in both plots.

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