February 13, 2025

Dear Yen Joe Tan,

Thank you for your constructive review of our manuscript titled "Evidence for Far-Field Wastewater Disposal Causing Recent Increases in Seismicity in Central and Northern Kansas". We include point-by-point responses to the external reviews provided below. I would also like to thank you again for your patience and understanding with the time it took me to resubmit this paper after moving and getting settled into my new position.

Sincerely,
Shannon Fasola
Mike Brudzinski
Noel Jackson

Editor (Yen Joe Tan)

Dear Shannon Fasola, Mike Brudzinski, Noel Jackson:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Evidence for Far-Field Wastewater Disposal Causing Recent Increases in Seismicity in Central and Northern Kansas". Thank you once again for submitting your work to Seismica.

Both reviewers agree that your manuscript can be an important contribution for improving our understanding of induced seismicity and seismic hazard in Kansas, but have suggestions that I think will help improve the clarity of your manuscript. Therefore, I believe your manuscript may be suitable for publication after some revisions.

When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Yen Joe Tan
The Chinese University of Hong Kong
yjtan@cuhk.edu.hk

Reviewer B

The authors present a detailed analysis of induced seismicity across Kansas. They make a remarkable effort to enhance the seismicity catalog using template matching. There is a vast increase in the number of earthquakes and there are significant implications for Kansas oil and gas related activities. This is an important finding and I would like to see this paper published. I admire the efforts the authors went to for this research.

There are a number of things in this paper that could be improved though. I recommend moderate revisions since there are many things that potentially need to be addressed. My suggestions fall into two categories: 1) ways to improve the flow + presentation of the paper, and 2) seeking clarity on certain concepts/hypotheses used in the paper.

Broadly speaking, there are a few main points that need to be addressed:

1. Length of paper. This is a very long paper! I think a lot of text could be removed/shortened; there is a lot of unnecessary repetition. There are several concepts that are repeated both in the introduction and then later in the manuscript.

   We made an effort to cut down the length of the paper by removing 'extra' information not crucial to the conclusion of this paper and repetitive content, and by being more concise during the discussion section. As also suggested by this reviewer, we more thoroughly divided up the discussion into more identifiable and organized subsections with shortened descriptions. This allows the reader to have access to the details if they are interested but also provides a summary if they just want to hear the key findings.

2. B-value and causal mechanisms. The authors lean heavily on a hypothesis that low b-values are related to fluid injection. I'm not sure how this can be true since b-values are traditionally thought to reflect the stress state. Perhaps a more accurate way of saying things would be that low b-values imply a stress state that is closer to failure (this has been very well demonstrated with lab studies of seismicity). It's rather clear that all of the earthquakes shown here are induced by fluid injection; there is no need to link that with the b-values.

   Based on this comment, this reviewer's 3rd comment, and a comment from Reviewer C, we removed the background information on b-values from the introduction and made the b-value discussion more concise to place less emphasis on our b-value interpretation. Given the

reviewer's statement "It's rather clear that all of the earthquakes shown here are induced by fluid injection; there is no need to link that with the b-values.", we sought to simplify the paper by not trying to link fluid injection with seismicity as strongly as we were trying. We also adjusted the wording in the b-value discussion based on the reviewer's comment about b-values being traditionally thought to reflect the stress state.

3. B-value stability. As the authors have demonstrated, b-values are notoriously unstable and difficult to calculate, especially over long time scales with variable magnitude of completeness. I would suggest using the van der Elst 2019 method of b-positive. It's very easy to use and code up and yet it provides much more reliable b-values than traditional methods. This may help with the challenge the authors see with the b-value dependence on magnitude of completeness and lend more credibility to the results.

   Thank you for this suggestion. We calculated the b-positive values using the van der Elst (2019) method and adjusted the text and the figure accordingly.

4. CoV interpretation. The authors also lean on the hypothesis that CoV can be used to determine if there is far-field injection related seismicity. This seems like a stretch to me. I've studied induced seismicity across many basins and I don't see how interevent time and distance from the anthropogenic forcing are related. I would interpret the CoV results as a reflection of the properties of the faults themselves.
   1. If CoV is low (~1), I would interpret this as a cluster where there is a steady driving force for seismicity. E.g., continuous wastewater injection, aseismic slip, or earthquake-earthquake interaction.
   2. If CoV is high (>2), I would interpret as a fault system that is very sensitive to small changes in stress. E.g., we see this with hydraulic fracturing and near critically stressed faults.
   3. That is, in my work I've seen a very strong link between injection type (HF vs. WD) and CoV. Since HF has little or nothing to do with the seismicity here, this means we have faults that are critically stressed. We see something to this effect in the Delaware basin; clusters on the edges of the basin have high CoV. I think this is something worth discussing and may likely warrant future work to truly understand (outside the scope of this paper).

   This was a helpful perspective on the interpretation for us to consider. We would point out that there is some relationship to our initial distance-related interpretation because in the Delaware Basin case, the cases with high CoV being interpreted as critically stressed faults are the ones that are far from injection. That is, the critically stressed faults are the ones that can be activated by distant injection since only a small change in effective stress is needed to induce seismicity on these faults. However, we agree that the description would be better with your interpretation woven together with ours. We have adjusted the text accordingly.

5. Effect of EOR. I think Figure S4 is excellent and I would advocate for moving it to the main text (Discussion). I think this paper might be in a good position to (gently) ask the question about the importance of EOR. I'm not advocating for any more work to be done, but rather, for the authors to discuss the possibility that EOR may play a role. I mention this because EOR is vastly under-studied, but it does cause large magnitude seismicity. The Snyder cluster in Texas is an example (see Gan & Frohlich, 2013: Gas injection may have triggered earthquakes in the Cogdell oil field, Texas).

   We moved Figure S4 into the main text and added a sentence to mention that the relationship between EOR and seismicity is vastly under-studied, yet EOR has been proposed to have caused large magnitude seismicity in Cogdell, TX oil field.

6. Format of results. Perhaps my view is traditional but I think the results should be more objective. For example, I would leave the (subjective) interpretation of CoV and b-value to the Discussion. This would also help cut down on the text.

   We moved the discussions of COV and b-values to a separate section on Summary of Seismicity Findings.

7. Format of discussion. The discussion at present is long and difficult to follow. I would suggest re-writing to a clearer format. For example, 4.1. summary of findings (enhanced catalog), 4.2. b-values and CoV interpretation, 4.3. relationships between seismicity and injection activities (but shortened from the current version), 4.4. Implications and sources of uncertainty.

   We have responded to this suggestion by more thoroughly dividing up the discussion into more identifiable and organized subsections. This includes a Summary of Seismicity Findings with a new table to help with the comparison across regions, a separate Relationship to Industrial Activities section with subsections for extraction/EOR/HF and each of the geographic regions but with shortened descriptions, and a clearer summary at the end.

8. Distant triggering mechanism. Another challenge with these findings is the apparent long-distance triggering. Although I am often one to favor the unlikely, I think some serious thought needs to be done here. The west Delaware basin has a similar observation – I think it's worth mentioning in the discussion. The current hypothesis is that distant deep-injection is causing seismicity there even though there are no nearby HF/SWD wells. However, this is a matter of current research and controversy. I suggest speaking with Jake Walter if possible – he may know this region better and have some ideas.

   We have responded by incorporating references to the long distance triggering in Oklahoma and Texas. We have also added a potential explanation provided by Goebel and Brodsky (2018) that injection above the basement into sedimentary layers is more likely to create a larger spatial footprint of seismicity. CKU injection occurs into the Arbuckle, which has very high permeabilities and diffusivities. So our interpretation now more clearly states how we propose the Jewell County seismicity is caused by efficient transmission of fluid pressures from the dense region of high-rate wells in CKU to highly sensitive, critically stressed faults in Jewell County.

I also have some specific comments/questions:

1. Figure 1. May be helpful to have a horizontal line at 37.75 degrees, especially for (a).

   Since Figure 1 is already busy, instead of adding a horizontal line at 37.75 degrees, we moved the latitude labeling to the right-hand side so the 37.75 degree tick mark is more obvious.

2. Line 395. I don't understand the sentence or the claim – typo?

   There was a typo here, but this sentence was also removed from the manuscript during the process of shortening the manuscript.

3. Section 5.1 should be in the discussion, not the conclusions.

   Section 5.1 has been moved to the Discussion.

Overall, this is a very interesting paper that warrants publication. After some changes, I think this will be a very strong paper. Although my comments have turned out to be lengthy, my intention is for the paper to really shine and highlight the great work that has been done.

Recommendation: Revisions Required

Reviewer C

This study examines the recent increases in seismicity in central and northern Kansas by using waveform cross-correlation and b-value analysis and comparing with well locations, volumes, stimulations, and pressure. The results suggest that wastewater disposal may influence seismicity up to 50-100 km away from disposal wells, farther than previously thought. This could have implications for the seismic hazard in Kansas, as well as potential future energy projects, such as carbon sequestration. The study is on an interesting topic with both scientific and societal implications, and is certainly appropriate for Seismica. It is also well written and generally easy to follow. I also appreciate the effort that was gone through to compile, clean up, correct, and provide the various data sets. I only have a few comments that I think would be helpful to address before it is accepted for publication.

A general comment – it might be helpful to have a table in the main text summarizing some of the seismicity parameters for each study region (e.g., the less and more conservative b-values, Mc, and COV), just to make it easier to compare the values for each region at a glance.

A table with the seismicity parameters was added to the manuscript as suggested.

Minor comments:

L. 110: Maybe cite the value of the previously suggested limit?

Done.

L. 142-143: The meaning of "a large number of earthquakes relative to the maximum magnitude" is a little unclear. Does this mean that these earthquakes aren't following a similar productivity law as typical

aftershock sequences (e.g., there are more earthquakes – above some magnitude? – than would be expected given a mainshock at the maximum magnitude)?

This phrase was removed from the sentence as it was a typo. The unclear phrase was a repetition of the next phrase 'multiple events near the maximum magnitude'.

L. 147-153: I was wondering if you could clarify a bit more the b-value changes that have been associated with induced seismicity. The way this section is worded makes it sound as if injection induced seismicity in general tends to have lower b-values than natural seismicity (l. 151), which I found a little surprising (and by "naturally occurring" seismicity, do you mean seismicity in general, or earthquake swarms?). I believe there have been some observations of high b-values associated with induced seismicity (e.g., Atkinson et al., 2015), although b-value estimation can potentially be difficult with changing catalog completeness and non-GR distributions that could occur with induced seismicity (see, e.g., Geffers et al., 2022).

Gail M. Atkinson, Hadi Ghofrani, Karen Assatourians (2015). Impact of Induced Seismicity on the Evaluation of Seismic Hazard: Some Preliminary Considerations. *Seismological Research Letters,* 86(3), 1009–1021, https://doi.org/10.1785/0220140204

G-M Geffers, I G Main, M Naylor (2022), Biases in estimating *b*-values from small earthquake catalogues: how high are high *b*-values?, *Geophysical Journal International*, 229(3),1840–1855, https://doi.org/10.1093/gji/ggac028

Based on Reviewer B's comments on the b-value discussion we removed this section from the introduction (as well as due to length). While yes induced seismicity in general tends to have lower b-values, as we and Reviewer C mentioned, there have been cases of high-values associated with hydraulic fracturing. As suggested by Reviewer B, we calculated b values using the van der Elst (2019) method which does not require the estimation of Mc. This method is thought to be more robust.

L. 201-205: Can this statement be further explained/clarified? It is not clear to me why earthquake interactions being the primary control of clustering further from WD wells should be suggestive of far-field pressure effects. To me, this suggests instead that farther from the wells, earthquakes are more likely to trigger one another than pressure effects.

Based on Reviewer B's comments on the CoV discussion we revised this section to clarify the behavior of critically stressed faults is the primary driver, but that small stress fluctuations associated with far-field WD can allow for more clustering due to earthquake-earthquake interactions.

L. 394-396: I thought that fluid extraction could also cause normal-faulting earthquakes (e.g., Segall, 1989; Gomberg and Wolf, 1999; Zoback and Zinke, 2002; Wetzler et al., 2019).

Segall, P. (1989). Earthquakes triggered by fluid extraction. *Geology,* 17 (10), 942–946. https://doi.org/10.1130/00917613(1989)017<0942:ETBFE>2.3.CO;2

Gomberg, J., and L. Wolf (1999). Possible cause for an improbable earthquake: The 1997 M_w 4.9 southern Alabama earthquake and hydrocarbon recovery. *Geology*, 27(4), 367–370. https://doi.org/10.1130/0091-7613(1999)027<0367:PCFAIE>2.3.CO;2

Wetzler, N., Shalev, E., Göbel, T., Amelung, F., Kurzon, I., Lyakhovsky, V., & Brodsky, E. E. (2019). Earthquake swarms triggered by groundwater extraction near the Dead Sea Fault. *Geophysical Research Letters*, 46, 8056–8063. https://doi.org/10.1029/2019GL083491

Zoback, M.D., Zinke, J.C. (2002). Production-induced Normal Faulting in the Valhall and Ekofisk Oil Fields. In: Trifu, C.I. (eds) The Mechanism of Induced Seismicity. Pageoph Topical Volumes. Birkhäuser, Basel. https://doi.org/10.1007/978-3-0348-8179-1_17

We corrected this sentence to indicate dip-slip faulting instead.

L. 632-635: What is the current seismic hazard map for Kansas? Please add a reference. Is there a state-produced seismic hazard assessment, or is this referring to the USGS 1-year hazard forecasts for induced seismicity?

We were referring to the 2023 US National Seismic Hazard Model (NSHM). We added this reference to the paper. We added more clarification for that sentence: Results from this study are also important for reconsidering the seismic hazard in Kansas. Induced earthquakes are not included in the traditional National Seismic Hazard Model as they are thought of as temporary features of seismicity, and a short-term (1 year) model forecast that includes induced seismicity has not been issued since 2018 (Petersen et al., 2024).

Petersen, M. D., Shumway, A. M., Powers, P. M., Field, E. H., Moschetti, M. P., Jaiswal, K. S., ... & Witter, R. C. (2024). The 2023 US 50-state national seismic hazard model: Overview and implications. *Earthquake Spectra*, *40*(1), 5-88. DOI:10.1177/87552930231215428

Supplement Section S1.1: How were the Mc values for the ComCat and KGS catalogs calculated?

We used the same method for calculating M_C values as for the template matched catalog. M_C was determined using the maximum curvature algorithm (Wiemer and Wyss, 2000). We added this clarification to the Supplement Section S1.1

Supplement Figure S3: In this figure, was Mc=1.95 used as the conservative Mc value for all of the areas? The catalog doesn't look quite complete down to this magnitude for the Jewell Combined and Salina 2001 catalogs (panels c and f).

Yes Mc=1.95 was used as the conservative Mc value for all of the areas. Because using the traditionally calculated MC value and our conservative value confused the reviewers, we sought to clarify the use of Mc. We included the traditionally calculated Mc values in Table 1 but we used a conservative value of Mc=2 for geospatial uniformity and to not miss events during our comparisons of seismicity and wastewater disposal. Looking at the FMD plots the Mc appeared to be closer to M 2 where we see the data roll off and where we set the inflection point for recalculating the b-values. Now that we are using the b-positive method to calculate the b-values, the Mc is less important in this study.

Supplement Figure S6: Please add what the different colored symbols represent in this figure.

Clarifying text has been added to the figure caption.

Recommendation: Revisions Required