Review Round 1:

Reviewer A

This paper presents a regional stress model for the Greater Permian basin of Texas. The results of this model, as well as mapped faults and observed seismicity are used to evaluate the seismic deformation caused by induced seismicity in this region. This research contributes to our knowledge of how stress accumulates within stable continental zones, as well as how oil and gas extraction may impact the seismicity of the area.

The research area is an important one, and within the public interest. The data and methods presented in this paper are clear and I believe will be good contributions to the literature. However I do have several suggestions for revisions and expansions to the discussion before this should be accepted, please see below.

Main Comments:
In the abstract, the seismicity induced by fluid injection within the basin is said to have generated significant seismic-moment release. I think that this is an important discussion point and would appreciate if a paragraph could be added, contextualizing the current debate regarding induced seismicity better. Additionally, I wonder if there is a way to quantify this seismic-moment release, or at the very least compare seismic moment release values for a period before fluid injection activities started, with that of seismic-moment release values since fluid injection started.

Regarding the format of the paper, I think that the Results and Discussion section ought to be split into two sections. As it currently is, this somewhat makes it hard to separate out what your results and observations are from the interpretations made.

I think that the manuscript would be aided by a figure of the stress model, one of the main results. Ideally this would be something that could be easily compared with other studies that cover the Permian Basin - such as a plot of SHmax orientations, similar to that of Figure 1 in Lundstern (2024).

One of my main concerns is that not enough discussion is given to the following sentence from the conclusion "the geodetically observed ground-surface subsidence in parts of the Permian Basin of Texas is unlikely caused solely by the current production activities (i.e., extracting hydrocarbons out of the underground geologic formations), but rather is due to systematic reactivation of normal or strike-slip faults." While there is some discussion of this, I think that there is more to the story than presented. For one, fluid injection might be one of the root causes for fault activation as the injection of fluids would increase the pore fluid pressure, reducing the failure criterion and potentially even lubricating the faults - thus the reactivation of these faults could very well be caused by current production activities. If the authors disagree with my logic here, then I would like to see some conceptual model presented for their arguments. Or perhaps some geological cross sections across the basin, showing the arrangement of the reactivated faults and that explains how ground subsidence might be accommodated.

Minor Comments:
Lines 23-24: re-using the phrase "tectonic significance" here feels repetitive, consider removing or rewording.

Line 31-32: consider changing wording to bold text “This heterogeneity is further evidenced by …”.
Line 34: typo “evaluate”.
Line 35: consider changing wording from “stronger” to “higher” seismic moment release as this is a measurable quantity and prevents repetition of stronger later on in the sentence.
Line 45: It would be nice to know what the depth range of seismicity is here without having to go to the references.
Line 99: The wording of this sentence is a little strange, I think there might be a word or two missing here, what was based on the waveform misfit and percentage of double-couple component?
Line 102: consider changing the wording regarding the misfit range (i.e. the misfit ranges between 0 and 2). Similar wordings for ranges is used throughout the manuscript, if you change the wording here, then please adjust all similar uses.
Line 131: typo “recommendation”.
Lines 133-134: Consider rewording to “We excluded grid-points without enough focal mechanisms from the inversion”.
Lines 146-149: is this not a part of the results section?
Line 151: wording for range could be changed here
Lines 172-173: Remove “by natural” from the end of the sentence “when S1 is mostly near vertical (plunge >70), its azimuth is less well defined”.
Lines 190-192: “This corroborates the fact that…”
Line 214: Consider changing “these” to “the” as it is in the beginning of a paragraph and the number of earthquakes was not mentioned previously
Line 218: remove “however”
Lines 219-220: The wording of this paragraph could be tightened up (Remove “Also”). Consider replacing “those” with “the”.
Line 358: typo “computer”
Line 359: typo “accessed”

Supplemental Materials: change the footer with the supplemental materials to reflect the correct journal.

Figure 1: The seismicity is plotted very small, and small clusters can be particularly hard to see, potentially try a slightly larger symbol? The frame annotation intervals are very busy, considering using a larger interval to reduce visual clutter.

Figure 2: I would really like to see panel a larger, especially as the seismicity within it is plotted so small. Consider scaling the histogram x axis to see the distribution a little more clearly

Figure 3: Why is the background grey? If it wasn’t coloured, the sub panels wouldn’t be as noticeable. Could label the basins. Consider using a colour-blind friendly color palette.

Figure 4: Not referenced in the text. Similar comments regarding background grey and the colour palette used to those in Figure 3.

Figure 5: Similar comments regarding background grey and the colour palette used to those in Figure 3.

Reviewer B

This manuscript studies the induced seismicity of an area in Texas where there has been fluid injection and hydrocarbon extraction. The key aim of this study is to invert the stress field from the focal mechanisms of recorded earthquakes. However this manuscript falls short of this aim, primarily due to a lack of information given about the hydrocarbon/injection activities and the interpretation of the stress field. I will note that my expertise is in the broad field of earthquakes, tectonics and stress field, rather than specific expertise in induced seismicity, however my concerns about the data presented and the methodology mean that the conclusions of the paper are not supported.

Missing information about the hydrocarbon/injection activities

There is no information included about the hydrocarbon activities that are hypothesised to cause the seismicity, for example injection wells, or fluid injection volumes. How does the induced seismicity compare spatially and temporally to hydrocarbon activities? How can you tell the difference between an induced earthquake versus a "natural" tectonic earthquake?

Interpretation of the inverted stress field

Additionally, I have concerns about the interpretation of the stress field. Injection of fluid into the sub-surface will locally change the magnitude and orientation of the stress field around the injection site (e.g. flipping sigma1 and sigma3). How can the authors be confident that the stress field they have inverted is truly representative of the tectonic stress, rather than the local stress field? Is this perhaps why there is a difference between the stress field inverted from borehole breakouts (line 54-68).

Organisation of the manuscript

At present, the manuscript is challenging to read and to follow the key arguments being made. There are results described in the methods section, methods given in the results section, and the results and the discussion sections are amalgamated. This means that it is difficult to follow what has been done in this study versus other previous studies. There are many sentences that are grammatically poor and difficult to understand, I have highlighted some of these in the pdf. I suggest the manuscript is reorganised into a more conventional layout, to help guide the reader through the evidence and arguments being made.

I have made other minor comments and suggestions as comments and highlights in the pdf which I hope are helpful to the authors

Zoe Mildon

Dear Dino, Yangkang Chen, Alexandros Savvaidis:

I hope this email finds you well. I should first introduce myself as the handling editor for your submission to Seismica, "Mapping the 2-D Crustal Stress Field and Seismic Moment Release for the Greater Permian Basin of Texas as Constrained by Earthquake Source Mechanisms". Thank you once again for submitting your work to Seismica, and please contact me via email or the discussion feature on the Seismica submission system should you have any questions.

After a quick turnaround from the reviewers, I have come to a decision to request that you revise the manuscript. Two reviewers have read the paper, and although they see value in the study, they also request, between them, quite substantial revision.

It is clear that the paper needs significant restructuring, in particular to separate observation and interpretation; results and discussion need to be different sections to achieve this. Another major issue is the degree to which seismicity (and stress states) can be ascribed to tectonic or anthropogenic processes.

Responses: As far as we understood, the "*anthropogenic process*" referred by the editor has the same meaning with "induced seismicity", whereas "tectonic" means the naturally occurred earthquakes.

To clarify, the oil&gas activities are widely spread out within the Permian Basin. The injection and extraction can occur independently in time and space at wells owned by different operators. They do not synchronize with each other. Studies have concluded that seismicity in the Permian Basin is certainly induced (i.e., "anthropogenic"). The seismogenic faults in the Permian Basin are under the heavy influence of the oil&gas activities. In other words, we consider the inverted model field as a representative of the "present-day crustal stress field". Given the current intense oil&gas activities, it is very challenging to separate the naturally occurred earthquakes from the induced one.

To better reflect the situation, we have changed the title of the paper to "Mapping the Present-day 2-D Crustal Stress Field and Seismic Moment Release for the Greater Permian Basin of Texas as Constrained by Earthquake Source Mechanisms".

To me the important revision is to clearly state what is known and what is inferred: what is the nature of the potentially seismicity-inducing fluid-injection and hydrocarbon extraction, what is the stress field and seismicity during times of hydrocarbon extraction and fluid-injection, and is there any control on stress and seismicity from before these activities began? Then, a separate discussion section can discuss the role of fluid-injection and hydrocarbon extraction on stress state and seismicity will all the facts already on the table.

Responses: There has been a long history of oil&gas activities in the Permian Basin before the wide-spread seismicity began. At that time, there was very limited coverage of seismic network and few documented seismicity as compared to what we currently have. Therefore, it is very challenging to get a good sense about the stress state before the induced seismicity began. As far as we are concerned, the faulting type projected by the inverted stress field corroborates the historic focal mechanisms (the number is few, though) that occurred decades ago.

Currently, a lot of the discussion is opinion, without a clear analysis of what effects injection and extraction may have, which should also be clarified in the new discussion section. For example, wouldn't fluid injection simply reduce effective stress hydrostatically, i.e. not rotate the stresses or change shear stress, whereas extraction may have a more anisotropic effect (for example, reducing fluid pressure but also, potentially triggering subsidence and reducing overburden). I am speculating here - but the manuscript as it stands makes bold statements without reasoning, which needs addressing (generally as highlighted by the reviewers.

Responses: One of the effects resulted from the increased pore-fluid pressure is exactly reducing the effective normal stress to favor the fault to slip. However, studies have suggested that the increased pore-fluid can also temporarily reorient the stress field around the injection well. The degree of reorientation depends on the magnitude of the injection pressure. The hydrocarbon extraction does reduce the overburden because the hydrocarbon bodies were withdrawn, but it could also increase the asperity of the nearby faults because of the compaction of the reservoir. The increase of the asperity can build up more seismic strain. Therefore, the future earthquake hosted by the same fault can come up with a higher magnitude. We have seen this hypothesized effect in other places of Texas where hydrocarbon extraction has caused more stronger earthquakes over times.

We have made revision accordingly to address the comments from the reviewers and the editor.

Overall, addressing the reviews requires some new analysis but mainly a substantial rewrite of the manuscript.

Please see below for the attached, external reviews. Note one reviewer attached an annotated manuscript that is too large for the email server - I will upload this to the discussion on your paper.

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,
Ake Fagereng
Cardiff University
ake.fagereng@seismica.org

Reviewer A:

This paper presents a regional stress model for the Greater Permian basin of Texas. The results of this model, as well as mapped faults and observed seismicity are used to evaluate the seismic deformation caused by induced seismicity in this region. This research contributes to our knowledge of how stress accumulates within stable continental zones, as well as how oil and gas extraction may impact the seismicity of the area.

The research area is an important one, and within the public interest. The data and methods presented in this paper are clear and I believe will be good contributions to the literature. However I do have several suggestions for revisions and expansions to the discussion before this should be accepted, please see below.

Main Comments:

In the abstract, the seismicity induced by fluid injection within the basin is said to have generated significant seismic-moment release. I think that this is an important discussion point and would appreciate if a paragraph could be added, contextualizing the current debate regarding induced seismicity better. Additionally, I wonder if there is a way to quantify this seismic-moment release, or at the very least compare seismic moment release values for a period before fluid injection activities started, with that of seismic-moment release values since fluid injection started.
<u>Response</u>: Regarding the historical moment-release, we could have collected all available information from literature or public database (e.g., the ANSS catalog). Because the reported seismicity rate is not comparable to the current one, it would be logical to compare the historic moment-release and the current one at the same magnitude level (i.e., M≥4). We have added a short paragraph in the introduction to describe the historic moment-release (lines 60-65 in the revised manuscript).

Regarding the format of the paper, I think that the Results and Discussion section ought to be split into two sections. As it currently is, this somewhat makes it hard to separate out what your results and observations are from the interpretations made.
<u>Response</u>: We have split the “Results and Discussion”.

I think that the manuscript would be aided by a figure of the stress model, one of the main results. Ideally this would be something that could be easily compared with other studies that cover the Permian Basin - such as a plot of SHmax orientations, similar to that of Figure 1 in Lundstern (2024).
<u>Response</u>: Agreed. We have added SHmax of ours in new Figure 5.

One of my main concerns is that not enough discussion is given to the following sentence from the conclusion “the geodetically observed ground-surface subsidence in parts of the Permian Basin of Texas is unlikely caused solely by the current production activities (i.e., extracting hydrocarbons out of the underground geologic formations), but rather is due to systematic reactivation of normal or strike-slip faults.” While there is some discussion of this, I think that there is more to the story than presented. For one, fluid injection might be one of the root causes for fault activation as the injection of fluids would increase the pore fluid pressure, reducing the failure criterion and potentially even lubricating the faults - thus the reactivation of these faults could very well be caused by current production activities. If the authors disagree with my logic here, then I would like to see some conceptual model presented for their arguments. Or perhaps some geological cross sections across the basin, showing the arrangement of the reactivated faults and that explains how ground subsidence might be accommodated.

Response: To clarify, hydrocarbon extraction in the Permian basin does involve fluid injection but its scale (i.e., the injecting volume) is not comparable to wastewater disposal. During the hydrocarbon extraction, fluid injection is used to retrieve hydrocarbons. And yes, we agreed that the injected fluid should have the same effect to reduce the normal stress and thus to reactivate pre-existing faults. However, those injected fluid during the hydrocarbon retrieval will have to flow back to the ground surface whereas wastewater disposal is continuously flooded into the reservoir and never flows back to the ground surface. Therefore, wastewater disposal is more likely to trigger a systematic and large-scale fault-reactivation. In comparison, the faults reactivated during hydrocarbon extraction are segmented and having smaller-sized faulting area (i.e., meaning a small slippage).

In other words, we agreed that the process of hydrocarbon extraction is capable of triggering fault-reactivation as well. From the original manuscript, what we meant was that simply the **disappearance/reduction of hydrocarbon bodies** alone CANNOT SOLELY account for the geodetically observed ground surface subsidence as claimed by Staniewicz et al. (2020). Instead, the reactivation of normal and strike-slip faults (triggered by wastewater disposal and the fracking-related fluid) is the root cause. Lee et al. (2023) has provided results of geomechanic modeling to support this assertion. We have revised our statement accordingly (see discussion in the new section 4).

Minor Comments:

Lines 23-24: re-using the phrase “tectonic significance” here feels repetitive, consider removing or rewording.
Response: Agreed. Changes noted.

Line 31-32: consider changing wording to bold text “This heterogeneity is further evidenced by …”.
Response: Changed noted.

Line 34: typo “evaluate”.
Response: Changes noted.

Line 35: consider changing wording from “stronger” to “higher” seismic moment release as this is a measurable quantity and prevents repetition of stronger later on in the sentence.
Response: Agreed. Changes noted.

Line 45: It would be nice to know what the depth range of seismicity is here without having to go to the references.
Response: We have added such description in the revised introduction.

Line 99: The wording of this sentence is a little strange, I think there might be a word or two missing here, what was based on the waveform misfit and percentage of double-couple component?
Response: To clarify, we meant to say that we used two criteria to select focal mechanisms (determined by waveform moment-tensor inversion) for stress inversion: 1. The misfit of waveform modeling resulted from the waveform moment tensor inversion for each individual earthquake, 2. the percentage of DC component out of the inverted full moment tensor (i.e., DC+CLVD+ISO=100%). We have rephrased the sentence.

Line 102: consider changing the wording regarding the misfit range (i.e. the misfit ranges between 0 and 2). Similar wordings for ranges is used throughout the manuscript, if you change the wording here, then please adjust all similar uses.
Response: We have rephrased the wording.

Line 131: typo “recommendation”.
Response: Changes noted.

Lines 133-134: Consider rewording to “We excluded grid-points without enough focal mechanisms from the inversion”.
Response: Changes noted.

Lines 146-149: is this not a part of the results section?
Response: We moved this part to the new section of results.

Line 151: wording for range could be changed here
Response: We rephrased the wording.

Lines 172-173: Remove “by natural” from the end of the sentence “when S1 is mostly near vertical (plunge >70), its azimuth is less well defined”.
Response: Changes noted.

Lines 190-192: “This corroborates the fact that…”
Response: Changes noted.

Line 214: Consider changing “these” to “the” as it is in the beginning of a paragraph and the number of earthquakes was not mentioned previously
Response: Changes noted.

Line 218: remove “however”
Response: Changes noted.

Lines 219-220: The wording of this paragraph could be tightened up (Remove “Also”). Consider replacing “those” with “the”.
Response: We changed the words “also” and “those”.

Line 358: typo “computer”
Response: Changes noted.

Line 359: typo “accessed”
Response: Changes noted.

Supplemental Materials: change the footer with the supplemental materials to reflect the correct journal.
Response: Changes noted.

Figure 1: The seismicity is plotted very small, and small clusters can be particularly hard to see, potentially try a slightly larger symbol? The frame annotation intervals are very busy, considering using a larger interval to reduce visual clutter.
Response: We have revised the figure as such.

Figure 2: I would really like to see panel a larger, especially as the seismicity within it is plotted so small. Consider scaling the histogram x axis to see the distribution a little more clearly
Response: We have revised the figure as such.

Figure 3: Why is the background grey? If it wasn't coloured, the sub panels wouldn't be as noticeable. Could label the basins. Consider using a colour-blind friendly color palette.
Response: We believed that the reviewer meant to say "Figure 4". We had been testing different combination of background colors, the color palette, and the subpanels. We realized that this way will best display the features of orientation and the plunge. We have added the names of the two subprovinces.

Figure 4: Not referenced in the text. Similar comments regarding background grey and the colour palette used to those in Figure 3.
Response: We believed that the reviewer meant to say "Figure 5". We realized that this way will best display the features of the faulting regime.

Figure 5: Similar comments regarding background grey and the colour palette used to those in Figure 3.
Response: We believed that the reviewer meant to say "Figure 6". We realized that this way will best display the features of the seismic-moment release.

Reviewer B:

This manuscript studies the induced seismicity of an area in Texas where there has been fluid injection and hydrocarbon extraction. The key aim of this study is to invert the stress field from the focal mechanisms of recorded earthquakes. However this manuscript falls short of this aim, primarily due to a lack of information given about the hydrocarbon/injection activities and the interpretation of the stress field. I will note that my expertise is in the broad field of earthquakes, tectonics and stress field, rather than specific expertise in induced seismicity, however my concerns about the data presented and the methodology mean that the conclusions of the paper are not supported.

Missing information about the hydrocarbon/injection activities

There is no information included about the hydrocarbon activities that are hypothesised to cause the seismicity, for example injection wells, or fluid injection volumes. How does the induced seismicity compare spatially and temporally to hydrocarbon activities? How can you tell the difference between an induced earthquake versus a "natural" tectonic earthquake?

Response: To clarify, that oil&gas activities cause seismicity in the Permian Basin is NO LONGER just a hypothesis. We believe that a number of studies have established a robust conclusion. Information regarding those oil&gas activities have been described in the papers cited by the authors (e.g., Savvaidis et al., 2020; Grigoratos et al., 2022; Woo and Ellsworth, 2023). From those studies, it has been concluded that the increased seismicity in the Permian Basin is induced by oil&gas activities. The consensus is that the stress field in the Permian basin has been constantly disturbed so the threshold of fault-slip was reduced to allow for fault reactivation (that's why earthquakes are constantly induced) and under the influence of the fluid injection (from both the wastewater injection and hydraulic fracturing). Therefore, having a discussion on the nature of the seismicity in the Permian basin (i.e., the causation factors – natural or induced) or distinguishing the natural earthquakes from induced earthquakes is not the aim of our study. Nevertheless, it is fair to say that the current seismicity in the Permian Basin is under the influence of oil&gas activities.

We also understand that not every reader is familiar with the oil&gas development in the Permian Basin of Texas. In the revised introduction we have added a brief description about the oil&gas activities in the Permian basin.

Interpretation of the inverted stress field

Additionally, I have concerns about the interpretation of the stress field**.** Injection of fluid into the sub-surface will locally change the magnitude and orientation of the stress field around the injection site (e.g. flipping sigma1 and sigma3). How can the authors be confident that the stress field they have inverted is truly representative of the tectonic stress, rather than the local stress field? Is this perhaps why there is a difference between the stress field inverted from borehole breakouts (line 54-68).

Response: From the paragraph of lines 54-68, we could not find such context referred by the reviewer. Our best guess was that the reviewer was referring to the difference between our results and the stress map of Lundstern (2024).

The stress map (i.e., *SHmax*) of the Permian Basin compiled by Lundstern (2024) was largely on the basis of borehole breakouts, and he did have used few focal mechanisms to invert for the stress field where the data were available (but not as many as what this study has). Secondly, in the original manuscript we used *S1* and *S3* to represent the stress field, whereas Lundsterns (2024) simply presented *SHmax* (Figure 7 of Lundsterns (2024)). In this revision, we have added our *SHmax* converted from the inverted 3D stress field (new Figure 5b) to provide another way to view the stress field. Form there, we do not see a fundamentally different stress field from that of Lundsterns (2024). The stress field in places where the two studies are overlapping show a high similarity.

Regarding the comments about the tectonic vs. the localized stress fields: As far as we are concerned, the stress disturbance created by the fluid injection is locally and of short-length-scale (i.e., few kilometers). Previous studies (e.g., Schoenball et al., 2014; Martinez-Garzon et al., 2013) on the induced seismicity in geothermal fields have indeed suggested that fluid injection around the site can alter the *in-situ* stress orientation and thus change the faulting pattern before and during the fluid injection. However, this change on the stress orientation DOES NOT lead to a fundamentally different faulting regime, say, changing from normal faulting to thrust faulting. Instead, from their studies, the change of faulting type is shifting between normal and strike-slip faulting. It was also shown that, deeper away from the injection well, there is no change of the faulting type before and during the fluid stimulation. This would imply that the stress field within the seismogenic volume way deeper than the bottom of the injection borehole is less disturbed, and the inverted stress field can thus be used as a proxy to represent the tectonic stress. It is also worth noting that the change of stress state is transient: after the stimulation ceased, the inverted stress field changes back toward close to the initial status.

For our 2-D stress inversion we were sampling focal mechanisms over a relatively large area. That covers the crustal seismogenic volume of every 0.3°x0.2° node and goes as deep below the basin-basement interface, and over a long duration of 7 years (since 2017). A vast majority of them are located in the top portion of the Pre-Cambrian crystalline basement where it is away from the bottom holes of the injection wells. Thus the results of the 2-D stress inversion can be largely representing the stress field associated with these basement-rooted faults that are subject the tectonic stress (i.e., the tectonic stress created/formed the Permian Basin that is inactive now). In addition, as far as we can tell, within the 7-year periods of observation (2017-present) there is no temporal variation of the focal mechanism types for each cluster. There is also no discrepancy between the current inverted focal mechanisms and historic focal mechanisms (of earthquakes that occurred before the oil&gas activities booming in the Permian Basin).

Nevertheless, because the basin is constantly under the influence of fluid injection and hydrocarbon extraction, the inverted stress model may not 100% reflect the original tectonic stress field. Given the intense oil&gas activities in our study area, it is challenging to separate the naturally occurred earthquakes from the induced ones, and further to restore the original tectonic stress field. We also understood that the wording may have created miscommunication. Therefore, to better reflect this situation, we have changed the title of this paper to “Mapping the Present-day 2-D Crustal Stress Field and Seismic Moment Release for the Greater Permian Basin of Texas as Constrained by Earthquake Source Mechanisms”.

Organisation of the manuscript

At present, the manuscript is challenging to read and to follow the key arguments being made. There are results described in the methods section, methods given in the results section, and the results and the discussion sections are amalgamated. This means that it is difficult to follow what has been done in this study versus other previous studies. There are many sentences that are grammatically poor and difficult to understand, I have highlighted some of these in the pdf. I suggest the manuscript is reorganised into a more conventional layout, to help guide the reader through the evidence and arguments being made.

Response: We have reorganized the structure of “Results and Discussion”, splitting it into “3. Results – the inverted stress model and faulting patterns” and “4. Discussion – the 2-D distribution of seismic-moment release and the tectonic implication”.

I have made other minor comments and suggestions as comments and highlights in the pdf which I hope are helpful to the authors

Zoe Mildon

Response: We appreciated the reviewer's time going through our manuscript. For us to easily respond to the comments in the same document, we have transferred the comments that reviewer made in the pdf to this response letter (see below):

Lines 61-64: "The …" This sentence is poorly worded. I suggest changing to "While the focal mechanisms present predominantely normal faulting, there is a distinct variation in hypocentral depths. To the north of the fault zone, earthquakes are deeper and located below the basin-basement interface, and to the south of the fault zone the earthquakes are shallower"
Can you give some values for how deep the earthquakes are, e.g. 5-10km, <5km?
Response: We have rewritten the introduction to better describe the seismicity, seismotectonics, and oil&gas activities in the Permian basin.

Lines 92-93: "We were based on …" This sentence does not make grammatical sense
Response: We have rephrased this sentence.

Line 99: "We were based" The same grammatical issue as above.
Response: We have rephrased this sentence.

Line 148: "Meanwhile …" These are results and should not be in the methods section
Response: Agreed. We have moved them to the section of results.

Lines 151-155: Again, this is results and should not be in the methods section
Response: Agreed. We have moved them to the section of results

Line 157: "3. Results and Discussions"... Usually Results and Discussions are two separate sections in a paper - I suggest you have two different sections
Response: Agreed. "3. Results – the inverted stress model and faulting patterns" and "4. Discussion – the 2-D distribution of seismic-moment release and the tectonic implication"

Lines 168-171: "At a glance …" These two sentences contradict each other.
Response: We have rephrased the sentence.

Lines 188-190: But your results in Figure 5 do not support this statement, as they show normal faulting throughout this area, not vertical dip-slip
Response: We agreed. We have rephrased the statement.

Line 209: more straightforward than what other approach? It is not clear why this diagram is best
Response: We are comparing with the traditional way – using the Anderson's faulting parameter (see line 198 from the original manuscript). To clarify, we do not consider our approach to be the best. We simply presented a new approach. We have rephrased the sentence to avoid miscommunication.

Line 231: Did you calculate the seismic moment for all earthquakes or only earthquakes with M>4?
Response: From the original manuscript, we calculated seismic-moment for all M≥1.5 earthquakes. Please note that we present the total seismic-moment release in a logarithmic scale. Please note that we realized that we previously misnoted the threshold. It should be "M≥1.0", and we have made a correction accordingly.

Line 237: This should be in the methods section
Response: We preferred to keep this part in this section. Please refer to the reason stated above.

Lines 256-259: Figure 6a doesn't support this statement. It is not clear if the seismic moment released has been calculated for M>4 or all earthquakes. In addition, it is unclear how the value of 82% has been calculated from some earthquake groups

Response: It occurred to us that there might be miscommunication caused by the scale of seismic-moment presented. Please note that it is a logarithmic scale of seismic-moment shown in the color pallet of Figure 6. As we clarified already, we calculated the seismic-moment for every M≥1.5 earthquake, respectively. We then summed them up to form the total seismic-moment release (here referred to as "sum1") of the Permian basin as a whole. Secondly, we summed up the seismic-moment of the earthquake groups (i.e., the "grid", if you will) that have M≥4 events included (referred to as "sum2"). Therefore, 82% is the results of "sum2 divided by sum1".

Please note that we realized that we previously misnoted the magnitude threshold. It should be "M≥1.0", and we have made a correction accordingly.

Line 268: What about the potential role of triggering (e.g. via static Coulomb stress transfer)?

Response: We agreed that it might be a plausible cause of the second M≥5 event in the Coalson sequence. However, whether it is related to stress transfer is beyond the scope of our aim for this paper.

Lines 296-298: But you have not shown any information about the extraction activities, e.g. is there a spatial or temporal correlation with the seismicity?

Response: We have revised the introduction to have a bit more info regarding the oil&gas activities. Also, more elaborated discussion between the oil&gas activities and the surface deformation has been added into the discussion section.

Figure 4: Please label the different basins in these figures to help the reader

Response: Agreed. We have revised the figure as such.

Review Round 2

Reviewer A

Thank you to the authors for reorganising the manuscript, it is much easier to read and follow.

However, I do not feel that the authors have addressed my comments about missing information about the hydrocarbon/injection activities, which may have been because I did not explain clearly. Their response focuses on clarifying that the seismicity is a result of hydrocarbon activities. They have not addressed the fact that they provide no information about the spatial distribution of hydrocarbon activities e.g. injection wells, to spatially correlated with earthquakes/mapped faults or the region of subsidence. This is really important because a large section of the discussion talks about the links between HF and SWD and seismicity, but the readers cannot make the link in this study because the spatial data of hydrocarbon activities isn't included. Please include this information in a figure, alongside the earthquake/fault locations.

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Line 398: should be "5. Conclusions"

Reviewer B

For author and editor

The authors have done a thorough job of responding to my previous comments, and reading through the revised manuscript and figures, I think that it is much improved. There remain several issues with tense and pluralization throughout the manuscript (Lines 52, 65, 103, 295 to name a few) which could be addressed during the copyediting stage.

I believe that my main concerns have been addressed appropriately and only really have a single comment for the authors to consider - I would be curious to see a figure that compares the fault traces (Figure 6a) with that of the SHmax directions (Figure 5) to see if the faults that are being reactivated are merely doing so because of their preferred orientation relative to the stress field - if this were the case, it could explain why some areas in the basin are relatively quiescent despite there being fault traces present.

I recommend that this paper be accepted for publication in Seismica.

Editor's line-specific comments (referring to the pdf with changes marked):

Line 111: Specify whether these 13 cm are uplift or subsidence (I assume subsidence from context)
Response: Yes it was subsidence.

Line 119: Typo in hypocentral
Response: Changes noted.

Line 121: Either 'has been monitoring seismicity … since 2017' or 'started to monitor … in 2017'.
Response: We meant "started to monitor seismicity for Texas in 2017". Changes noted.

Section 2.2: I think it's important to spell out some assumptions of this stress inversion - mainly that (1) slip in earthquakes is parallel to the greatest resolved shear stress on the fault plane, and (2) stress is constant in space and time within each defined group of focal mechanisms. (unless there is something in your method that relaxes these assumptions, but i didn't spot it). Ok, some of this is addressed in new text later, but I think it should be raised in the methodology (and then addressed in the results as it already is).
Response: We added the two assumptions in the first paragraph of section 2.2.

Line 189-190: What exactly does this measure uncertainty in? The inversion as reflected by the range of focal mechanisms that went into it? To what extent is the uncertainty in the focal mechanisms themselves, and the uncertainty in the velocity model, taken into account here? Do you really know the uncertainties, confidently, to within one-tenth of a degree?
Response: We used bootstrap resampling of the data set to estimate the uncertainties of *S1,* S2, and S3 from the inverted stress tensor of each respective grid point. That approach is part of the inversion codes we were using. Note that the uncertainty of the stress orientation for each grid point is not being estimated individually. Therefore, the uncertainty of the stress orientation for each respective grid point reflects the range of obtained stress orientations using the resampled dataset over the bootstrap runs. To clarify, lines 189-190 noted down the "average" uncertainty of *S1*, *S2*, and *S3*, respectively (averaged from all grid-points that have the uncertainties of *S1*, *S2*, and *S3*).

The algorithms we used for stress inversion do not take the uncertainties of focal mechanisms into account. The number of focal mechanisms we used to invert for the stress tensor of each grid point is sufficient to estimate the uncertainty down to 0.1°. In the revision we have decided to present the uncertainty down to only the 1st decimal place, instead of two-decimal places.

Line 195: 'there' should be 'the'
Response: Changes noted.

Line 197 and elsewhere: The orientation of stress axes is typically expressed as a trend (= azimuth) and plunge (=inclination) - so I don't understand the reference to 'orientation and plunge' - because the plunge is an integral part of the orientation. I assume trend and plunge is what is meant?
Response: We may use the terminology differently from yours. Our "orientation" referred in the manuscript has the same meaning of "trend" to the editor. In the revision, we have replaced "orientation" with "trend" in order to avoid confusion.

Line 199: Please avoid the word 'shallow' when referring to a gentle or low-angle plunge as 'shallow' is a depth term. Especially important in a paper that also talks about stress at different depths, where shallow will be used to describe a depth range. Also, same line, please avoid the term 'dip' for the inclination of lines, 'dip' is reserved for the inclination of planes.
Response: Agreed. We have changed to "low-angle"

Line 201: That will always be true as a vertical line, by definition, does not have an azimuth.
Response: Agreed.

Lines 219-220: I would typically think of an 'extensional stress field' as a stress field favouring normal faulting, which indeed leads to horizontal extension. Strike-slip, on the other hand, is not necessarily a cause of horizontal extension, as in ideal strike-slip faulting any horizontal extension would be balanced by shortening in another (also horizontal) direction.
Response: Our best knowledge is that strike-slip faulting can take place in either extensional or compressional environment. The strike-slip faulting alone cannot define extensional or compressional. It has to co-exists with normal or thrust faulting. Particularly, the strike-slip faulting co-exists with normal faulting in the Midland Basin. Its presence appears to accommodate the neighboring normal faulting systems. We have rephrased the wording.

Line 260: By what metric is it 'significant'? Are you able to say something quantitative about the moment? Significant relative to what? (moment in other areas, moment expected from the relatively slow strains in this continental interior, etc?)
Response: The combination of lines 60-65 and lines 254-259 has provided a comparative metric to define what "significant" means. For the same Permian Basin, sixty-four M≥4 earthquakes (seven of them are having M≥5) occurred within the last 8 years (2017-2025; lines 254-259). In comparison, lines 60-65 has stated that over the past 90 years (1931-2016), there were only three M≥5 earthquakes. We have rephrased the wording to make the comparison more clear.

Line 272; No hyphen in 'seismic-moment'
Response: We removed the hyphen.

Line 327: While this is conventional wisdom in plate boundary areas, the model by Caleis et al (GRL, 2016) for intraplate regions argues that this model of tectonic strain build-up is less applicable there.
Response: To clarify, we understand that currently the Permian Basin is not located in a present-day plate boundary and the conventional wisdom (the strain build-up model) is not applicable to the Permian Basin (see our wording in Lines 327-330 from the first revision). Through out the manuscript, we DID NOT ever imply that the conventional seismogenic model could work for our study area at present time, especially for explaining why multiple M5+ earthquakes can occur in the Permian Basin.

We appreciate the paper recommended by the editor, and we went through it. As far as we understand, Calais et al. (2016) only argue if the "strain build-up" at present time is applicable to current intra-plate earthquakes. They however did not exclude the scenario that "tectonic strain" was built up during the past tectonism and was preserved in the lithosphere as a plausible seismogenic source for current induced earthquakes, and the situation in our study area fit this scenario. Based on their theory, we hypothesize that the Permian Basin has been a "strain-reservoir" already. In addition, there is an active and long-stretching Rio Grande Rift existing right next to our study area (see the revised Figures 1 and 6) but we previously ignored its role as a plausible secondary stress source at present time. In this revision, we have added a paragraph to discuss its role in our study area.

New text line 355-378: I don't really see how the new work contributes to this section. This all seems based on existing literature, and is not surprising. Either make it clear how this is new and relevant in this discussion, or remove.
Response: We disagreed. To clarify, this paragraph was intended in response to the comments made by reviewer A from the first review, who argued that hydrocarbon extraction does involve fluid injection and can have equivalently contributed to the fault reactivation but we ignored it in our

original manuscript. Review A would like to see more elaborated discussion about the role of the fluid injection from the fracking jobs on fault reactivation, where wastewater disposal and fracking jobs co-exist. The purpose for this paragraph is to present the evidences from existing literature and to clarify that fluid injection from wastewater disposal is the most dominant factor to trigger fault reactivation in a large scale.

Line 381: More widespread deformation than what/where/when? The comparative term 'more' requires clarification.
Response: We simply meant to say "deformation in a widespread area" (i.e., spatially), and did not intend to make any comparison. We realized that the wording "more" has caused miscommunication. We have rephrased the wording.

Line 382-384: What is the evidence for this? One could argue that it is the distribution of pore pressure along the long faults that allow the larger events, in which case the pore pressure perturbation is as important as the available fault length.
Response: The evidence was given by Morris et al. (2024) and Ge et al. (2022), where pore-pressure perturbation plays a role ONLY in the reactivation of pre-existing fault. The fault architecture plays a role in the event magnitude after the fault was reactivated. Combined with the mapped fault traces and the results of pore-pressure modeling, Morris et al. (2024) has shown that higher pressure perturbation on a short-and-segmented fault would not result in larger earthquake. In other words, pore-pressure has no control on the quantity of released seismic moment whereas larger seismic-moment release are associated MAINLY with a lengthy and hard linked fault system.

We have rephrased the context to make it more clear.

Line 389-391: Speculative - can you write off a model where the crust is essentially critically stressed (from far-field tectonic stresses as much as (or more than) a stress memory from past tectonism)?
Response: We disagreed. It is a fact that tectonic faults were created during tectonic events. The creation of tectonic faults may have involved occurrence of seismic events in the past geological time, as we are NOT referring to the strain build-up at present time. Our hypothesis stated in lines 389-391 is not different from that of Calais et al. (2016) – tectonic strain built up during the past tectonism and stored in the lithosphere, and becomes a seismogenic source at present time due to the perturbed stress field (i.e., by the current oil and gas activities). The Mw5.7 event on Apr 1995 (the old Figure 5, now new Figure 7) has shown that the stored tectonic strain is already capable of releasing seismic moment at a level of Mw5.7. It is worth noting that at the time the oil and gas activities were not active around the area of the Mw5.7 earthquake.

As far as we are concerned, the Rio Grande Rift (located at immediate west of our study area) is also part of the products of the past tectonism. This rift system features Quaternary faulting, sedimentary basins, widespread past volcanic activity, and thinned lithosphere. The lithosphere's thinning process (i.e., thermal uplifting force) may have been the plausible stress source and created a strain-reservoir (Calais et al., 2016) in the past geological time. Currently, the crustal extension associated with the rift can be geodetically observed in the region immediate west of the Grisham fault zone – GFZ (Murray et al., 2019), indicative of active crustal deformation in proximity to our study area. An M5.8 earthquake occurring (in 1931) within the Rio Grande Rift further evidences that the rift system is still active at present time (see revised Figures 1 and 6). The observed normal-faulting mechanisms in our study area reflect the extended structures associated with the system of Rio Grande Rift. Therefore, we hypothesize that the top portion of the crystalline basement has been pre-stressed during the past tectonism or the far-field current tectonic stress from the Rio Grande Rift. We have revised this statement.

Reference: Murray, K. D., M. H. Murray, and A. F. Sheehan (2019). Active deformation near the Rio Grande Rift and Colorado Plateau as inferred from continuous Global Positioning System measurements, *J. Geophys. Res.*, 124, no. 2, 2166–2183, doi: 10.1029/2018JB016626.

Lines 391-396: I don't see how you separate this from the alternative hypothesis where the fault system provides an existing weakness rather than a strong asperity, and that this weakness is activated by fluid injection into critically stressed crust.
Referred to above: Calais, E., Camelbeeck, T., Stein, S., Liu, M., & Craig, T. J. (2016). A new paradigm for large earthquakes in stable continental plate interiors. Geophysical Research Letters, 43(20), 10-621.
Response: This comment appeared to be part of the previous comment, and we have made response in the above paragraph.

Reviewer A:

Thank you to the authors for reorganising the manuscript, it is much easier to read and follow.

However, I do not feel that the authors have addressed my comments about missing information about the hydrocarbon/injection activities, which may have been because I did not explain clearly. Their response focuses on clarifying that the seismicity is a result of hydrocarbon activities. They have not addressed the fact that they provide no information about the spatial distribution of hydrocarbon activities e.g. injection wells, to spatially correlated with earthquakes/mapped faults or the region of subsidence. This is really important because a large section of the discussion talks about the links between HF and SWD and seismicity, but the readers cannot make the link in this study because the spatial data of hydrocarbon activities isn't included. Please include this information in a figure, alongside the earthquake/fault locations.
Response: We have added the locations of injection and fracturing to Figure 1.

Line 92-95: The authors say that there is a distinct shift in the depths of earthquakes either side of the Grisham Fault Zone, but there is no evidence given to support this. I suggest adding a N-S cross-section with earthquake epicentres plotted to Fig 1 to demonstrate this.
Response: One of the lead author's previous papers had demonstrated this evidence (Huang et al., 2022). We should have cited this paper but we forgot. We have figured that displaying seismicity in a mapview at various depth layers can best present 3-D spatial distribution of seismicity. Therefore, in the revision, we followed a similar ways done by Huang et al. (2022). The map views of seismicity at various depth layers) is now in new Figure 2.

Line 111-112: add "subsidence" to this sentence to clarify the vertical deformation
Response: Changes noted.

Line 209: add the GFZ to Figure 4 to help back up this statement
Response: Changes noted. Figure 4 is now Figure 5.

Line 272-298: These lines describing the seismic moment calculations is methodology and should not be included in the Discussion section. This needs to be moved to the Methods section.
Response: Changes noted. They are now in section 2.3 (lines 186-215)

Line 398: should be "5. Conclusions"
Response: Changes noted.

Recommendation: Revisions Required

Reviewer B:

The authors have done a thorough job of responding to my previous comments, and reading through the revised manuscript and figures, I think that it is much improved. There remain several issues with tense and pluralization throughout the manuscript (Lines 52, 65, 103, 295 to name a few) which could be addressed during the copyediting stage.

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Response: We have revised Figure 6a (now it's now re-labeled as Figure 7a) by combining SHmax and the fault traces.

I recommend that this paper be accepted for publication in Seismica.

Recommendation: Accept Submission

Response: We appreciate the constructive comments from you.