Reviewer Comments

For author and editor

Reviewer B

Dear Authors,

I read your work with great interest and found it to be clear, engaging, and presented with excellent figures.

However, I always find it challenging to assess the true impact of studies that focus on a few events discussed in great detail within the broader context of earthquake nucleation. From your analysis, it seems to me that the events you detected are likely aftershocks of the magnitude 4 event preceding the mainshock, rather than true foreshocks.

That said, the analysis is clear, straightforward, and well-suited for publication.

Best regards,
Piero Poli

Reviewer I

Dear Editor,

“Foreshocks to the 2020 Mw 4.8 Mentone Earthquake in west Texas: Insights into earthquake nucleation and earthquake triggering” by Bolton et al. reports on a sequence of induced earthquakes leading up to a Mw4.8 in Mentone, TX with the goal of determining the nucleation process and triggering mechanism of the Mw4.8 event. The authors look at the location and timing of 11 events leading up to the Mw4.8 event. Events are clustered in space and time, and most seem to trigger one another, but not the mainshock. They also estimated stress change from each of the 11 foreshocks and conclude that only the Ml4.0 has a large enough stress change impact the Mw4.8. Based on these results they conclude that neither the preslip or cascade model fit this sequence of events and therefore additional triggering mechanisms must be present.

The manuscript is well written and logically organized. The figures present useful and relevant data, but presentation could be easily improved in some cases. However, I think there are some avenues and insights the authors can investigate that would add depth to the paper and perhaps a more through answer to the overarching question. I have included some questions and comments below which I hope will improve the paper.

Major Comments:

1. Why is this sequence a foreshock-mainshock sequence, when most induced seismicity is swarm-like (Skoumal et al., 2015, JGR)? I think a discussion of this would be incredibly relevant to the main question of how are induced earthquakes (or earthquakes in general) triggered. Both in how the question is posed and in the interpretation of results.
2. What happened in the time between the M4.0 and M4.8? This is one of the largest questions I was left with after reading and I think a discussion of this will enhance the paper significantly. Can you comment on this, even speculatively? Perhaps by linking to other literature. Or can you place bounds on the amount of stress change or slip required to load the M4.8 patch? Is this reasonable? For example, lines 381-382, propose that afterslip from the M4.0 may trigger the M4.8, but it cannot be confirmed without geodetic measurements. Are there other measurements of afterslip from similar earthquakes and how would the afterslip compare to this M4.0?
3. Please justify the use of a 6.5 hour window prior to the mainshock. Based on Figure 1B is seems like there are several larger earthquakes (>M2) that may have contributed to stress changes prior to the mainshock. A figure similar to 5a or 4k with varying time to failure windows would be interesting to see.
4. This work assumes all ruptures are constant, circular, Brune-type ruptures. Is this assumption supported by source-time functions or other methods? Particularly, can you provide a better rupture area for the mainshock?
5. The authors spend a significant amount of time discussing the stress drop calculation details for the small events, but the stress drop details are in the supplementary for the M4.0 event and the M4.8 event is not discussed (unless I missed something). Please add the details of the M4.0 and M4.8 events to the main text and discussion uncertainties since this event seems to have a larger impact on the study then the smaller events.

Minor comments:

1. It seems like several of the smaller events could be aftershocks of the M4.0 or foreshocks to the M4.8. This is acknowledged, but more discussion of the implications (or lack thereof) would be beneficial.
2. The paper would benefit from an additional definition or figure/cartoon explaining the preslip-cascade differences. This could link nicely to a discussion of what you expect to see from the 11 foreshocks if this specific M4.8 was triggered by each mechanism.
3. Please add a little more detail regarding how the slip model is converted into a stress change.
4. Lines 71-75: Can you add some additional information about induced events specifically? It may be worthwhile to compare foreshocks of induced and non-induced events. Additionally, a comparative discussion of this event with the 2011 Mw5.7 Prague, OK sequence would be interesting. Keranen et al., 2013 (Geology) and Sumy et al., 2014 (JGR) suggest the Mw5.7 event was triggered by stress changes from nearby Mw5.0 events.
5. Line 84: a space between Dieterich 1986 and 1992 is missing.
6. Line 94: While I agree that at the point of initiation, it is unknown whether or not a foreshock will grow large enough to be the mainshock, it is incorrect to say large events

occur due to “random chance”. Events reach a breakaway point if it can continue to fail more fault elements (Ellsworth and Beroza, 1995). Failure of successive fault patches depends on fault conditions (Galis et al., 2017; Rubino et al., 2022; Ke et al., 2018; Cebry et al., 2023; Lockner et al., 1982; Gvirtzman & Fineberg, 2021).

7. Lines 107-109: Might be worthwhile to mention that critical nucleation length is proportional to effective normal stress and note how that might change how we expect preslip and cascade models to look.
8. Line 123/Repeaters in general: Are they true repeaters? Can you run a cross correlation/analysis and definitively say? If they’re only neighboring fault patches slipping, each event can trigger the next one. If they are truly the same fault patch getting reloaded and slipping again, one event cannot trigger the next one and a different loading mechanism is required. This distinction would be relevant.
9. Line 127: Skoumal et al., 2021 presents an overview of what I believe is the same sequence, despite differences in magnitude. They propose a different fault plane based on focal mechanisms which agrees with the focal mechanisms from Huang et al., 2022. I did not see a 64 strike and 65 dip fault plane in either of these references, although perhaps it is in Horne et al., 2021 which was missing from the reference list. It may also be worth comparing your rupture area for the M4.8 with that in Skoumal et al., 2021.
10. Between line 138 and 154: How does the timing of this pore pressure and poroelastic stressing compare to the timing and locations shown in Figure 4 and 5?
11. Line 165: Please provide an estimate or expected range for the critical nucleation length scale.
12. Foreshock at t = -271 s: This event occurred in a region of negative shear stress (Fig 4H and 5B), could this be due to a depth uncertainty either from this event or the M4.0 event?
13. Line 339: Similarly, can you provide an estimate of how the increase in shear stress would change from moving the M4.0 and M4.8 closer together or further apart? Within the location uncertainty, that is.
14. Lines 343-345: Since the M4.0 event created a region with a 2MPa increase in shear stress which exceeded the predicted coulomb failure, do you have any thoughts as to why the M4.8 initiated where there was a 180 kPa increase instead?
15. Reference List: Both Horne et al., 2021 and 2022 are missing from the reference list, perhaps others as well.

Comments on figures:

1. Please flip the depth axis on all relevant plots.
2. Figure 2C: make mainshock a star to match with other subplots.
3. Figure 3: if ordering the events by time, use a consistent rainbow color scheme for all events, if they are not meant to be color-coded by time, find a totally different color scheme. Bonus points either way for using a color-blind friendly option.
4. Figure 3: Please be clear in the caption which foreshocks you’re showing. It was a little unclear which 7/11 and 8/11 you are referring to.
5. Figure 4: Perhaps the color bar would be better as a log scale?
6. Figure 5A: zoom in on the y-axis for clarity, it is difficult to interpret as presented.

The University of Texas at Austin

10/30/24

Prof. Ake Fagereng

Editorial Office Seismica

Re: Foreshocks, aftershocks, and static stress triggering of the 2020 M_w 4.8 Mentone Earthquake in west Texas

Dear Prof. Fagereng,

We are re-submitting our manuscript: Foreshocks, aftershocks, and static stress triggering of the 2020 M_w 4.8 Mentone Earthquake in west Texas *by David C. Bolton, Nadine Igonin, Yangkang Chen, Daniel T. Trugman, Alexandros Savvaidis, and Peter Hennings.*

We appreciate the time and effort that you and the reviewers devoted to our manuscript, and we are grateful for your comments and suggestions. All the comments raised by the reviewers have been addressed below and in the revised manuscript. The discussion has been expanded to address several key points raised by both reviewers. We now acknowledge the possibility that several of the earthquakes that preceded the Mentone mainshock are perhaps better classified as aftershocks of the initial M_L 4.0 foreshock as opposed to foreshocks of the Mentone mainshock (see Discussion section). We test this hypothesis by measuring the nearest-neighbor distances using the method presented by Zaliapin and Ben-Zion (2013) and show the linkages between pairs of earthquakes in Figure 6. This analysis demonstrates that most of the events that followed the initial M_L 4.0 are aftershocks, and the M_L 4.0 can be considered a foreshock to the M_L 4.9 mainshock. This idea reinforces our claim that the neither the preslip nor cascade model can successfully explain this sequence of 11 events that preceded the Mentone mainshock. We added additional Figures (3B-3C) to help validate the claim of repeating ruptures. In addition, Figures 5B-C now consider the location uncertainties of the mainshock an initial M_L 4.0 foreshock, which demonstrates that between 60-400 kPa of stress could have been imparted at the Mentone mainshock due to the initial 4.0 foreshock. This range far exceeds the modeled stresses and pressures associated with subsurface fluid injection and highlights the importance of earthquake-earthquake triggering in induced seismicity sequences. Our work also highlights the difficulty in explaining foreshocks under the classic preslip and cascade models and suggests that hybrid-models that invoke multiple, yet complementary, mechanisms of aseismic creep and stress transfer might be better alternatives.

Thank you for your help and please contact us if you need any additional information.

Sincerely,

David C. Bolton (for all authors)

Review comments are provided verbatim below in black Times New Roman font.
Our responses are provided in red Times New Roman font.

We include a clean version of the manuscript and one showing track changes.
Line numbers quoted here are for the clean version of the manuscript.

Reviewer B:
Dear Authors,
I read your work with great interest and found it to be clear, engaging, and presented with excellent figures.
However, I always find it challenging to assess the true impact of studies that focus on a few events discussed in great detail within the broader context of earthquake nucleation. From your analysis, it seems to me that the events you detected are likely aftershocks of the magnitude 4 event preceding the mainshock, rather than true foreshocks.
That said, the analysis is clear, straightforward, and well-suited for publication.
Best regards,
Piero Poli
Recommendation: Accept Submission

Thank you, Piero, for the comments and review. Indeed; this analysis is strictly focused on the Mentone mainshock and the events that preceded it. We agree that it is challenging to assess the generalization of our results in the broader context of earthquake nucleation and its connection, or lack thereof, to foreshock activity. We now acknowledge that some of the apparent foreshocks are likely a set of aftershocks triggered by the initial M4.0, which happened to trigger the M4.8 mainshock. We measured the nearest-neighbor distances between event pairs using the method presented in Zaliapin and Ben-Zion (2013) and show that 10 of the smaller ML 1-2 earthquakes can be considered as aftershocks of the initial M_L 4.0. Strictly speaking, neither the pre-slip or cascade model, can accurately describe this sequence. This observation dovetails nicely with your recent work, (Martinez and Poli, 2024), and suggests that the classic nucleation models are likely too simple to explain all foreshock sequences.

Reviewer I:

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Major Comments:

1. Why is this sequence a foreshock-mainshock sequence, when most induced seismicity is swarm-like (Skoumal et al., 2015, JGR)? I think a discussion of this would be incredibly relevant to the main question of how are induced earthquakes (or earthquakes in general) triggered. Both in how the question is posed and in the interpretation of results.

   Thanks for the thought-provoking question. Differentiating mainshock/aftershock sequences from swarms is an interesting question. It's important to note that our work alone cannot provide a definitive answer to this question. Nevertheless, it highlights an important research avenue that should be explored in future work. Based on laboratory experiments, we propose that the initial stress could be a key factor that differentiates swarms from classic mainshock-aftershock sequences. See section Induced Earthquakes: Fluid-induced swarms versus foreshock-mainshock-aftershock sequences for an extended discussion. L539-577

2. What happened in the time between the M4.0 and M4.8? This is one of the largest questions I was left with after reading and I think a discussion of this will enhance the paper significantly. Can you comment on this, even speculatively? Perhaps by linking to other literature. Or can you place bounds on the amount of stress change or slip required to load the M4.8 patch? Is this reasonable? For example, lines 381-382, propose that afterslip from the M4.0 may trigger the M4.8, but it cannot be confirmed without geodetic measurements. Are there other measurements of afterslip from similar earthquakes and how would the afterslip compare to this M4.0?

   Thank you for the thoughtful comment and set of questions. These are excellent points. The fact that there is a delay time of ~ 6.5 hours between the M4.0 and mainshock is likely an indicator of fault zone heterogeneity, in the form of stresses or frictional strength. The delay time between the foreshock and mainshock could also indicate that the loading rate that is driving failure has changed between the two events. As you can see in Figure 5, the static stresses from the 10 ML 1-2 foreshocks only imparted ~ 1-2 kPa of stress at the hypocenter of the mainshock. Though it's possible that these stresses led to the initiation of the mainshock, they likely played a minor role. We propose two possibilities.

In the context of nucleation models, it's possible that the delay time is caused by the intrinsic slip dependence of the nucleation phase of the mainshock (e.g., Dieterich, 1978; 1986). If this model is accurate, then it indicates that the mainshock will not rupture until a zone of accelerating aseismic creep reaches a critical length scale, of H*, (Dieterich, 1978; 1986). The zone of accelerating aseismic creep could be initiated by elevated fluid pressures and/or poroelastic stressing (e.g., Cebry et al., 2021; 2022). This model would be supportive of a preslip model for foreshock occurrence. However, the fact that foreshocks do not exhibit spatiotemporal characteristics of a preslip model does not necessarily rule out the possibility that the mainshock was preceded by an extended nucleation phase.

Another possibility is that the initial ML 4.0 foreshock triggered afterslip/aseismic creep, which in turn, loaded the fault that initiated the mainshock. If true, then perhaps the initial stress perturbation from the ML 4.0 foreshock led to a significant clock advance in rupture time of the mainshock and additional loading from post-seismic afterslip of the 4.0 supplied the additional stressing needed to initiate failure.

Both scenarios are different in terms of whether the events that preceded the Mentone mainshock should be labeled as foreshocks or aftershocks. However, both ideas point to heterogeneity as a potential cause in delay time between the two events and invoke aseismic creep as potential mechanism for driving failure of the mainshock.

This has been added to the discussion section. See lines 519-538

3. Please justify the use of a 6.5 hour window prior to the mainshock. Based on Figure 1B is seems like there are several larger earthquakes (>M2) that may have contributed to stress changes prior to the mainshock. A figure similar to 5a or 4k with varying time to failure windows would be interesting to see.

   Good point. See lines 181-212 for rationale behind the 6.5 hour time window used in the analysis.

4. This work assumes all ruptures are constant, circular, Brune-type ruptures. Is this assumption supported by source-time functions or other methods? Particularly, can you provide a better rupture area for the mainshock?

   Good point. It's important to remember that we only calculate the stress drop of the largest foreshock (i.e., ML 4.0) using EGFs. We assume that the smaller foreshocks have a 2 MPa stress drop (Figures 3-5) and we perform a sensitivity analysis on our results using stress drops between 0.5-8 MPa (see supplement). This range in stress drops was selected based on previous analyses of source parameters in west Texas (Trugman and Savvaidis, 2021).

   The source spectral models assume that the earthquake rupture behaves as a circular crack with constant stress drop and that the corner frequency of the source spectrum is inversely proportional to the source radius This is a common assumption in many modeling studies, though it may oversimplify some aspects of the earthquake source processes (Abercombie, 2021). For example, we know from finite-fault modeling that this is an oversimplification because ruptures are often chaotic, and do not always grow in a symmetric radial pattern (e.g., Jia et al., 2023). Nevertheless, without high-quality seismic and geodetic data it is difficult to constrain the precise details and geometry of the rupture process. Therefore, we follow the work of others and assume that the foreshocks ruptured a circular fault patch. Also note that we perform a sensitivity analysis with respect to stress drop uncertainties for the smaller M_L 1-2 events. See lines 392-403.

5. The authors spend a significant amount of time discussing the stress drop calculation details for the small events, but the stress drop details are in the supplementary for the M4.0 event and the M4.8 event is not discussed (unless I missed something). Please add the details of the M4.0 and M4.8 events to the main text and discussion uncertainties since this event seems to have a larger impact on the study then the smaller events.

   As noted above we estimate the stress drop of the first foreshock (i.e., M_L 4.0) using EGFs and assume that the smaller M_L 1-2 foreshocks have a 2 MPa stress drop based on our previous work (e.g., Trugman and Savvaidis, (2021)). Because we only measure the stress drop of one event in our catalog, we prefer to leave the details of the stress drop calculations in the supplement. We included the caveat about the constant stress drop/crack model assumption in the discussion. See lines 392-403.

Minor comments:

1. It seems like several of the smaller events could be aftershocks of the M4.0 or foreshocks to the M4.8. This is acknowledged, but more discussion of the implications (or lack thereof) would be beneficial.

   Good point. See lines 435-517 for a discussion on whether these events are foreshocks or aftershocks.

2. The paper would benefit from an additional definition or figure/cartoon explaining the preslip-cascade differences. This could link nicely to a discussion of what you expect to see from the 11 foreshocks if this specific M4.8 was triggered by each mechanism.

   Thanks. We prefer not to include a schematic because this is a rather more complicated foreshock/aftershock sequence that is not easily explained by the cascade or preslip models.

3. Please add a little more detail regarding how the slip model is converted into a stress change.

   Done; see lines 302-303.

4. Lines 71-75: Can you add some additional information about induced events specifically? It may be worthwhile to compare foreshocks of induced and non-induced events. Additionally, a comparative discussion of this event with the 2011 Mw5.7 Prague, OK sequence would be interesting. Keranen et al., 2013 (Geology) and Sumy et al., 2014 (JGR) suggest the Mw5.7 event was triggered by stress changes from nearby Mw5.0 events.

   Good point. We now discuss the Prague and Pawnee mainshocks in the introduction and discussion sections and highlight their similarities with the Mentone sequence. See lines 117-121; 428-434.

5. Line 84: a space between Dieterich 1986 and 1992 is missing.

   Fixed

6. Line 94: While I agree that at the point of initiation, it is unknown whether or not a foreshock will grow large enough to be the mainshock, it is incorrect to say large events occur due to "random chance". Events reach a breakaway point if it can continue to fail more fault elements (Ellsworth and Beroza, 1995). Failure of successive fault patches depends on fault conditions (Galis et al., 2017; Rubino et al., 2022; Ke et al., 2018; Cebry et al., 2023; Lockner et al., 1982; Gvirtzman & Fineberg, 2021).

   This is an insightful comment, we have revised this statement to clarify this point. See lines 94-105.

7. Lines 107-109: Might be worthwhile to mention that critical nucleation length is proportional to effective normal stress and note how that might change how we expect preslip and cascade models to look.

   Done; See lines 79-131.

8. Line 123/Repeaters in general: Are they true repeaters? Can you run a cross correlation/analysis and definitively say? If they're only neighboring fault patches slipping, each event can trigger the next one. If they are truly the same fault patch getting reloaded and slipping again, one event cannot trigger the next one and a different loading mechanism is required. This distinction would be relevant.

   Thanks. Figure 3 now includes a cross-correlation matrix derived from the foreshocks. 4/11 foreshocks have overlapping source radii and exhibit high-waveform similarity (> 0.70), supporting the idea that these events are repeaters. See lines 283-287 and Figure 6.

9. Line 127: Skoumal et al., 2021 presents an overview of what I believe is the same sequence, despite differences in magnitude. They propose a different fault plane based on focal mechanisms which agrees with the focal mechanisms from Huang et al., 2022.

I did not see a 64 strike and 65 dip fault plane in either of these references, although perhaps it is in Horne et al., 2021 which was missing from the reference list. It may also be worth comparing your rupture area for the M4.8 with that in Skoumal et al., 2021.

Yes, the fault plane solutions of the mainshock are presented in Horne et al., 2021. Indeed, the slip inversion performed by Skoumal et al (2021) shows a more heterogenous slip distribution for the mainshock than the constant/circular rupture that we assume from our stress drop measurements. This is not surprising, and we acknowledge this caveat in lines 392-402. However, the slip distribution of the mainshock does not affect the overall results nor interpretation of the foreshock sequence which is the focus of our work.

10. Between line 138 and 154: How does the timing of this pore pressure and poroelastic stressing compare to the timing and locations shown in Figure 4 and 5?

The models reported by Tung et al. (2021), Tan et al. (2024), and Smye et al. (2024) show that the CFS increased by 20-80 kPa at the hypocenter and origin time of the Mentone mainshock. The injection data is sampled monthly, so one cannot estimate changes in CFS at the temporal resolution of the sequence of 11 earthquakes in Figures 3-5.

11. Line 165: Please provide an estimate or expected range for the critical nucleation length scale.

It would be nice to place bounds on the critical nucleation length scale, H^*. However, we do not have the necessary data to confidently place bounds on the theoretical nucleation length scale. Part of the difficulty in doing this is that it's not clear how one should scale up the rate-state constants to sesimogenic depths and stresses. Another approach to putting bounds on h* would be to measure the spatial extent of the foreshock region; however, this would indicate that the foreshocks are a byproduct of a preslip nucleation model. However, our analysis shows that 11 earthquakes that preceded the mainshock cannot easily be explained using a preslip or cascade model. Due to these caveats, we prefer not place bounds on h* for this sequence of events.

12. Foreshock at t = -271 s: This event occurred in a region of negative shear stress (Fig 4H and 5B), could this be due to a depth uncertainty either from this event or the M4.0 event?

Good point; Yes, due to the depth uncertainty on the order of ~ 130 m it's possible that the foreshock at a TTF of -271 (min) could have nucleated outside the rupture area of the initial foreshock, where the stresses were increased from the ML 4.0. This is mentioned in lines 505-508.

13. Line 339: Similarly, can you provide an estimate of how the increase in shear stress would change from moving the M4.0 and M4.8 closer together or further apart? Within the location uncertainty, that is.

Thanks for the comment. Taking into account the uncertainties of the mainshock and ML 4.0 foreshock, the static stresses can range from ~ 60-400 kPa. The lower and upper bounds represent the case when the events are separated by their maximum and minimum distance permitted by their location uncertainties. See Figure 5.

14. Lines 343-345: Since the M4.0 event created a region with a 2MPa increase in shear stress which exceeded the predicted coulomb failure, do you have any thoughts as to why the M4.8 initiated where there was a 180 kPa increase instead?

    Good point. It's important to acknowledge that there is uncertainty in the stress drop measurements and event locations, which in turn, will shift the locations of the maxima and minima in the modeled stresses. Nevertheless, differences in stresses associated with the foreshock and mainshock could be a reflection of stress and strength heterogeneity along the fault. The fact that the events that followed the initial foreshock nucleated in area where the shear stress was increased by ~ 2 MPa, could indicate that these areas along the fault had a lower initial stress state relative to the location that hosted the mainshock (Figure 5B). If the location that hosted the mainshock had the same initial stress state as the smaller foreshocks then it too would have needed a higher stress perturbation to "kick" it to failure. The fact that the events that followed the initial 4.0 foreshock did not evolve into a larger mainshock is again likely due to strength heterogeneity along the fault and/or a deficiency in elastic strain energy to drive a large rupture along the fault plane (e.g., Ke et al., 2018; Cebry et al., 2024). In other words, the fault conditions were not conducive to facilitate a large rupture (e.g., McLaskey, 2019). Alternatively, it could be that the critical nucleation dimension of the mainshock was larger than the smaller foreshocks, and thus, needed more time/slip to evolve into a dynamic rupture (see introduction).

15. Reference List: Both Horne et al., 2021 and 2022 are missing from the reference list, perhaps others as well.

    Fixed.

Comments on figures:

1. Please flip the depth axis on all relevant plots.

   Done

2. Figure 2C: make mainshock a star to match with other subplots.

   Done

3. Figure 3: if ordering the events by time, use a consistent rainbow color scheme for all events, if they are not meant to be color-coded by time, find a totally different color scheme. Bonus points either way for using a color-blind friendly option.

Done

4. Figure 3: Please be clear in the caption which foreshocks you're showing. It was a little unclear which 7/11 and 8/11 you are referring to.

   Done

5. Figure 4: Perhaps the color bar would be better as a log scale?

   Thanks. We prefer to leave the color bar on a linear scale.

6. Figure 5A: zoom in on the y-axis for clarity, it is difficult to interpret as presented.

   Done; See Figure 5B. Note the main purpose of showing the full range is to demonstrate that initial 4.0 imparted most of the static stresses at the hypocenter of the mainshock; the 10 events that follow the initial 4.0 only induced between 1-2 kPa of stress at the mainshock location.

Recommendation: Revisions Required