Reviewer Comments

For author and editor

In this study, the authors analyzed the earthquake source characteristics of the Sheldon earthquake sequence in Nevada using earthquake detection, relocation, moment tensor inversion, and source spectra analysis. They have illuminated the fine-scale fault structure, temporal evolution of the sequence, and the stress drop of small earthquakes. It is comprehensive analysis given that the station coverage is not good. However, I have two major comments on this manuscript.

- There are many detailed mentioned in the text that are not shown in the figures, which makes audience hard to follow only if they work hard checking the other related references.
- Many descriptions of the methods are lack of either details or references in the manuscript, especially some choices of the parameters, which is important if other people want to apply the methods to other regions.
- Even though there are lots of results shown in the manuscript, many of them cannot support the discussion of earthquake triggering mechanisms. Moreover, there is a temporal migration of earthquake sequences that worth to be considered under fluid movement or aseismic slip. If the sequence is related to the hydrothermal processes, could you please provide more discussions on how this process can explain all of these observations? Can you also discuss more on the differences and similarities between this sequence and the Cahuilla swarm you have studied before?

More detailed comments are listed below.

Line 30-31, Line 62-64: What is the time duration of the cluster? What time period did these 26 M4 earthquakes occur? Can you provide a magnitude-time plot in the manuscript or in the supplementary material to further support your description?

Line 59-61: Could you please provide the reference for this moment tensor dataset?

Line 68: Sorry, I cannot follow this sentence well: “while unique it its detailed characteristics”. Did you mean “while its detailed characteristics”?

Line 70: It might be helpful to have a small subfigure in Figure 1 showing the map view of the whole walker lane shear zone with the study area denoted. It is challenging for audience who are not familiar with the walker lane shear zone to sense the big picture from the information you provided.

Line 72-75: Are these stations the same as what you showed in fig1? If so, could you please cite fig1 here?

Line 77: For the 6-channel station, what are the other three channels? Why does it help detection and location?

Line 85-105: Where are HRCC, McDermitt Caldera complex, Sheldon sequence (and Sheldon wildlife refuge), Warner range, Surprise Valley, 1968 Adel Oregon earthquake sequence (and the community of Adel)? Could you please denote these places on the map in figure 1? It would be easier for audiences to understand the geological background.

Fig1 caption: It seems like the summation of moment tensors is only described in the caption. Would you like to also discuss this in the main text?

Line 120-125: Any references for the Antelope Datascope and the ComCat? And how to access the NSL database?

Line 134-135: Where and how did you download these waveform records from distant stations?

Line 142: What is the used 1D velocity model?

Line 150: Any references for the relocation algorithm?

Section 2.4, Line 196-199: In this Earthquake detection step, you only use one station for detection and EQtransformer may detect events that are distant from the station. Even though you did associate P and S wave based on a reasonable differential time, it is still possible that EQtransformer may detect a P-wave arrival for an event several hundred kilometers away from the station with a S-wave arrival for a nearby event several seconds later. Moreover, even if the detected event is close to the COLR station, it is also possible that the event is not located around the sequence but is located somewhere else. I think it worth mentioning somewhere in the paper to make the audience notice these issues.

Line 205-206: This assumption may not valid, either. I think some uncertainty tests should be done to validate this assumption.

Line 235-236: It seems like you also used S-wave coda right? It worth mentioning that using relatively long time window can suppress the directivity effect.

Line 243-244: How did you correct these effects? Aki and Richards (2002) is a book instead of a paper focusing on the path and site effects correction. It might be clearer by citing a specific paper or add more details in either the manuscript or the supplementary materials.

Line 253: How did you choose this frequency band? It seems like it is a very narrow and low frequency band, which is very easy to obtain high correlation value. 0.75 is not very high value considering closely located event pairs at this frequency range.

Line 269: How do you know that the results are not sensitive to the choice? Any references or sensitivity tests?

Line 293-295: It might be better to denote the time period on figure 3 and cite figure 3 here.

Line 367-369: It seems like you have figure 3 plotted before describing the moment magnitude result. I am not sure whether it is a proper way to do so. Maybe you can consider moving figure 2c to figure 6.

Line 381: Cite figure 8a here？

Line 384: Could you please highlight the magnitude of completeness (before and after detection) on figure 7c?

Line 413-440: You have mentioned a lot of places along the walker lane shear zone in this paragraph. Is it possible to have an overview figure showing the walker lane shear zone, the relative location of the Sheldon sequence along the walker lane, the long valley fault system, and/or other key features? It is difficult to have a sense of the importance of this sequence without the context of walker lane shear zone, which is not provided in this manuscript and requires the audience to check other references to understand. Since you spend a whole paragraph discussing the implication of this sequence to the walker lane shear zone, it might worth a figure highlighting all of these mentioned key features.

Line 445-451: It seems like you have ruled out many possible triggering mechanisms. However, the spatiotemporal variation of the sequence show that it gradually migrated from the NW shallower area to the SE deeper area. Is it possible to be caused by fluid migration? Like what the Cahuilla swarm (Ross et al., 2020)? Since they two share similar spatiotemporal patterns and relatively long durations, it would be great to discuss the similarities and the differences.

Ross, Z. E., Cochran, E. S., Trugman, D. T., & Smith, J. D. (2020). 3D fault architecture controls the dynamism of earthquake swarms. *Science*, *368*(6497), 1357-1361.

Line 478-480: If it is related to the fluid from the hydrothermal activity under a volcanic geologic context, what I can imagine is a bottom-up process with upward seismicity migration. Downward migration might be related to snow melt and some other top-down processes. Do you have any explanation on the downward migration of seismicity and how it is related to the fluid movement processes? What is the source of the fluid?

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For author and editor

This paper describes an impressively comprehensive seismic analysis of the 2014-2016 Sheldon, Nevada earthquake sequence. Although this sequence exhibited high rates of relatively large-magnitude seismicity over an extended time period, it occurred in a very remote area with a sparse seismic network (but fortunately one temporarily installed nearby station), adding an extra challenge to the usual analyses. The combination of multiple careful analyses, including earthquake detection, precise relocation, moment tensor analysis, stress inversion, and spectral analysis (including stress drops) provides a surprisingly detailed view of this sequence despite its sparse monitoring. Overall, this is an excellent and impressive paper – it's clearly written and illustrated and could be published essentially in its current form. I have only one substantive comment (still minor) plus a few minor comments and suggestions as outlined below. Nice work!

Main comment:

The authors use the b-positive algorithm to determine a b-value of 0.74, which they note is somewhat lower than the canonical value of 1.0. However, from Figure 7c it looks like this analysis is based on the ML magnitudes. Based on the conversion relationship presented in this manuscript, the b-value would be much higher (approximately 1.5x) if the analysis were repeated using Mw magnitudes (b-value will scale directly with factor multiplying the magnitude in the Mw vs ML relationship). In other words, the Mw-based b-value would be >1.0. Although reasonable people can argue about which b-value is most relevant, I think it’s an important point to note here, and it implies that the b-value may not in fact be “low” for this sequence (most analyses that derive a b-value of ~1.0 are looking at larger magnitudes where the scaling between ML and Mw is more similar).

Minor comments:

Line 36: “hundreds” seems to be 93 moment tensors that were actually used for the stress inversion? Please clarify.

Line 101: There may be potential for confusion between this “Long Valley” and the more famous one in eastern California. Noting the location of this Long Valley on the map in Figure 1 would help.

Lines 97-102: This is a monster sentence. Consider breaking this up for improved readability.

Figure 2, parts a) and b): The background shaded relief plot here is dark enough that it's hard to see the red labels and black dots in top of it. Consider lightening the background or altering the color scheme on top (similarly with Figure 3a).

Figure 3 caption (and I think also the text) mention the existence of an Mw/ML relationship, but it's not shown until Figure 6 and line 360 in the text. Referencing Fig 6 in the Fig 3 caption would be helpful.

Figure 4: If it's possible to make part (a) larger, it would be easier to see the moment tensors and their variations.

Line 360: How exactly was this relationship determined? Was it a fit, or was the slope of 0.67 chosen based on theoretical grounds and then verified?

Lines 394-397: No revision necessary, but from my biased perspective, it's interesting to see a similar progression in b-value to that seen in the 2014 Long Valley Caldera swarm (Shelly et al., 2016, https://doi.org/10.1002/2015JB012719). Based on the patterns of hypocenters, we interpreted that might reflect a transition in fluid confinement (initially confined along existing 2D faults, later unconfined and propating with a 3D volume) that changed the "sampling" of fault structures. A similar interpretation may or may not be relevant here, in addition to the possible relaxation of differential stress already mentioned on lines 462-463.

Movie S2 caption (on Zenodo page): It would be helpful to explain more clearly what the view is here. It seems to be a cross-section perpendicular to the main fault plane?

Response to Reviewers

Reviewer B:

In this study, the authors analyzed the earthquake source characteristics of the Sheldon earthquake sequence in Nevada using earthquake detection, relocation, moment tensor inversion, and source spectra analysis. They have illuminated the fine-scale fault structure, temporal evolution of the sequence, and the stress drop of small earthquakes. It is comprehensive analysis given that the station coverage is not good. However, I have two major comments on this manuscript.

- There are many detailed mentioned in the text that are not shown in the figures, which makes audience hard to follow only if they work hard checking the other related references.
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- Even though there are lots of results shown in the manuscript, many of them cannot support the discussion of earthquake triggering mechanisms. Moreover, there is a temporal migration of earthquake sequences that worth to be considered under fluid movement or aseismic slip. If the sequence is related to the hydrothermal processes, could you please provide more discussions on how this process can explain all of these observations? Can you also discuss more on the differences and similarities between this sequence and the Cahuilla swarm you have studied before?

More detailed comments are listed below.

Thank you for taking the time to read and thoroughly review our manuscript. Based on your comments, we have focused our revision on (i) clarifying the technical details and references throughout the text and (ii) improving the discussion of potential triggering mechanisms, including the relation to the Cahuilla swarm. We believe these actions have greatly improved the manuscript and thank you for the thoughtful suggestions. We provide further details on the revisions in our response to your detailed comments below.

Line 30-31, Line 62-64: What is the time duration of the cluster? What time period did these 26 M4 earthquakes occur? Can you provide a magnitude-time plot in the manuscript or in the supplementary material to further support your description?

We provide a magnitude-time plot in Figure 3 of the revised manuscript. The M4 earthquakes are not from a single cluster, but rather distributed in time from November 2014 through January 2016. We have revised the abstract to clarify this period of greatest activity.

Line 59-61: Could you please provide the reference for this moment tensor dataset?

All moment tensors produced by NSL staff (in this case, by co-authors Smith and Ruhl) are listed at http://www.seismo.unr.edu/Earthquake and also submitted to ComCat. We reference both of these repositories in the Data and code availability section of the revised manuscript, and note that the moment tensor database is available as a supplementary dataset archived at Zenodo (https://doi.org/10.5281/zenodo.8030954.).

Line 68: Sorry, I cannot follow this sentence well: “while unique it its detailed characteristics”. Did you mean “while its detailed characteristics”?

Thank you for pointing out the potential for confusing language. We have revised this sentence.

Line 70: It might be helpful to have a small subfigure in Figure 1 showing the map view of the whole walker lane shear zone with the study area denoted. It is challenging for audience who are not familiar with the walker lane shear zone to sense the big picture from the information you provided.

Yes, this is a great suggestion. We have revised Figure 1 based on this suggestion and your other comments to annotate the Walker Lane, Adel sequence, and key faults.

Line 72-75: Are these stations the same as what you showed in fig1? If so, could you please cite fig1 here?

Yes, Figure 1 shows stations used in the location analysis. We have revised the text to cite Figure 1 and adjusted the Figure 1 caption.

Line 77: For the 6-channel station, what are the other three channels? Why does it help detection and location?

Station COLR has three-component broadband [HH] and strong-motion [HN] sensors, six channels total. This is particularly helpful for stations near the source region, which may clip for large earthquake. We have clarified this point in the revised manuscript.

Line 85-105: Where are HRCC, McDermitt Caldera complex, Sheldon sequence (and Sheldon wildlife refuge), Warner range, Surprise Valley, 1968 Adel Oregon earthquake sequence (and the community of Adel)? Could you please denote these places on the map in figure 1? It would be easier for audiences to understand the geological background.

Sure. We've modified Figure 1 to annotate key landmarks to understand the Sheldon sequence.

Fig1 caption: It seems like the summation of moment tensors is only described in the caption. Would you like to also discuss this in the main text?

We describe the moment tensors in detail in later sections. The Kostrov summation is simply a good wave of visualizing the composite moment tensor and hence is best described in the caption.

Line 120-125: Any references for the Antelope Datascope and the ComCat? And how to access the NSL database?

Antelope and ComCat references are listed in the Data and code availability section. As described in the revised text, all NSL events, phase arrivals, and waveforms relevant to this sequence are publicly available through ComCat or IRIS.

Line 134-135: Where and how did you download these waveform records from distant stations?

The current version of the NSL operational moment tensor code downloads waveforms through ObsPy, though older versions of the code active during 2014-2018 used SOD. We now clarify this point via citations in revised text and note this in the Data and code availability sections.

Line 142: What is the used 1D velocity model?

The velocity model we use is based on recent relocation work along the California-Nevada border [Ruhl et al., 2016; Trugman et al., 2023]. Because it may be of use/interest to readers, we have now uploaded the model in text form to the Zenodo repository and indicate this in the revised text.

Line 150: Any references for the relocation algorithm?

The relocation algorithm is standard for GrowClust3D.jl. This was cited a few sentences below, but we agree that it may be clearer to reference the algorithm up front. We have revised the text accordingly.

Section 2.4, Line 196-199: In this Earthquake detection step, you only use one station for detection and EQtransformer may detect events that are distant from the station. Even though you did associate P and S wave based on a reasonable differential time, it is still possible that EQtransformer may detect a P-wave arrival for an event several hundred kilometers away from the station with a S-wave arrival for a nearby event several seconds later. Moreover, even if the detected event is close to the COLR station, it is also possible that the event is not located around the sequence but is located somewhere else. I think it worth mentioning somewhere in the paper to make the audience notice these issues.

Thanks for the suggestion; it was an omission on our part to not better discuss the key assumptions of this analysis. For COLR, the Sheldon event rates are so high that they dominate the detected events, so even though EQT may pick up more distant events, this is rare enough to be contribute negligibly to the overall event statistics. Distant events observed at COLR would usually be sufficiently large that they would be visible on other stations, and hence processed by human analysts. The short S-minus-P times of the detected earthquakes at COLR confirm that they are indeed local events.

This is an important point to discuss in the manuscript, and we have revised the text accordingly [lines 198-204].

Line 205-206: This assumption may not valid, either. I think some uncertainty tests should be done to validate this assumption.

In our revision, this point has been clarified along with the previous one on the assumption related to COLR processing. In terms of magnitude calculations, the difference in the distance correction for the closest and most distant Sheldon event from COLR is ~0.1 M units, which is small compared to the typical inter-station variability in magnitude estimates and thus doesn't contribute much to the uncertainty in our analysis. The main uncertainty here is using a single station to measure the magnitude, not the precise position of the station. We have revised the manuscript to clarify this point [lines 218-220].

Line 235-236: It seems like you also used S-wave coda right? It worth mentioning that using relatively long time window can suppress the directivity effect.

Great point. The time windows are long enough that they may include coda, which does have that feature [which is desirable for this purpose]. We have revised the text to clarify this point.

Line 243-244: How did you correct these effects? Aki and Richards (2002) is a book instead of a paper focusing on the path and site effects correction. It might be clearer by citing a specific paper or add more details in either the manuscript or the supplementary materials.

Good point; we have revised this section to include the formula used for spectral moment calculations (equation 2).

Line 253: How did you choose this frequency band? It seems like it is a very narrow and low frequency band, which is very easy to obtain high correlation value. 0.75 is not very high value considering closely located event pairs at this frequency range.

The frequency band was selected such that it was (i) above the regime where low-frequency noise dominates the signal at COLR [~0.5Hz based on waveform inspection] and below the expected corner frequency of typical target events. This choice is guided by previous work including Abercrombie [2015], Abercrombie et al. [2017] and Ruhl et al. [2017] which discuss the EGF selection process in some detail. We would not expect the target event to cross-correlate well with EGFs at frequencies higher than the corner frequency. Note that the objective here is not to select events with identical waveforms, rather to select ones with similar enough locations and mechanisms that the EGF assumption is valid within the frequency band of interest. We have revised this sentence to clarify these points, with additional referencing [lines 267-273].

Line 269: How do you know that the results are not sensitive to the choice? Any references or sensitivity tests?

The sensitivity of the prior distribution in spectral inversions is discussed in some detail Trugman [2022]; as long as the prior encompasses all realistic possibilities (true in this case because normal distributions are unbounded), the exact characteristics of the prior only affects the inversion in data-limited regimes. For these inversions, each target event has hundreds of viable EGFs (see Figure 5a), so the data dominates the prior when performing inference. We clarify this point and the reference in the revised text [lines 289-295].

Line 293-295: It might be better to denote the time period on figure 3 and cite figure 3 here.

Thanks for the suggestion. We have annotated Figure 3 to mark this time period and revised the text accordingly.

Line 367-369: It seems like you have figure 3 plotted before describing the moment magnitude result. I am not sure whether it is a proper way to do so. Maybe you can consider moving figure 2c to figure 6.

Thanks for spotting the anachronism. We've moved the moment release plot to Figure 6c as suggested.

Line 381: Cite figure 8a here？

Done, thanks.

Line 384: Could you please highlight the magnitude of completeness (before and after detection) on figure 7c?

Good suggestion. We have annotated Figure 7 accordingly.

Line 413-440: You have mentioned a lot of places along the walker lane shear zone in this paragraph. Is it possible to have an overview figure showing the walker lane shear zone, the relative location of the Sheldon sequence along the walker lane, the long valley fault system, and/or other key features? It is difficult to have a sense of the importance of this sequence without the context of walker lane shear zone, which is not provided in this manuscript and requires the audience to check other references to understand. Since you spend a whole paragraph discussing the implication of this sequence to the walker lane shear zone, it might worth a figure highlighting all of these mentioned key features.

Yes, this is an excellent point. We have updated Figure 1 [along with your other suggestions above] to highlight the Walker Lane.

Line 445-451: It seems like you have ruled out many possible triggering mechanisms. However, the spatiotemporal variation of the sequence show that it gradually migrated from the NW shallower area to the SE deeper area. Is it possible to be caused by fluid migration? Like what the Cahuilla swarm (Ross et al., 2020)? Since they two share similar spatiotemporal patterns and relatively long durations, it would be great to discuss the similarities and the differences.

This is a nice suggestion. We did not intend to imply that we had ruled out all of these mechanisms, rather that what is observed in the Sheldon sequence is complex enough that it is not easy to devise a simple story to explain what happens. Rather, it is likely that a combination of several different drivers, perhaps active at different times, may be responsible. The connection to the Cahuilla swarm is apt and worth describing; it is actually a contrasting case where the initial migration is beautifully simple (spreading out from a point) before complexity emerges after the mainshock. We have expanded our discussion to include these points, and also discuss the down-dip migration [see point below]. See lines 479-486 in the revised manuscript.

Line 478-480: If it is related to the fluid from the hydrothermal activity under a volcanic geologic context, what I can imagine is a bottom-up process with upward seismicity migration. Downward migration might be related to snow melt and some other top-down processes. Do you have any explanation on the downward migration of seismicity and how it is related to the fluid movement processes? What is the source of the fluid?

Interesting point. The apparent down-dip migration later in the sequence is intriguing but is not easy to explain. Snow melt is a nice idea but perhaps unlikely, given the depth of these events [6-10km below surface]. Rather than speculate to much along these lines, our revised text highlights the issue as something that remains unexplained but reflects well the complexity of the sequence.

Recommendation: Revisions Required

Reviewer F:

This paper describes an impressively comprehensive seismic analysis of the 2014-2016 Sheldon, Nevada earthquake sequence. Although this sequence exhibited high rates of relatively large-magnitude seismicity over an extended time period, it occurred in a very remote area with a sparse seismic network (but fortunately one temporarily installed nearby station), adding an extra challenge to the usual analyses. The combination of multiple careful analyses, including earthquake detection, precise relocation, moment tensor analysis, stress inversion, and spectral analysis (including stress drops) provides a surprisingly detailed view of this sequence despite its sparse monitoring. Overall, this is an excellent and impressive paper – it's clearly written and illustrated and could be published essentially in its current form. I have only one substantive comment (still minor) plus a few minor comments and suggestions as outlined below. Nice work!

Thank you for taking the time to carefully review our manuscript, and for your kind words about it! We really appreciate the constructive comments and suggestions and have revised our manuscript accordingly.

Main comment:

The authors use the b-positive algorithm to determine a b-value of 0.74, which they note is somewhat lower than the canonical value of 1.0. However, from Figure 7c it looks like this analysis is based on the ML magnitudes. Based on the conversion relationship presented in this manuscript, the b-value would be much higher (approximately 1.5x) if the analysis were repeated using Mw magnitudes (b-value will scale directly with factor multiplying the magnitude in the Mw vs ML relationship). In other words, the Mw-based b-value would be >1.0. Although reasonable people can argue about which b-value is most relevant, I think it's an important point to note here, and it implies that the b-value may not in fact be "low" for this sequence (most analyses that derive a b-value of ~1.0 are looking at larger magnitudes where the scaling between ML and Mw is more similar).

This is really well said. The b-value in question is for ML, and if we switch/convert to Mw, the b-value would indeed increase to ~ 1.1 (a bit more complicated of course than a simple rescaling because the slope of 2/3 only pertains to events below M3.5). We have revised the manuscript to clarify this point and how the b-value would change if we used Mw, assuming we can extrapolate that line to very small events. [Lines 415-419].

Minor comments:

Line 36: "hundreds" seems to be 93 moment tensors that were actually used for the stress inversion? Please clarify.

Yes, this is a bit confusing. More than 100 moment tensors were computed for this sequence, but we use a quality-controlled subset of 93 for the stress inversion. For clarity, we have revised the abstract to use the smaller number.

Line 101: There may be potential for confusion between this "Long Valley" and the more famous one in eastern California. Noting the location of this Long Valley on the map in Figure 1 would help.

Good suggestion. We've added several new annotations to Figure 1 based on this comment and several others from another reviewer and have clarified the text to indicate that the Long Valley in question is in NW Nevada.

Lines 97-102: This is a monster sentence. Consider breaking this up for improved readability.

Definitely, this was a confusing sentence! We have broken it into two as suggested in the revised text.

Figure 2, parts a) and b): The background shaded relief plot here is dark enough that it's hard to see the red labels and black dots in top of it. Consider lightening the background or altering the color scheme on top (similarly with Figure 3a).

Thanks for pointing this out. We think the topography is important to the story as the seismicity follows some of the obvious topographic features. We've adjusted the contrast and color scheme to make things more readable.

Figure 3 caption (and I think also the text) mention the existence of an Mw/ML relationship, but it's not shown until Figure 6 and line 360 in the text. Referencing Fig 6 in the Fig 3 caption would be helpful.

Yes, good point. We decided to move the moment release plot to Fig 6 so that it isn't discussed out of turn.

Figure 4: If it's possible to make part (a) larger, it would be easier to see the moment tensors and their variations.

Good suggestion. We've revised the figure to highlight the moment tensors more clearly.

Line 360: How exactly was this relationship determined? Was it a fit, or was the slope of 0.67 chosen based on theoretical grounds and then verified?

Thanks for point out this omission; the slope of 0.67 is a direct fit to the data. We have revised the text to clarify that this is a data fit and not an assumption.

Lines 394-397: No revision necessary, but from my biased perspective, it's interesting to see a similar progression in b-value to that seen in the 2014 Long Valley Caldera swarm (Shelly et al., 2016, **https://doi.org/10.1002/2015JB012719).** Based on the patterns of hypocenters, we interpreted that might reflect a transition in fluid confinement (initially confined along existing 2D faults, later unconfined and propagating with a 3D volume) that changed the "sampling" of fault structures. A similar interpretation may or may not be relevant here, in addition to the possible relaxation of differential stress already mentioned on lines 462-463.

This is a great connection to make, thanks for pointing it out! We have revised the Discussion immediately following the differential stress relaxation hypothesis [lines 503-509]. to include the potential relation of the Long Valley Caldera swarm to our observations.

Movie S2 caption (on Zenodo page): It would be helpful to explain more clearly what the view is here. It seems to be a cross-section perpendicular to the main fault plane?

Thanks for point this out, both movie captions needed better description. Now fixed.

Recommendation: Revisions Required