Review Reports

Reviewer A Comments

For author and editor

The current manuscript analyses earthquake catalog and waveform data in the northern Chile subduction margin and brings evidence for remote triggering on the plate interface and within the forearc region. The manuscript is very well-written and the reported results interesting and of relevance for understanding the remote triggering of earthquakes. I have some minor to moderante concerns that I would like to ask the authors to address in a revised version of the manuscript.

1) The authors mention (in title as well) about the triggering on the plate interface. Are you able to discriminate between the earthquakes that occcur at the plate interface and those within the subducting and ovelying plates? If not, might be safer to name them subduction zone earthquakes and add a brief discussion. Regarding the catalog data, some more details on the location errors (in particular depth) might be of interest (and also related to the discrimination I mentioned above).

2) Could some of the triggered signals represent non-volcanic tremor rather than earthquakes? Did you detect triggered tremor as well, in particular when using waveform data? Please discuss.

3) At the detection of local earthquakes using waveform data, the authors mention "move-out signatures" (line 355). Could you please be more precise? Did you use Ts - Tp times or particle motion for discrimination?

4) Introduction and Discussion would benefit from including references to studies done on remote triggering in Japan. For example, Miyazawa (2011) and Enescu et al. (2016) document the immediate triggering of earthquakes, after the 2011 M9.0 Tohoku-Oki an 2016 M7.3 Kumamoto earthquakes, respectively, at active faults and volcanoes/geothermal areas throughout Japan. Opris et al. (2018) document the short-term decay characteristics of the triggered earthquakes and Takeda et al. (2024) investigate the dynamic triggering in a systematic analysis for north-east Japan, before and after the 2011 M9.0 Tohoku-Oki earthquake. I also consider that both the plate interface and volcanic/geothermal environments are characterized by an abundance of fluids, so I would expect the excitation of fluids to have a role in the triggering process.

5) Minor mistype: at line 51, please replace "?" with the intended reference. At the end of caption of Figure 3, please replace "??" with the intended figure number.

Bogdan Enescu, 2024/07/19

References:

Enescu, B., Shimojo, K., Opris, A. and Yagi, Y., 2016. Remote triggering of seismicity at Japanese volcanoes following the 2016 M7.3 Kumamoto earthquake. *Earth Planets Space,* **68**, 165 (2016). https://doi.org/10.1186/s40623-016-0539-5.

Opris, A., Enescu, B., Yagi, Y., and Zhuang, J., 2018. Triggering and decay characteristics of dynamically activated seismicity in Southwest Japan, *Geophys. J. Int.*, **212**(2), 1010–1021, https://doi.org/10.1093/gji/ggx456.

Miyazawa, M., 2011. Propagation of an earthquake triggering front from the 2011 Tohoku-Oki earthquake, *Geophys. Res. Lett.*, **38**, L23307, https://doi.org/10.1029/2011GL049795. Takeda, Y., Enescu, B., Miyazawa, M., and An, L., 2024. Dynamic Triggering of Earthquakes in Northeast Japan before and after the 2011 M 9.0 Tohoku-Oki Earthquake, *Bull. Seismol. Soc. Am.*, https://doi.org/10.1785/0120230051, 2024.

Reviewer B Comments

For author and editor

Harrington et al. studied the dynamic triggering in the northern Chilean subduction zone by analysing seismicity change during 29 main events. They found a significant increase in the seismicity in low locking and aseismic regions. They do not see any significant threshold for triggering in the region, and many parameters or non-linearity may cause triggering; the following are my major and minor comments.

Major Comments:

The authors analysed the earthquake catalogue of a longer duration of 28 days (14 days before and 14 after the main event), whereas waveform data of only 2 hours durations (one hour before and after). The authors themselves agreed that finding the trigger in the catalogue is impossible due to the high magnitude of completeness. I also agree that finding the trigger in the catalogue is sometimes impossible. The authors should analyse the waveform data for longer to establish delay triggering in the region. Automatic detection and manual identification of the local events may be carried out to establish triggering in the region.

Authors confirm earlier published work (Victor et al., 2018), and I do not see an addition to the earlier study. If so, it may be explained in the text.

The figure quality is not very excellent, and it may be improved.

Please show an example of a delay triggering up to 14 days.

Minor Comments:

In Figure 1(b), the periods of networks 8F and 8G differ in insert and caption. Is it CX or CK network?

I do not see any reason for showing decay lines for three earthquakes (M7, M8, M9) shown in Figure 2.

Figure 3: Please add the explanation for different colours in the Histogram, and in the caption, please write the Figure number in the place of ??.

Figure 5: Please write the unit of time and the same issue with Figure 6. Please also increase the size of the figures.

References: Please write the doi of all the references.

Line 18: Please add a space between "twofold".

Line 51: I do not understand (e.g. ?)

Please provide a reference for common triggering near geothermal regions.

Line 126: The authors reported M_c of 2.7 estimated by Hainzl et al. (2019). It would be better to show your estimation and its variation with time.

Line 161: Please provide the original reference at the place of Li et al. (2023).

Line 162: "s" should be in subscript.

Line 471-472: In 1985-1991.... . Please show in a figure with your estimations.

In the caption of Figure 6, correct the punctuation after "(lower six plots)".

The following references may be added.

Saini, T., Bansal, A.R., Rao, N.P. *et al.* Tiny stresses are capable of triggering earthquakes and tremors in Arunachal Himalaya. *Sci Rep* **13**, 22223 (2023). https://doi.org/10.1038/s41598-023-49068-3

Bansal, A.R. & Ghods, A. 2021, Remote triggering in Iran: Large peak dynamic stress is not the main driver of triggering, Geophysical Journal International, 225, 456-476, https://doi.org/10.1093/gji/ggaa573.

Dixit, M., Bansal, A.R., Kumar, M. R., Kumar, S., Teotia, S. S., 2023, The sensitivity of the intraplate Kachchh Rift Basin, NW India to the direction of incoming seismic waves of teleseismic earthquakes, *Geophysical Journal International*, 232, 17–36, https://doi.org/10.1093/gji/ggac289

13 August 2024

Dear Editor Andrea Llenos,

Thank you very much for your help with our manuscript. Our responses to these reviews are below in ***BLUE TEXT.*** We appreciate the time you and the reviewers have given to our work and the suggestions on how to improve it. Accordingly, we have updated our acknowledgments section to thank you and the two reviewers.

We have uploaded the three required documents for your approval:

- 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.

Please note that the line numbers cited in our responses refer to the cleaned version of the manuscript. Thank you again, and let me know if you require any additional information.

Kind regards,

Rebecca Harrington

rebecca.harrington@rub.de

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Response to Editors’ comments

> ***Thank you for your help with our manuscript. As you suggested, we have updated the acknowledgment section of our manuscript to thank you and the two reviewers.***

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Response to Reviewer A’s comments:

The current manuscript analyses earthquake catalog and waveform data in the northern Chile subduction margin and brings evidence for remote triggering on the plate interface and within the forearc region. The manuscript is very well-written, and the reported results interesting and of relevance for understanding the remote triggering of earthquakes. I have some minor to moderate concerns that I would like to ask the authors to address in a revised version of the manuscript.

> ***We thank the reviewer for these kind words.***

1) The authors mention (in title as well) about the triggering on the plate interface. Are you able to discriminate between the earthquakes that occcur at the plate interface and those within the subducting and ovelying plates? If not, might be safer to name them subduction zone earthquakes and add a brief discussion. Regarding the catalog data, some more details on the location errors (in particular depth) might be of interest (and also related to the discrimination I mentioned above).

Thanks for pointing out the ambiguity in the text here. Our primary focus is between the subduction faults and the overriding forearc faults in the Atacama fault system, as the latter has already demonstrated a propensity for triggering at shallow depths, as documented in Victor et al. (2018). Interestingly, where seismicity rates change after a stressing event (i.e., the passage of seismic waves of a candidate triggering mainshock), the 3D plots of cataloged earthquakes suggest seismicity-rate increases occur both on the plate interface and within the overriding plate in the forearc. We do not try to distinguish plate-interface from intraplate events in the downgoing plate; instead, we focus on the interaction between the subduction system faults and the forearc. We phrased the two types of earthquakes as "forearc" and "subduction system" earthquakes on l. 105 of the intro, primarily because the latter may include events on the plate interface or on the nearby system of secondary faults (including those within the downgoing plate or the forearc). We intentionally chose the word "system" rather than "zone" as it is meant to include all faults within the system that may not be directly related to the plate-interface fault.

To clarify our focus, we edited the text in several places.

- ***We changed the title to read "...near the plate interface…" instead of "...on the plate interface…", as well as in the abstract.***
- ***We have edited the sentences in the Introduction on l. 108-111 to more clearly explain the focus on the full-depth range included in the Sippl et al. catalog.***
- ***On l. 424-4, we added a sentence noting that we don't distinguish intraplate events in the downgoing plate.***
- ***On l. 428-430, we have added a sentence to note how the 3D earthquake distribution could indicate changes in frictional properties on both forearc and subduction-system faults. We also make an explicit reference to Fig. S4, which shows the 3D distribution. Additionally, we edited the first sentence in the last paragraph of the Conclusions to be consistent with the above changes (starting on l. 519).***
- ***We have edited the caption of Fig. S4 to more clearly point out that the seismicity-rate changes following stressing events occur both within the overriding plate in the forearc and on the plate interface. (See sentence starting with: "Notably, the 3D seismicity distribution ...").***

2) Could some of the triggered signals represent non-volcanic tremor rather than earthquakes? Did you detect triggered tremor as well, in particular when using waveform data? Please discuss.

The reviewer points out an interesting question, but based on our regional study, we don't uncover any compelling evidence of tectonic tremor activity within the study area. The Sippl et al. (2023) catalog that we use for the statistical analysis contains primarily earthquakes, as the aim of their work was to study earthquake statistical properties (see also the Hainzl et al. (2019) reference in the manuscript), velocity structure, and earthquake source properties. The magnitude of completeness of ~2.7 also implicitly suggests that if any low-amplitude tremor signals were present, they would likely be missing from the catalog.

Our waveform analysis is based on high-frequency energy above 5 Hz that allows us to remove most of the mainshock surface wave energy. Tectonic tremor is commonly centered around the 2-8 Hz band, so if it is present, we would expect to remove some (likely significant) portion of its energy. We don't find any obvious signs of LFE or tremor energy within our filtered waveforms. That being said, we also inspected waveforms and moveout properties of the additional events we detected. The moveout speeds we find, 8.2 and 4.8 km/s for the p- and s-waves, respectively, are consistent with seismic wave speeds for this region (Kaila et al., 1999). The qualitative appearance of these uncataloged earthquakes resembles impulsive, high-energy arrivals consistent with "garden variety" earthquakes. We are also not aware of any other studies that have documented tremor observations in our study area. For example, recent studies by Chao et al., 2019 and Saez et al., 2019 observe triggering in southern Chile near the triple junction and to the north of the equator in Colombia and Ecuador at distances of 20 degrees of arc length or more from northern Chile, but do not show evidence for low-frequency earthquake activity in our study area.

In order to clarify that the signals we have observed are primarily attributed to (fast) earthquake activity, we have added the word "earthquake" on l. 109. We have also added text at the end of section Section 3.2 to note that the waveforms we inspected are generated by earthquakes and have distinguishable phase arrivals that are commonly obscured in LFE/tremor waveforms (l. 359-368).

3) At the detection of local earthquakes using waveform data, the authors mention "move-out signatures" (line 355). Could you please be more precise? Did you use Ts - Tp times or particle motion for discrimination?

You raise a good point! In our initial work, we did not assess the move–out speeds. Initially, we only focused on the P- and S-wave signals and the energy distribution that started at one station and moved systematically outward to nearby stations. We have now updated the figure to time-order the record sections by seismic wave arrival times, which makes the move-out speed signatures more visible (See Figure A below; this was Figure 6 and is now Figure 7). We have updated the manuscript (last paragraph of Section 3.2, as noted above) to include information about the move-out speeds, which we calculated from event A from Figure 7 (second row, first column). These speeds are 8.2 km/s and 4.8 km/s for P-wave and S-wave energy, respectively. Figures representing how these values were calculated are shown in Figures B and C below. Note these two figures were created solely to respond to the reviewer's question and are not included in the updated manuscript. Our estimated seismic wave speeds are consistent with seismic wave speeds for this region. Kaila et al. (1999) reported that the P-wave velocity ranged from 8.04 km/s at 40 km to 8.28 km/s at 250 km depth and that the S-wave velocity remained almost constant at 4.62 km/s at 40 to 210 km depth.

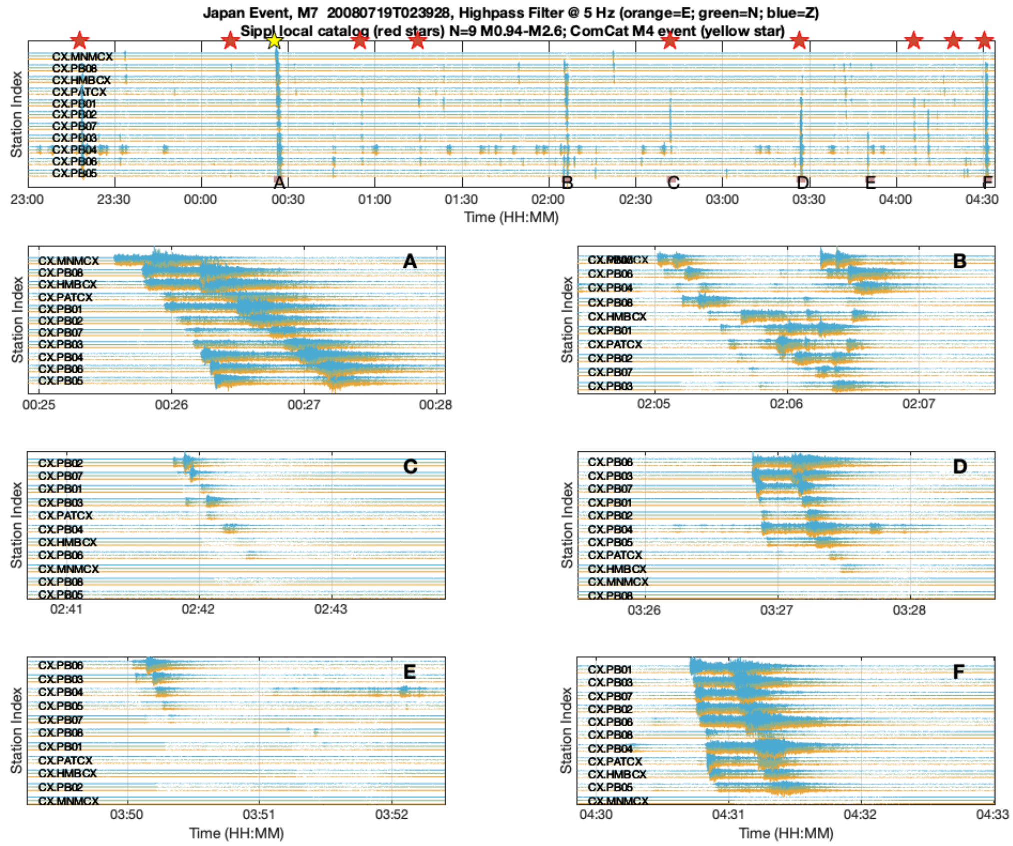

Figure A. Example of the reformatting of the original Figure 6 (now Figure 7), where we now order the traces by the arrival times. In doing so, the move-out becomes more apparent.

We have also added the units of time to the x-labels, as suggested by Reviewer B. We appreciate the reviewer's feedback; their comments greatly improved this figure. Thanks!

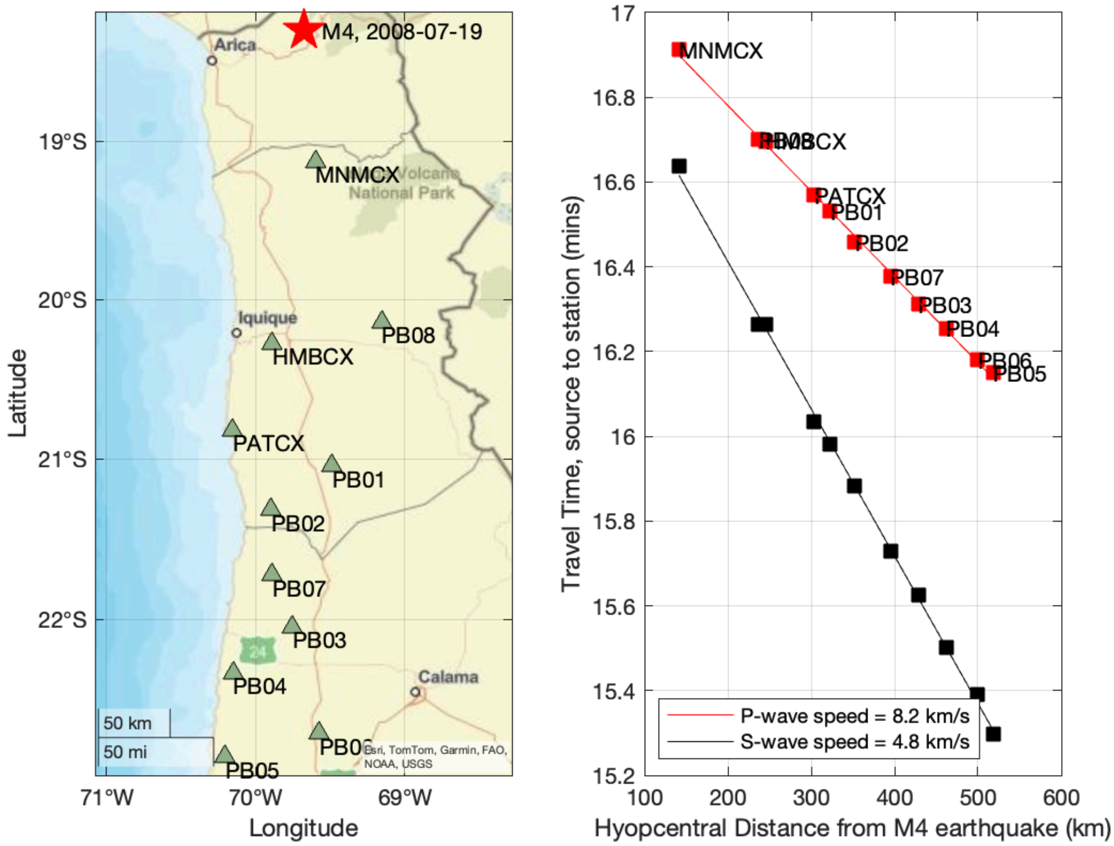

Figure B. Figure showing how the seismic wave speeds were determined using data from a 2008-07-19 M4 earthquake (see Figure B) that occurred north of our study region (map on left). A linear fit was used to compute seismic wave speeds based on time/distance information (figure on right). As you can see, there is minimal variation from the linear fit line, suggesting these speeds are robust and are consistent with seismic waves from an earthquake. Figure for illustrative purposes only to respond to the reviewer's question. This figure is not included in the updated manuscript.

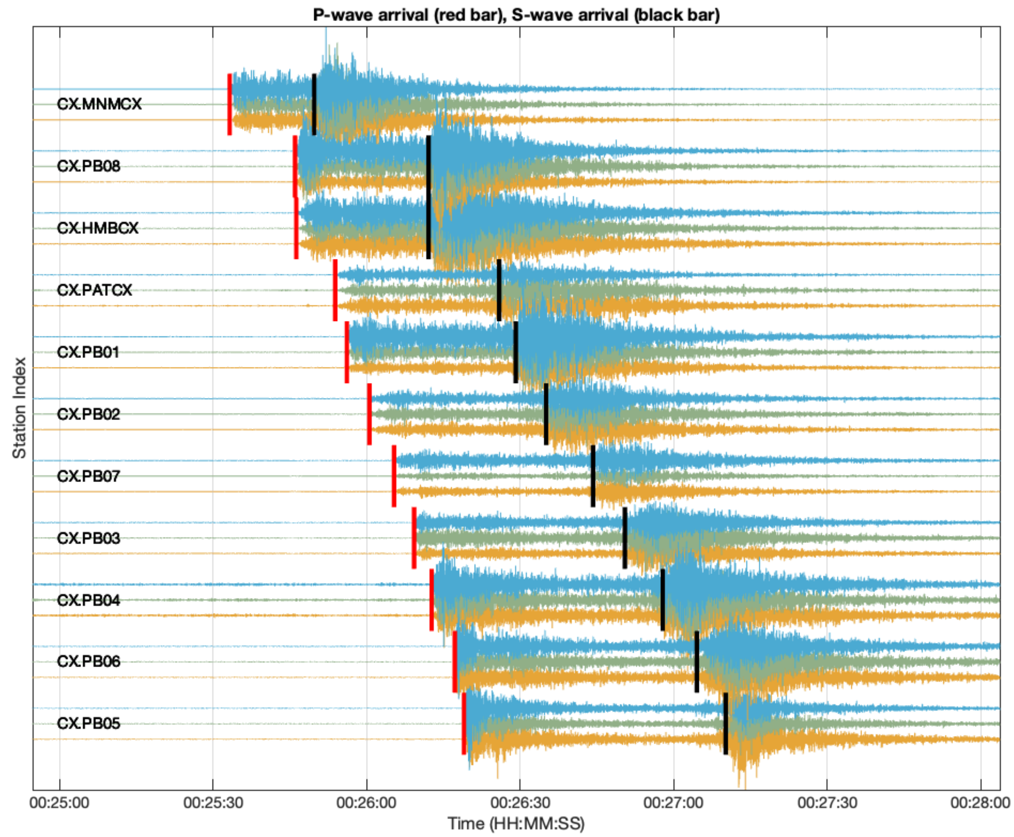

Figure C. Data from the M4 2008-07-19 earthquake (part of Figure 7A). To determine seismic wave speeds, we identified P-wave (red bars) and S-wave (black bars) arrival times. Figure for illustrative purposes only to respond to the reviewer's question. This figure is not included in the updated manuscript.

4) Introduction and Discussion would benefit from including references to studies done on remote triggering in Japan. For example, Miyazawa (2011) and Enescu et al. (2016) document the immediate triggering of earthquakes, after the 2011 M9.0 Tohoku-Oki an 2016 M7.3 Kumamoto earthquakes, respectively, at active faults and volcanoes/geothermal areas throughout Japan. Opris et al. (2018) document the short-term decay characteristics of the triggered earthquakes and Takeda et al. (2024) investigate the dynamic triggering in a systematic analysis for north-east Japan, before and after the 2011 M9.0 Tohoku-Oki earthquake. I also consider that both the plate interface and volcanic/geothermal environments are characterized by an abundance of fluids, so I would expect the excitation of fluids to have a role in the triggering process.

Thank you for the suggestion. We have added these corresponding Japanese references to the updated manuscript.

5) Minor mistype: at line 51, please replace "?" with the intended reference. At the end of caption of Figure 3, please replace "??" with the intended figure number.

> **Thank you for catching the missing citation and figure references (and for supplying them below); they have now been corrected.**

Bogdan Enescu, 2024/07/19

References:

Enescu, B., Shimojo, K., Opris, A. and Yagi, Y., 2016. Remote triggering of seismicity at Japanese volcanoes following the 2016 M7.3 Kumamoto earthquake. *Earth Planets Space,* **68**, 165 (2016). https://doi.org/10.1186/s40623-016-0539-5.

Opris, A., Enescu, B., Yagi, Y., and Zhuang, J., 2018. Triggering and decay characteristics of dynamically activated seismicity in Southwest Japan, *Geophys. J. Int.*, **212**(2), 1010–1021, https://doi.org/10.1093/gji/ggx456.

Miyazawa, M., 2011. Propagation of an earthquake triggering front from the 2011 Tohoku-Oki earthquake, *Geophys. Res. Lett.*, **38**, L23307, https://doi.org/10.1029/2011GL049795. Takeda, Y., Enescu, B., Miyazawa, M., and An, L., 2024. Dynamic Triggering of Earthquakes in Northeast Japan before and after the 2011 M 9.0 Tohoku-Oki Earthquake, *Bull. Seismol. Soc. Am.*, https://doi.org/10.1785/0120230051, 2024.

Kaila, K. L., Krishna, V. G., Khandekar, G., 1999. Preliminary models of upper mantle P and S wave velocity structure in the western South America region, J. Geodyn., v. 27, n. 4-5, pp. 567-583.

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Reviewer B:

Harrington et al. studied the dynamic triggering in the northern Chilean subduction zone by analysing seismicity change during 29 main events. They found a significant increase in the seismicity in low locking and aseismic regions. They do not see any significant threshold for triggering in the region, and many parameters or non-linearity may cause triggering; the following are my major and minor comments.

> ***Thank you. This is a nice summary of our work.***

Major Comments:

The authors analysed the earthquake catalogue of a longer duration of 28 days (14 days before and 14 after the main event), whereas waveform data of only 2 hours durations (one hour before and after). The authors themselves agreed that finding the trigger in the catalogue is impossible due to the high magnitude of completeness. I also agree that finding the trigger in the catalogue is sometimes impossible. The authors should analyse the waveform data for longer to establish delay triggering in the region. Automatic detection and manual identification of the local events may be carried out to establish triggering in the region.
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We agree that our study focuses on a more broad regional-scale investigation of triggering, which we place briefly in a broader context in the Introduction on l. 105-111, and in the Discussion section in the first paragraph, and on l. 491-92 (of the original version), and missing events are inevitable using our methodology. We focused on the two-hour time window as prior work has indicated this is a reasonable window to establish triggering (Pankow et al., 2004; Politz et al., 2012; Prejean & Hill, 2018), and it is more likely that events might be missed because of overprinting from the remote mainshock seismic energy. In the original manuscript, we include the longer windows in the statistical tests for completeness to explore delayed triggering, although we note that it is difficult to establish a causal relationship for longer time windows (See discussion of time window selection in the review by Prejean and Hill, 2018 (cited in the manuscript)). Exploring waveforms of longer durations in more detail would not significantly change our findings, nor would the results provide a greater understanding of the physics of the region as they would only net point measurements at a limited number of locations. A proper treatment would require a systematic approach to lower the magnitude of completeness of the entire region, which is beyond the scope of this work.

Our results are intended to demonstrate the presence or absence of triggering observations within the region and, if present, in which localized areas. Indeed, they demonstrate localized triggering responses within the region and allow us to link them to geological features and faulting conditions. The presence of additional "missing" earthquakes in the waveform analysis corroborates the statistical catalog results. As such, the results stand on their own, and provide evidence that continued work aimed at pinpointing triggering mechanisms performed on a more detailed spatial and temporal scale is worthwhile.

Specifically, the localized areas with increased triggerability provide guidelines indicating where future studies directed at uncovering specific triggering mechanisms should be conducted using enhanced catalogs and more in-depth waveform analysis. Moving forward productively will require a different approach than the one used in our study to enhance the catalog, such as template matching (i.e., Skoumal et al., 2014; Diaferia et al., 2024), AI (Mousavi et al., 2018; Ross et al., 2018;), or other methods that systematically and automatically enhance the catalog. However, doing so would be a heavy lift and clearly outside the scope of this study. More importantly, although additional catalog enhancements using our methodology might reveal some more subtle triggering locations, these would not be all that helpful because, to move forward, the most obvious triggering locations should be investigated first (i.e., the ones we have already identified). The sound results already presented in the manuscript provide a roadmap for the next steps. In order to make the broad objectives of this study clearer, we have edited the text on l. 106-111 in the Introduction section. We have also modified the sentence on l. 365-66 to note that the waveform analysis corroborates the statistical results. Finally, we have edited the text in the last paragraph of the Discussion (l. 500-502) to highlight the aims and findings of this work, and to emphasize that future work will focus on isolating triggering mechanisms using detailed approaches (e.g., catalog enhancement).

Authors confirm earlier published work (Victor et al., 2018), and I do not see an addition to the earlier study. If so, it may be explained in the text.

Apologies, but we do not understand this suggestion. We searched for Victor in our original manuscript (12 locations) and found no indication of missing text or confusion about past studies. Perhaps the reviewer meant Sippl, not Victor? However, on L121-124 of the original manuscript, we clearly state how the Sippl et al. (2023) catalog differs from the Sippl et al. (2018) catalog, as listed below. No change was made.

> ***The catalog-based method quantifies changes in local seismicity bracketing stressing events (i.e. candidate main shocks) using a new, local seismicity catalog described in Sippl et al. (2023). The Sippl et al. (2023) catalog is an expansion of previous work (Sippl et al., 2018) that includes an additional seven years of data and phase picks from a larger number of stations.***

The figure quality is not very excellent, and it may be improved.

Thank you for pointing this out. We initially submitted low-resolution figures to keep the file size manageable, but now we have added high-resolution figures to the updated manuscript and enlarged Figs. 2, 3, 5, 6, 7, and S2 to make details more visible. We have also expanded the original Figures 5 and 6 into three figures (new Figures 5, 6, and 7)) to emphasize significant details.

Please show an example of a delay triggering up to 14 days.

Fig. S2 shows P-values exceeding 0.95 for two candidate mainshocks with time window lengths of 14 days: 2 April 2018 (global) and 7 April 2011 (Japan). We now make specific reference to these two mainshocks in the caption of Fig. S2 and on l. 4448-449 of the revised manuscript.

Minor Comments:

In Figure 1(b), the periods of networks 8F and 8G differ in insert and caption. Is it CX or CK network?

Networks 8F and 8G are temporary deployments operated by the FU Berlin and the GFZ Potsdam. They are separate from the permanent CX network operated by the Integrated Plate Boundary Observatory Chile (IPOC) network, which is why we distinguished them in the caption. We have corrected the time period for network 8G in the map legend and the figure caption, noted the distinction in the text in the figure caption, as well as noted that the data section contains information on where to obtain the data.

I do not see any reason for showing decay lines for three earthquakes (M7, M8, M9) shown in Figure 2.

We prefer to retain the three decay lines in Fig. 2 because they give the reader a sense of the differences in PGV generated by different magnitudes of earthquakes. Similar plots with multiple lines are used in other triggering studies (e.g., Pena Castro et al., 2019; Fan et al., 2021; both referenced in the manuscript; Zhang et al.,(2018), etc.). We have elected to make no changes to this figure.

Figure 3: Please add the explanation for different colours in the Histogram, and in the caption, please write the Figure number in the place of ??.

Thank you for catching this omission and the typesetting error. We have added a revised version of the figure that includes a legend, and an explanation of the colors in the caption, and have fixed the reference to figure S2.

Figure 5: Please write the unit of time and the same issue with Figure 6. Please also increase the size of the figures.

We have added the unit of time to Figure 5 (now Figures 5 and 6) and Figure 7; thank you for alerting us to that omission. We have also divided Figure 5 into two separate figures, which increases the figure size and improves the figure's readability.

References: Please write the doi of all the references.

All available DOIs have been added to the references.

Line 18: Please add a space between "twofold".

We have left the word "twofold" together as written in the New Oxford American Dictionary.

Line 51: I do not understand (e.g. ?)

Thank you for catching the missing reference. It has now been fixed.

Please provide a reference for common triggering near geothermal regions.

We have added a reference in response to R1 (Enescu et al., 2016) that refers to triggering in geothermal fields.

Line 126: The authors reported M_c of 2.7 estimated by Hainzl et al. (2019). It would be better to show your estimation and its variation with time.

Thank you for your suggestion. The primary aim of this study is to establish if northern Chile is susceptible to remote-dynamic triggering on a broad, regional scale, and, if so, to pinpoint which areas are more susceptible. Both the catalog statistical study and the waveform analysis enable us to highlight specific localized areas where seismicity rate changes occur in response to stressing events, and enable us to make interpretations based on geology and inferred faulting conditions. Given the relatively high Mc for this catalog, our results will be robust in finding the most obvious triggering locations. Confirming this statement, the work by Hainzl et al. (2019) estimated Mc rigorously for both the catalog in aggregate, as well as sub-populations of earthquakes (e.g., in the downgoing plate, at intermediate depths, in the forearc, etc.). Their Figs. 2 and 3 (included in Figure D below for ease) demonstrate that in obvious triggering locations, we uncover that the Mc of 2.7 is an upper limit for the different areas that are compared, and that in the bulk of seismicity in the catalog that occurs in the forearc and downgoing plate, the Mc is lower (Mc ~2).

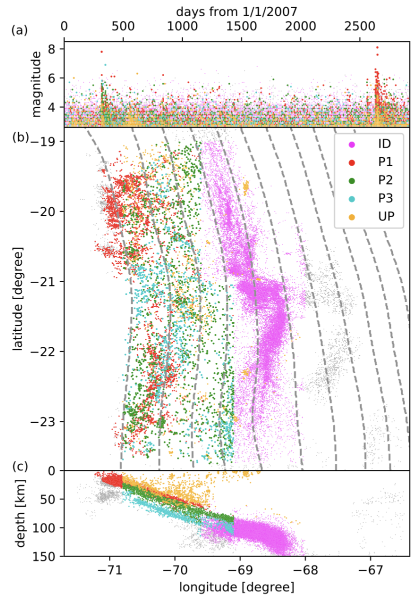

Figure 2. (a) Time series, (b) map view, and (c) cross section of the earthquakes with $m \geq 2.7$ recorded in Northern Chile between 1 January 2007 and 1 January 2015. Colored points refer to the different subsets introduced by Sippl et al. (2018): interface (P1), upper band (P2), lower band (P3), intermediate depth (ID), and the upper plate activity (UP). The remaining events are shown as gray dots. The slab depth of 20 to 220 km is indicated in steps of 20 km by dashed gray contour lines according to the Slab1.0 model (Hayes et al., 2012).

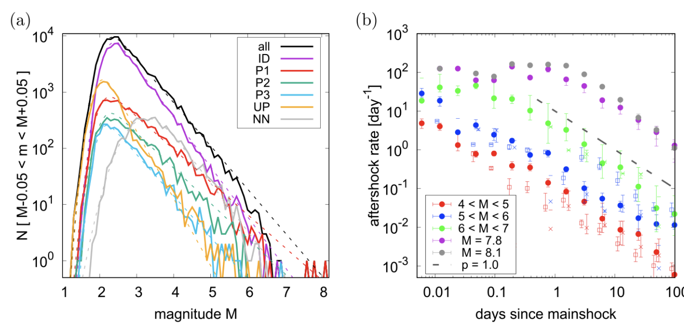

Figure 3. (a) The observed (solid line) and fitted (dashed line) frequency-magnitude distributions of the different subsets. The fitted lines refer to the model of Ogata and Katsura (1993) with parameters provided in Table 1. (b) The temporal decay for aftershocks in the subset P1 (points), P2 (squares), and P3 (crosses) for different mainshock magnitude ranges. Here the squares and crosses are shifted slightly in time to enhance visibility. The points and error bars refer to the mean and the 90% confidence interval related to alternative aftershock selection parameters, while the dashed black line refers to a Omori p value of 1.0. The numbers of mainshocks for the standard selection parameters are $N_{P1} = 118, N_{P2} = 140, N_{P3} = 71$ for $4 \leq M < 5$; $N_{P1} = 20, N_{P2} = 23, N_{P3} = 9$ for $5 \leq M < 6$; and $N_{P1} = 4, N_{P2} = 0, N_{P3} = 1$ for $6 \leq M < 7$.

Figure D. Figures from Hainzl et al. (2019) manuscript showing that the Mc level derivation in their study was very rigorous.

We agree with the reviewer that our results demonstrate the merit of continuing future work that might investigate triggering on localized spatial and temporal scales with an enhanced catalog with lower Mc. We intend to continue this work in the future. But, such extensive work is outside the scope of this study and would not change our interpretations. (Please also see the last part of the response to Reviewer B's first comment.) The current reference from Hainzl provides robust Mc estimates for the current catalog for which we could offer no improvements, and from which we have

obtained robust results that would not be changed with an additional Mc estimation. In that context, no additional text or figures were added to the updated manuscript.

Line 161: Please provide the original reference at the place of Li et al. (2023).

The original reference has been added.

Line 162: "s" should be in subscript.

Thanks, corrected.

Line 471-472: In 1985-1991…. . Please show in a figure with your estimations.

Please see response to the comment regarding l. 126 above and discussion of the Hainzl et al., 2019 reference that includes the figures above.

In the caption of Figure 6, correct the punctuation after "(lower six plots)".

Thank you for catching the typo; it has been corrected.

The following references may be added.

Saini, T., Bansal, A.R., Rao, N.P. *et al.* Tiny stresses are capable of triggering earthquakes and tremors in Arunachal Himalaya. *Sci Rep* **13**, 22223 (2023). https://doi.org/10.1038/s41598-023-49068-3

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Thank you for these references. They have been added to the manuscript and the reference list.

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