AGU Advances

Authors' Response to Peer Review Comments on

Very low frequency earthquakes in between the seismogenic and tremor zones in Cascadia?

Wenyuan Fan[1], Andrew J. Barbour[2], Jeffrey J. McGuire[2], Yihe Huang[3], Guoqing Lin[4], Elizabeth S. Cochran[5], & Ryo Okuwaki[6]

[1] Scripps Institution of Oceanography, UC San Diego, La Jolla, California, USA
[2] U.S. Geological Survey, Earthquake Science Center, Moffett Field, California, USA
[3] Department of Earth and Environmental Sciences, University of Michigan, Ann Arbor, Michigan, USA
[4] Rosenstiel School of Marine and Atmospheric Science, University of Miami, Miami, Florida, USA
[5] U.S. Geological Survey, Earthquake Science Center, Pasadena, California, USA
[6] Mountain Science Center, Faculty of Life and Environmental Sciences, University of Tsukuba, Tsukuba, Ibaraki, Japan

Authors' Response to Peer Review Comments on Original Version of Manuscript (2021AV000607)

[Please see attachment that begins on the next page.]

UNIVERSITY OF CALIFORNIA, SAN DIEGO UCSD

BERKELEY · DAVIS · IRVINE · LOS ANGELES · MERCED · RIVERSIDE · SAN DIEGO · SAN FRANCISCO SANTA BARBARA · SANTA CRUZ

INSTITUTE OF GEOPHYSICS AND PLANETARY PHYSICS
SCRIPPS INSTITUTION OF OCEANOGRAPHY
9500 GILMAN DRIVE, MC0225
LA JOLLA, CALIFORNIA 92093-0225

WENYUAN FAN
IGPP MUNK 323
wenyuanfan@ucsd.edu
858-246-4585

February 3, 2022

Dr. Thorsten Becker
AGU Advances

Dear Dr. Becker,

We would like to thank the editor Dr. Becker and the reviewers for their thoughtful, constructive suggestions, which have led to improvements in our paper. We have uploaded a new version of the manuscript and a copy with changes noted. Our response to the comments and changes are described below.

Sincerely,
On behalf of the authors,
Wenyuan Fan

Comments from editor Dr. Becker:

Alas, I have now received three detailed and, I think, quite helpful reviews of your manuscript, which are included below and/or attached. Some are critical, but all constructively so. As you can see, the reviews indicate that major revisions are needed before we can consider proceeding with your paper. I am therefore returning the paper to you so that you can make the necessary changes.

Everyone, including me, agrees that you have important observations and that your study is very well done. The reviewers do suggest some different emphasis of what's important here, and some do question some of your fundamental inferences and conclusions. You will have to address those concerns, including reviewer one's suggestion that the degree of advance is more limited than portrayed, e.g. in light of previous work, and perceived limited extent of generality.

So, no doubt that you have a very strong paper here, but I will have to make the judgment of suitability for AGU Advances contingent on your responses and additional discussion of these points. Obviously, we think this has potential, else we wouldn't have sent it for review in the first place, but "slow slip" is a crowded field and takes some detailed expert assessment to distill the robust novelties.

We thank Dr. Becker for his positive evaluation and review, as well as the thoughtful and thorough reviews given by the three anonymous reviewers. We have revised the paper thoroughly to address concerns from the reviewers, and we believe this has improved the clarity of our findings and conclusions. Our detailed responses are described below.

Comments from Reviewer #1:

I'll start by saying I sincerely apologize for the lateness of my review!

The authors present a very meticulous whodunit of three very low-frequency sources in Cascadia that appear to be triggered by a M6.9 earthquake. Given their proximity to slow slip and their resemblance to "classical" VLFEs (oxymoron?), the authors suggest they have observed some of the largest VLFEs to date. They validate the observations with both seismic and strain records, and build a convincing case that these are in fact independent tectonic events and not say, scattered waveforms from the triggering earthquake. The manuscript is well written and the figures are clear and informative.

I commend the authors for the careful work presented here. That said, I would say the two "big picture" takeaways here are a bit overblown (more details in comments below). Re megathrust extending into the downdip gap: there has already been much debate about this possibility and I'm not sure how an observation of a large VLFE moves the debate forward. Re slow earthquake scaling: the manuscript does not discuss two major papers (noted below) that have already called into question the M∼T scaling proposed by Ide et al., Nature, 2007 more than two years ago now. That is why my overall feeling is that while the manuscript and observations are carefully presented and should be published, I'm not sure that this paper is quite appropriate for the intended broad audience of AGU Advances.

We thank the reviewer for the critical review. We have revised the slow-earthquake scaling discussion section to include the suggested references (L511-524). It is true that Michel et al., *Nature* 2019 and Frank & Brodsky, *Sci. Adv.*, 2019 have examined the scaling relations of slow slip events, but there is a relative lack of similar investigations for VLFEs, which is the focus of this study. To date, VLFEs remain challenging to observe in Cascadia, and they are poorly understood compared to other classes of slow earthquakes in the region: the physical nature of VLFEs in Cascadia is still under heated debate (e.g., Gomberg et al., *JGR*, 2016; Hutchison & Ghosh, *GRL*, 2016). We also agree with the reviewer that the idea of a megathrust rupture possibly extending into the downdip gap in Cascadia is not new. However, no moderate to large seismic events, including both earthquakes or VLFEs, have been robustly observed in this region; the 2001 Nisqually earthquake had moment magnitude of 6.8, but it predates dense geodetic measurements. The lack of such robust seismic observations and a lack of offshore geodetic instruments leave the seismogensis of the gap zone unclear, and that is why there is the ongoing debate (as the reviewer notes). This knowledge gap can only be addressed by robust seismic and geodetic observations of slip events in the region with careful uncertainty analyses.

Our observations conclusively show that VLFEs (for the lack of a better term) exist in Cascadia and they are capable of generating continental scale coherent surface wavefields. More importantly, we report the first geodetic observation of VLFEs across all subduction zones. Furthermore, we report the largest VLFE with a moment magnitude of 5.7 that has ever been reported at all subduction zones. We evaluate the results with a suite of uncertainty analyses and demonstrate the fidelity of the observations. Even though our observations do not directly address the general stress/strength and frictional properties of the megathrust at the gap zone, these well-constrained VLFEs indicate permissive fast slips with a large moment in the region, which have direct implications in the rupture dynamics at the gap zone. Our case study points out that

there is a clear need for additional research to detect additional events and better understand this phenomenon. Because of the important implications, such solid observations would be of interest to a broad scientific audience beyond the slow earthquake community, including geodynamics, seismology, geodesy, theoretical geophysics, rock physics, and structural geology. We believe our study will facilitate further discussion and research on the topic in the subduction zone science community, therefore, suitable for publication at *AGU Advances*.

I detail below my list of comments:

- I would avoid using acronyms in a plain language summary, especially jargon laden ones like ETS

Done.

- Line 59 Brown et al. is a paper about LFEs, not tremor

Done.

- Line 60 "couple" implies a physical interaction with some mechanism, when it is just rather a spatiotemporal correlation that defines this phenomenological relationship

Revised (L60–61).

- Line 62 "semi-regularly" every 11-15 months (add average recurrence time)

Done.

- Line 97-108 I think it would read better to put the following paragraph here first, describing the method first, and then discussing how and why the method is efficient for the study at hand

Done.

- Line 133 I don't quite understand what this optimal location is, if it isn't the location that provides the minimum misfit...?

The optimal location minimizes the arrival angle misfit defined in Equation 5 in Fan et al., *GJI*, 2018. We have revised the text for better clarity (L148–149).

- Lines 144, 147, etc. no hyphen needed for focal mechanism

Done.

- Line 144-158 same remark as before: the method should be described before it's stated why it's effective

We agree with the reviewer on the point. The L144–158 in the previous manuscript only described the procedure and did not comment on its performance. We have further revised the section for better clarity (Section 2.2).

- Line 159 how are the synthetic waveforms aligned with the observed waveforms? Because if there are differences in arrival times with the best-fit source, that phase difference will impact the correlation coefficient if the synthetics aren't properly aligned. This is likely not a major issue, but I'm still curious. If there is no alignment to account for errors in travel times and location, is this at least part of the reason why focal mechanisms can't be estimated for the other two events?

The E3 synthetic waveforms are cross-correlated with the observations for alignment: the synthetic waveforms include 3600 s noise-free time series and the observations are 300 s waveforms such as those outlined by the yellow band in Figure 3. The E1 and E2 events occurred early and their waveforms are in the coda waves of the Gulf of California earthquake (e.g., Figure S2). While they can be detected by cross-correlating nearby stations of each triad, the signal-to-noise level is too low to yield robust focal mechanism solutions. There is also a lack of high-quality VLFE catalogs near E1 and E2, while E3 is spatially close to the events identified in Ide, *JGR*, 2016. We have revised the texts to improve the clarity at L176 and L181–182.

- Line 203 why not just define this equation as a function of C, the number of components of the strain meter? As written, the number of terms seems arbitrary. Something rather like epsilon = sqrt[sum(g_c) / C] would be more straightforward

This is a good point; we have changed the equation to a more general form.

- Line 212 stresse $->$ stress

Done.

- Line 274 relinquishing = strange word choice

Revised (L283).

- Line 283 a subhorizontal dip is a bit surprising, no? How does it compare to the expected dip from slab models? What sort of uncertainty is estimated for the dip angle?

We agree that the subhorizontal dip of E3 may suffer large uncertainties. The Slab2 model in the region has a dip around 10°, which is likely in the uncertainty range of the moment tensor solutions in Ide, *JGR*, 2016. The E3 focal mechanism is determined by matching the reported solutions in Ide, *JGR*, 2016. The catalog might not include all possible mechanisms. We have added discussion at L293–294.

- Line 288 The expected depth based on Slab 2.0 should be in text

Done.

- Line 293 the m in Newton meters shouldn't be capitalized

Done.

- Line 328 contracts $->$ I assume "contrasts"?

Correct; fixed.

- Line 334-336 One big question (I know it is not a simple one): why did this M6.9 EQ trigger these events and not these other bigger earthquakes? How do the dynamic stresses at the location of E3 compare for each of these earthquakes? For example, a significant difference in dynamic stressing could explain

The M6.9 Gulf of California likely ruptured at a supershear speed with its Mach cone directed towards Cascadia. The detailed rupture process analysis is documented in a companion paper (Fan et al., *in revision at GJI*, 2021). We speculate that the earthquake rupture process might have generated abnormally large strain with atypical frequency content across the west coast US. We have revised the part for better clarity (L426–429) but invite the reviewer to read Fan et al., *in revision at GJI*, 2021 for technical details (included in this submission). Investigating the significance of the dynamic triggering process requires a systematic investigation of seismic sources and the dynamic strains over an extended observational period. It is an important topic but far beyond the scope of this study.

- Line 420-439 I'm not sure why this takeaway message is presented as novel (megathrust could extend into gap zone). We see that some slow slip extends into this region, but tremors do not (line 416); this is similar to neighboring but not overlapping tremor and slow slip in Boso (Nankai) and Guerrero (Mexico). It is mechanically difficult to explain that this gap zone is creeping continuously between a long-term locked up dip zone (mega thrust) and a short-term locked down dip zone (slow slip). The simplest solution is then that this gap zone is locked on some intermediate time scales, slipping sometimes as slow slip or VLFEs or perhaps even in a future earthquake. Basically, I'm not sure what the observation of a VLFE within the gap zone demonstrates further than what we already knew?

We have revised the discussion to emphasize that:

1. **As the reviewer mentioned, tremors and slow slips have been well observed globally with clear, non-overlapping characteristics in the gap zone, but concrete evidence of VLFEs occurring in the region has not been reported before, leaving the possible rupture properties of the region unconfirmed.**
2. **Even though the gap zone in Cascadia has been hypothesized as locked on some intermediate time scales, there have never been robust seismic field observations to validate the hypothesis. This study is the first to show conclusive evidence of slip behaviors reminiscent of such interpretations with moderate size locking patches.**
3. **The magnitude of E3 is likely around Mw 5.7 and the magnitude quantitatively implies the scale of the locked megathrust fault patches, which has been difficult to resolve given the current seismic and geodetic networks.**

We appreciate the critical review and have revised the section, but we disagree with the reviewer that the gap zone properties are well-known.

- Line 441-462 New observations related to the scaling of slow earthquakes have been reported by Michel et al., Nature, 2019 and Frank and Brodsky, Sci. Adv., 2019, with both studies suggesting the M $\sim$T does not hold (although the assumptions made in each paper are different). The message from the results here corroborates the takeaway that a M$\sim$T scaling conflicts with observations, but the takeaway presented here that M$\sim$T scaling doesn't hold is not necessarily new. In any case, these papers should be discussed in light of the observation made here

We thank the reviewer for the important references and we have revised the discussion accord-

ingly (L511–531).

- I would suggest that it's essential to show a figure similar to Bartlow et al., GRL, 2020 Figure 3a that maps the cumulative distribution of slow slip. That way it becomes obvious to the reader how the location of the reported VLFEs compare to the distribution of slow slip, which is a key message the authors want to convey here.

We thank the reviewer for the excellent suggestion. We have added a new figure to respond to the suggestion (Figure 8). This figure shows the detected VLFEs along with the best fitting region that resolves both the seismic and geodetic locations, the transition zone, and tremor and slow slip distributions.

- I would strong suggest against using the acronym ETS. It conflates geodetic observations with seismic observations, with no real added value. It can even be misleading as in line 428: the loading stresses discussed are due to the aseismic slow slip, not the seismic tremor. I would suggest just simply using "slow slip" for transient aseismic slip and "tectonic tremors" for the seismic signal. This would not only simplify the discussion of what actually happens in the gap zone (i.e. slow slip extends up dip, but tremor does not), but make the entire manuscript more friendly to the uninitiated by having one less acronym.

We thank the reviewer for the suggestion. Done.

Comments from Reviewer #2:

Summary:
This is an overall strong paper with significant contributions worthy of publication with minor revisions. New VLFEs were detected in the Cascadia subduction zone, with at least one located in the gap zone between ETS events and the locked seismogenic zone. This event is the largest VLFE recorded to date and very impressively the first to be detected geodetically. The location of these VLFE(s) in the gap zone has implications for the modes of slip that we can expect in the gap zone and thus the seismic hazard. The paper is well organized, and the figures are clear and descriptive. I would like to see more details about the two VLFEs that were not modeled and more of a background about VLFEs in general and in Cascadia to better set the stage for this paper's significance in the context of what we currently know about VLFEs and slow slip.

We thank the reviewer for the thorough, constructive review. As suggested, we have further discussed E1 and E2 in the manuscript and better reviewed the current understanding of VLFE in general and in Cascadia.

General comments:
- Could the 2009 ETS have been triggered by the 2009 Gulf of California earthquake? Please address this in the discussion section.

We thank the reviewer for the suggestion. The 2009 Cascadia ETS could have been dynamically triggered by the earthquake. The triggering mechanism might have been nonlinear or the 2009 ETS may have generated tremor and slow slip asynchronously before the documented starting date in Bartlow et al., *GRL*, 2011. We have addressed the point at L402–409. The M6.9 Gulf of California likely ruptured at a supershear speed with its Mach cone directed towards Cascadia. The detailed rupture process analysis is documented in a companion paper (Fan et al., *in revision at GJI*, 2021), which is included in this submission.

- Provide a figure showing the VLFEs (at least E3) compared to a local earthquake and tremor (i.e., 20-50 s band-pass filter for VLFE, 2-8 Hz band-pass filter for tremor, highpass filter for earthquake). This will provide more confidence that you have detected new VLFEs.

As the reviewer suggested, we have added a new supplementary figure (Figure S4) comparing seismic records of the three classes of seismic sources at five PNSN stations. The comparison confirms that E3 is not a regular earthquake.

- The analysis of E3 is robust and I think you have found a new VLFE. I am less certain about E1 and E2, but I am okay with this because you do not interpret these as much. However, I still want to see more waveforms of these events if you are going to call them VLFEs. Please include a figure either in the main text or supplement showing the waveforms for E1 and E2, perhaps something like the waveforms in Figure 5 at a station far enough away for there to be a separation between the triggering earthquake code and the VLFEs.

As the reviewer suggested, we have added a new supplementary figure (Figure S2) comparing seismic records of E1, E2, and E3. The E1 and E2 waveforms are less obvious comparing to the E3 records, possibly impacted by the mainshock coda waves.

Comments by section:
Abstract/Introduction:
- Key Point 2: Based on the location uncertainties, E2 and possibly E1 may not be in between the seismogenic and tremor zones. Consider changing to "These VLFEs likely occurred?"

Done.

- Key Point 3: The geodetic detection of E3 is an important finding and contribution in this paper. Consider adding this to Key Point 3, something like: "The largest VLFE has a moment magnitude of 5.7; this is the largest VLFE detected and is the first to be detected geodetically."

Done.

- Abstract Line 24: "Out of all types of slow earthquakes, very low frequency earthquakes (VLFEs) are most similar to regular earthquakes" – I do not agree with this statement. How are VLFEs more similar to regular earthquakes than LFEs, which although are depleted in higher frequencies do have some overlapping frequency content with local earthquakes, unlike VLFEs? Further, if "the physical nature of VLFEs are poorly understood despite their frequent occurrence" (line 26) then how do you know they are most similar to regular earthquakes? This requires more explanation, or should be rephrased, stating instead that they could potentially be the most similar to regular earthquakes. Last point, is the occurrence of VLFEs really that frequent? Not many VLFEs have been reliably detected, which is partly why this work is significant.

We agree with the reviewer that the VLFE mechanism are less well understood, leaving it difficult to directly compare them to regular earthquake. We have revised the Abstract (L25–28).

Introduction:
There should be more background discussing VLFEs in general and particularly in Cascadia. For example, please include that VLFEs are rich in low frequency energy in a band of ∼20-50 seconds and depleted in frequencies higher than 1 Hz compared to local earthquakes of similar magnitudes and compared to tremor which has most energy in a 2-8 Hz band. It would also be good to include that VLFEs are thought to be a seismic manifestation of slow slip, similar to tremor and LFEs. Please also discuss a little background of VLFEs detected in Cascadia from other studies like Ghosh et al. 2015 (i.e., spatiotemporally correlated with the 2011 ETS event) and Hutchison and Ghosh, 2016 (i.e., temporally but not spatially correlated with tremor during the 2014 ETS event but located in the ETS depth zone). This will be important later in the discussion where you make the point that these VLFEs are different than previously detected VLFEs in the region.

We thank the reviewer for the great suggestion and have revised the introduction to address the comment (L80–91).

- Line 54: State how slow earthquakes differ from regular earthquakes

As suggested, we have clarified the differences between slow and regular earthquakes (L80–82).

- Line 59: Why "seismic tremor" instead of "tectonic tremor" or "non-volcanic tremor" which is how the references you provide refer to it? All tremor – volcanic and nonvolcanic is "seismic."

Agreed and revised (L60).

- Line 65: "Additionally, typical VLFEs in the region can have equivalent moment magnitudes ranging from 2.1 to 4.1... These events accommodate a portion of the slip deficit at the subduction zone and concentrate along a band at depths of 30–50 km, about 10 to 15 km deeper than the downdip edge of the seismogenic zone." This makes it sound like the VLFEs concentrate at 30-50 km but I think you are referring to ETS concentrating at 30-50 km ? please make this clear. Again, please also state somewhere in the Introduction where VLFEs have previously been located in Cascadia. Same depths as ETS?

As suggested, we have revised the introduction to improve the clarity of these statements (L63–68).

- Line 85: You state in the Abstract that this earthquake occurred in the Gulf of California, state that here too for consistency. Also state what day in August it occurred and give some more background about this earthquake. You give more information about this earthquake in the first paragraph of Results, but it would be better to introduce this here.

Revised (L97–98).

Datasets and Methods:
- Why use vertical components instead of horizontals to find VLFEs? Please explain.

We use Rayleigh waves to detect and locate seismic sources. Without knowing the source location, vertical components can provide more coherent records for each subarray to extract the propagation directions than horizontal components. While horizontal records can also be used, the array needs to be dense enough that the inter-station separation does not impact the Love and Rayleigh wave projections to the NS and EW components. The USArray configuration is not suitable for such a direct application to the horizontal records. We are currently developing new techniques to use horizontal records and the study will be submitted later this winter. Details have been added at L123–124.

- Line 115: Give a brief, one sentence explanation of what quality control is done because this is important. It does not need to be long, but please provide some idea of what QC entails, either in the main text or the supplement.

Done (L120–121).

- Line 125: "Due to the spatiotemporal correlation" Is it a spatiotemporal correlation, or just a temporal correlation? The 3 VLFEs are not located near the mainshock event, I would not call them spatially correlated. The main correlation is temporal, i.e., the VLFEs occurring just after the passing of the mainshock. If you are instead trying to make the point that the 3 VLFEs are nearly co-located compared to the mainshock distance, please make this clear.

As suggested, we have revised the text for better clarity (L139). The spatiotemporal correlation results from the separation distance between the earthquakes and VLFEs and the occurrence times of these events.

- Line 128: Do you think more subarrays detected E3 (i.e., SNR is higher) because it is larger magnitude than E1 and E2 (it likely is because static strains are not evident following these events), or because E1 and E2 occur during the coda waves from the triggering earthquake? Or both?

E3 is the easiest to detect and locate because of its late occurrence time, reducing impacts of the coda waves from the earthquake. Our beamforming analysis only allows one detection every 180 s, and the strong coda waves might have caused less detections of E1 and E2. However, we are not certain whether E3 has a greater magnitude than the other two events. As the reviewer suggested, we have revised the text for better clarity (L142–145).

- Line 182: This sentence stating why the near-field stations are not used should be stated sooner in this section.

As suggested, we have reorganized the text for better clarity (L161–163).

Results:
- Results paragraph 1: the background provided for the Canal de Ballenas earthquake should be included earlier in the paper.

As suggested, we have reorganized the text for better clarity (L214–220).

- Line 264: In addition to the time after the triggering earthquake origin time, include how long E3 occurred after the triggering earthquake passed its location. If it is being dynamically triggered, this is the timing that is most important.

As suggested, we have further elaborated on the temporal relations between the earthquake and E3 (L274–275).

- Line 275: I don't like the logic that since they are in the vicinity of Cascadia slow earthquakes, they are most likely VLFEs. They should be identified as VLFEs for observational reasons; you stated this well in the previous sentence. Further, you argue that these VLFEs are not collocated with ETS. I suggest removing "Therefore, being in the vicinity of the Cascadia slow earthquakes, our newly located sources are most likely VLFEs."

Agreed and revised (L284–285).

Discussion & Conclusion:
- Very thorough and well-written discussion section!

We thank the reviewer for the positive evaluation.

- Line 458: How are these VLFEs different from previous events? Please be explicit since this is an important point. As stated in my earlier comment about giving more background about previously identified VLFEs in the area in the Introduction, previous studies should again be discussed here and similarities/difference be analyzed.

As suggested, we have carefully compared our detections with previous reported VLFEs in Cascadia (L524–529).

- Could the ETS event 3 days after E3 have been triggered by the triggering earthquake? You do a nice job discussing the possible triggering of the ETS event by E3, but please discuss triggering by the mainshock in Section 4.2 as well. This could include a discussion of dynamic stresses from the mainshock vs static stress changes from the VLFEs.

We thank the reviewer for the suggestion and have added discussion on the possible triggering relations between the mainshock, E3, and the 2009 Cascadia ETS (L402–409).

- Very strong and concise Conclusion.

We thank the reviewer for the positive evaluation.

Minor edits/comments:
- Line 35: Change “which” to “whose”

Done.

- Line 109: “The AELUMA method...”

Done.

- Line 110: Change “propagating” to “propagate”

Done.

- Line 124: “soon after” – please be more specific

Revised.

- Lines 131 & 137: Figure 1, not Figure 2, shows the location uncertainty

Done.

- Line 196: Missing a “.”

Done.

- Line 212: Change “stresse” to “stress”

Done.

- Line 328 – contracts should be contradicts?

Done.

- Line 435: whose duration, not which duration

Done.

- I could not find a reference to Figure 9 in the main text – if it is missing, please add it. I think this figure could also be moved to the supplement.

As the reviewer suggested, we have moved the previous Figure 9 to the supplement as Figure S3.

Figures:
- Figures in general are very clear. Please add scale bars to all of your maps.

Done.

- Figure 1: Include the August 3, 2009 date of the VLFEs somewhere, either on the map or in the figure caption, not just the time of day. Include the origin time of the mainshock time somewhere as well so it?s easy for the reader to see the temporal correlation/dynamic triggering. Label tectonic plates.

Done. We have added the information in the figure legend.

- Figure 3: Briefly describe the polarity plot more, either in the caption or in the main text, in case the reader is not familiar with what a polarity plot is.

Done. We have explained the polarity plot in the figure caption.

- Figure 5: I like that you show the waveforms of mainshock and E3.

We thank the reviewer for the positive evaluation.

- Figure 8a: Can the colors for E1-E3 be consistent with Figure 1 and 2? Or at least have E3 be indicated in red instead of E1.

Good suggestion; we made the colors consistent.

- Include a figure (even in the supplement) showing the best VLFE (E3) compared to a local earthquake and tremor (i.e., 20-50 s filter for the VLFE, 2-8 Hz filter for tremor, HP filter for earthquake). This will provide more confidence that you have detected new VLFEs.

As the reviewer suggested, we have added a new supplementary figure (Figure S4) comparing seismic records of the three classes of seismic sources at five PNSN stations.

- Include a figure either in the main text or supplement showing the waveforms for E1 and E2, perhaps something like the waveforms in Figure 5 at a station far enough away for there to be a separation between the triggering earthquake and the VLFEs.

As the reviewer suggested, we have added a new supplementary figure (Figure S2) comparing seismic records of E1, E2, and E3.

Comments from Reviewer #3:

This paper reports dynamically triggered earthquakes in the Cascadia region. Geodetic analysis by strain meters strongly supports that these events occurred in this region. These are very interesting phenomena and should be worth of publications. The text is clearly written and methods seem to be sound. In the presented observations, however, it is not clear to me whether these events actually occurred between the seismogenic and tremor zones, while the title is "Very low frequency earthquakes in between the seismogenic and tremor zones in Cascadia?". In addition, quantitative discussion should be necessary to support that these are very low frequency earthquakes. I think that this is a significant finding whether they are VLFE or not, and whether they occur at the gap region or not. After modifying discussion and conclusions, this paper should be worth publishing in AGU Advances.

We thank the reviewer for the constructive review. As suggested, we have further discussed the E3 location and its uncertainty in the manuscript (L348–384). We also compared E3 with local earthquakes and tremor to better characterize its source properties (L511–531 and Figure S4).

#1 Location of very low frequency earthquakes
Error of located epicenters seems to be too large to conclude that these events occurred between seismogenic and tremor zones (i.e. the gap region). Ellipsoids shown in Fig. 1 overlap tremor zone, even in the narrowest case (i.e., E3). Moreover, the geodetically located source region of $\chi^2 = 1$ in Fig. 7c largely overlaps the tremor zone. These results seem to suggest that E3 occurred in the gap region or tremor zone. In the discussion and conclusions, however, it is concluded that E3 occurred only in the gap region without evidences to constrain the region. I think that a dynamically triggered M5.7 earthquake in the tremor zone is also very interesting, and worth reporting. Discussion and conclusion of such a case (i.e. events in the tremor zone) should be also included.

We thank the reviewer for the suggestion and have discussed a possible triggered event in the tremor zone (L370–384). We have now added a new figure to compare the relative locations between E3 uncertainty, geodetically located source region, and the Cascadia tremor zone (Figure 8). The E3 (seismic) location uncertainty is estimated based on a very conservative criteria (within 25% of the misfit minimum instead of the commonly used 5% threshold), which likely overestimated the uncertainty ellipse. The uncertainty in the geodetic location is unavoidably large because additional stations are needed to rigorously constrain the source based on the static deformation pattern. From the location uncertainty contours, the result seem to suggest that E3 could have occurred in the tremor zone as well. However, teleseismic surface waves are sensitive to source depth, and a change of E3 depth from 15–20 km to 30–50 km is highly unlikely as shown in Figure 4b. We have revised the text to improve the clarity (L348–384).

#2 Very low frequency earthquakes
It is not clear whether detected events are actually VLFEs. At first, the definition of very low frequency earthquakes (VLFEs) should be clarified in this paper. In terms of the definition of the scaling law by Ide et al. (2007), E3 is not a VLFE, as the slip of an M5.7 slow event is expected to last for more than several days (as discussed in Section 4.4). In this paper, VLFEs seem to be defined by the high frequency radiation. However, quantitative discussion cannot be found, even in Fig. S1. High frequency waves around E3 are recognized in Fig6b and Fig. S1. If regular earthquakes with similar

seismic moment occur close to E3, comparison of spectra would give sufficient information. Otherwise, theoretically expected amplitude in the case of regular earthquakes would be useful to show that these are VLFEs. In addition, typical duration of M6 regular earthquakes are about 10 s. Fig 4c suggests that the source duration is similar to that of regular earthquakes. Thus, I am not sure if this event is a VLFE. Some discussion on this point might clarify the difference from regular earthquakes.

We agree with the reviewer that the observed event share resemblance with both regular earthquakes (duration) and VLFEs (little high-frequency radiation). As the reviewer noted, in this study, we focus on the relative high-frequency radiation of E3 to interrogate its physical mechanism. We have revised the discussion to clarify the point (L511–531). Ideally, comparing the E3 spectra with local earthquake spectra would help to distinguish the two type of events. However, for stations in the Pacific northwest, E3 is largely buried in the coda waves of the Gulf of California earthquake and there are few M$\geq$5 earthquakes in the region. By comparing to local crustal earthquake waveforms, E3 is clearly not a regular local earthquake (Figure S4). Additionally, the Gulf of California earthquake is likely a supershear earthquake (Fan et al., *in revision at GJI*, 2021, see uploaded file), further complicating the amplitude estimation. The similarity between the E3 duration and that of a typical earthquake might suggest that slow and regular earthquakes follow similar governing physics and they might share similar scaling laws (L511–531). Supportive findings have been documented in Michel et al., *Nature* 2019, Frank & Brodsky, *Sci. Adv.*, 2019 and Wei et al., *Nat. Comm.*, 2021. We have revised the text to discuss the uncertainty of the source mechanism (L524–529).

The following comment is optional.
Some numerical models of VLFEs are recently proposed, for example, Wu et al. (2019, GRL), and Wei et al. (2021, Nat. Comm.). Perhaps, VLFEs in the latter paper may be similar to the detected events. Discussion of numerical models might be also interesting.

We thank the reviewer for the good suggestion and the references. We have now revised the discussion to compare the observations with numerical models (L464–469,L518–520).

#3 Comparison of geodetic observation (Fig. 6) It is not clear to me whether the observed strain change can be well explained by the synthetic data. Fig. 6a shows synthetic strain change expected from model. Fig 6b shows the observed strain changes. I think that Fig 6a and 6b cannot be compared directly, as they are related by the calibration coefficients given in Table S3. A figure to compare the amount and direction of strain might be significant to justify the modeled fault. In addition, values shown in contours in Fig 6a are too small to read, while bottom part of maps is not used. If possible, please magnify the target region. The discussion of geodetic observation is an essential part to support that these are really local events and not caused by seismic velocity structure. A nice figure would make this paper more convincing!

We thank the reviewer for the important comments and suggestion. The intention of this Figure 6 is to show the raw strain changes directly, and point out that the stations showing static strain are in the region where static deformation would be resolvable by the BSMs, assuming the source location and mechanism determined by the seismic analyses. Effectively, this is important confirmation that the static strains are not spurious, and the reason for deriving an independent location from the geodetic data – the subject of Figure 7.

Nonetheless your suggestions are helpful, and we have modified the figure to make the quantities more legible, and better utilize space. We have also revised the caption to better explain this.

#4 Strain meter, B003 In this paper, large misfit at B003 is attributed to the effect of non-linear ocean tide and uncertainty of the location of lobes (Lines 339-347). However, other stations (e.g. B004) are also close to ocean, and seem to have the same problem in terms of non-linear tide. Effect of lobes seems to be less sensitive in the case of RMS and areal strain. Can the data of B003 be ruled out by these reasons? In addition, if the effect of lobes is true, the geometry of fault might be strongly constrained.

These are excellent questions. It is certainly possible that data from B003 could be dismissed for the reasons pointed out, but the coastal tidal distortion is so strongly non-linear that it is not necessarily fair to rule out the effect (tidal distortion) because a nearby station is subject to the same distortions. Numerous studies have documented systematic discrepancies at B003 that are seemingly absent at B004, including most notably Roeloffs 2010, Hodgkinson et al 2013 Reuveni et al 2014, etc. Hence, we think the tidal distortion is only one of many factors that make B003 less reliable than the others nearby; we have modified the discussion points accordingly.

On the matter of deformation lobes, Figure 7 is our attempt to both confirm E3 and quantify the geographical uncertainty in the source of the static strain. While the maximum likelihood location derived from the strain observations sits close to the seismic-based one, the region of $\chi^2 = 1$ results encompass a relatively wide area; this is because of the relatively limited station coverage. Rather than attempt to use the strains to improve the source location, we prefer that they represent independent confirmation of the seismic-based source location and mechanism for E3.

Further, it would be possible to tightly constrain the geometry of the fault using static deformation observations had the strainmeter network given better azimuthal coverage; however, this is obviously not possible because of the ocean and the general cost associated with deploying borehole strainmeters. Until seafloor and fiber-optic geodetic technologies mature, these are limitations we must admit.

#5 Existence of similar events I am interested in how rare such events are. Some comments would clarify the rarity of these events, if author analyzed long-term continuous data. This is just an optional comment.

We thank the reviewer for the good suggestion. It is currently unclear how rare these events are (or what they are as discussed in comment #2). It is one of our eventual goals to produce a long-term catalog for VLFEs in the region to investigate the occurrence frequency. This effort is currently in progress.

#6 Events E1 and E2 Can the magnitude of E1 and E2 be roughly estimated, for example based on amplitude? At the timing of E1 and E2, can small high frequency waves be found as in Fig. S1?

We estimated the E3 magnitude by modeling its surface waves. E1 and E2 occurred soon after the mainshock and their waveforms are largely masked by the coda waves. More importantly, their locations are poorly constrained, limiting further investigations. This poses challenges in

modeling the E1 and E2 waveforms or measuring the waveform amplitude with fidelity at the 20 s period band, thus computing the E1 and E2 magnitude is difficult. From our previous studies, the detection threshold of our method is around M3.5.

Due to difficulties to model the high-frequency waves, it is unclear that the observed highpass records coinciding with the modeled E3 waveforms are definitely from the VLFE E3. As shown in Figure S4, these radiations might have been generated by tremors. However, they were not reported in previous tremor catalogs.

As demonstrated in a companion study, the mainshock likely ruptured at a supershear speed with its Mach cone orienting towards Cascadia (Fan et al., *in revision at GJI*, 2021, see uploaded file). Such mainshock rupture process could also generate abnormal ground motions. We have revised the discussion to clarify the point (L426–429).

#7 Fig. 4 In Line 746 (caption), ”N.M” should be ”Nm”. An explanation of the color scale in Fig. 4b, 4c should be added.

Done.