Review Summary for: ***Tectonic tremor: the chatter of magic underplaying beneath southern Vancouver Island?***

This manuscript went through two rounds of revisions:

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Version 1: Submitted Manuscript

Review #1

Recommendation: Revisions Required

The paper by Littel et al. submitted to Seismica highlights the presence of several clusters of tectonic tremors just above the plate interface in the Cascadias subduction zone. It represents a very interesting and careful case study focusing on the distribution of deep events and their meaning in a regional perspective. I enjoyed reading the paper which is well-written, accessible for non-specialists and well organized. I have been quite impressed (surprised?) by the resolution obtained on such remote processes and by the great level of understanding enabled by the observations shown there. Because I am not a seismologist, I mostly have (minor/moderate) remarks on the deep-seated geological processes and their interpretation. Some minor adjustments with bibliographic references is suggested as well.

I only have one major comment which is on Figure 11: I am not totally convinced with this mass balance calculation. First, I do not understand where the value of D=8000 km comes from. Second, each of the reflective plane in the E-layer (that should be call E-zone instead, because it is not a layer) corresponds to a sequence of ancient thrust faults that are not active anymore and that have different activity ages. At no time in the history of duplex formation were these planes tectonically active together. This is why I find that including all of these paleo-surfaces at once in this calculation is unjustified.

Line per line comments:

L.20, L.190 and later: I recommend not using the term ‘erosion’ that is typically used for the downscrapping of the upper plate crust (tectonic erosion; see papers by Scholl, von Huene, Clift or Vannucchi). I would replace it by ‘offscrapping’ or ‘peeling’ or whatever other adequate term, if possible already used in the existing literature.

L.74: around here, a brief section on regional geology and geodynamics would be useful to have, in order to better understand the context and why you have chosen this locality for your study.

L.113: have you evaluated the role of the chosen velocity model on the robustness of the results presented here? In particular, I am referring to the 39km cluster depth estimate which seems in my understanding exceedingly accurate.

L.150: are there any regions with high topography around this area that are devoid of tremor activity?

L.174: ‘above the locus OF active tremor activity’? missing word?

L.181: Do we have any estimation of the relative amount of mafic versus sedimentary material in the studied region of the Cascades subduction margin? In my understanding, the bulk of the Olympus mountains (and also likely on the Vancouver Island) is constituted by low grade metasediments (Brandon & Calderwood (1990) and references therein) with only minor metavolcanic sequences.

L.183-185: any idea of the plate interface temperature at 39km depth in this part of the SZ? That would be useful to evaluate the likelihood of lithostatic pore fluid pressures and where the main dehydration reactions are expected. The presence of lawsonite in the Brandon & Calderwood (1990) study indicates a low temperature thermal gradient. On the other hand, these authors estimate the underplating depth at 11km, which is quite different from your inferred depth (39km). Any comments on that?

As for the referencing, the concept of having lithostatic pore fluid pressures at these depths was proposed long time before the paper you are referring to in this section. See papers by Richard Sibson and field investigations on an exhumed paleoduplex system in Angiboust et al. (2015, G-cubed).

L.191: there, you may refer to the work from Richter et al. (*Journal of the Geological Society*, 2007, vol. 164, no 1, p. 203-214.) to support this statement.

L.218: Kimura & Ludden is indeed a relevant study to cite. You may also refer to a recent review paper in geosphere focusing on deformation processes in this region (Angiboust et al., 2022).

L.219-221: chlorite and talc are known to flow in a ductile way at very low stress. Plus, if there are such minerals in the Arosa zone, they form only very locally as thin rims around rounded tectonic blocks, due to gradients in chemical composition. So, these chlorite/talc-rich zones (few cms-thick at the maximum) never form interconnected planes able to play a major rheological role on plate interface dynamics at a large scale. Note that talc and chlorite would form after chemical interaction (metasomatism) between a mantle rock and a crustal rock (mafic or sedimentary). If you have no evidence for mantle rocks in these localities, the formation of such Mg-rich phyllosilicates is unlikely. I suggest removing these three lines.

L.255: any clue on the likelihood of only offscrapping topographic highs into the duplex edifice? What would cause a deep-seated basaltic layer (isolated from the upper plate by a variably thick sediment layer) to be incorporated inside the deep duplex? See the paper by Bonnet et al. (2019, geosphere) for a Zagros belt case study where such idea is developed. See also the interesting study by Ueda (2005, tectonics).

L.264: the year of this paper by Menant et al. is 2019 (scientific reports, vol. 9, no 1, p. 9714.), not 2020 where only the question of topographic effect is addressed.

L.268: Any comments on the potential role of recurrent megathrust ruptures on each underplating event? And on the comminution process? Fragmentation is expected along the megathrust plane so the final product should not be viewed only as the result of ductile dismembering of rigid bodies.

L.269: this concept of having an extremely water-overpressurized level was already suggested in a study by van Avendonk et al (2010) in central America (https://doi.org/10.1111/j.1365-246X.2010.04552.x). Such narrow, fluid-saturated level (where cataclasites and hydrofracs are expected) has been documented in the W. Alps in the exhumed record (Angiboust et al., 2015, G-cubed).

L.273: 'ductile': yes, macroscopically ductile. Depending on the thermal gradient (see my question above), it may not be accommodated via crystal plasticity. I am highlighting this point in order to avoid confusion for the reader. At such conditions, pressure solution creep accommodates the bulk part of the slow deformation process (the 'creep' of geophysicists; see the seminal paper by Stöckhert, 2002 https://doi.org/10.1144/GSL.SP.2001.200.01.15).

L.286: compare this estimation with the 400-600m range of topographic uplift associated with each underplating event as shown in Menant et al. (2020).

L.299: if there are high pore fluid pressures, there might be fluid-filled opened cracks. What would be the effect on the seismic signal of such objects? How can you distinguish between distributed porosity and potentially fluid-filled cracks, which is something commonly imaged in exhumed metabasaltic bodies exhumed from this depth? (e.g. Bonnet et al., geosphere, 2019).

Review #2

Recommendation: Revisions Required

Although the behavior of tectonic tremor on large scale is well described, strong uncertainties in tremor location make it difficult to get insight into its smaller scale (10s km, hours—days) dynamics. The low-amplitude and non-impulsive nature of tremor make it especially hard to locate, especially at depth. This work presents results from a detection method that allows to precisely (±300m) locate hypocenters of individual tremor events at depth. The results reveal a complex 3D organization of activity in overriding planar clusters. Combined with geophysical imaging from previous studies and simple but compelling modeling arguments in the manuscript, the authors interpret the spatial distribution of tremor sources as revealing underplating processes under Vancouver Island. They conclude by suggesting that tremor could be evidence of underplating across the Cascadia tremor zone, but also in the Nankai tremor zone.

The results of this work are impressive: the quality of the detection allows to spatially resolve the organization of tremor activity with a precision that is rarely achieved. It confirms that tremor originates from structures that persist over several slow-slip cycles. It allows to precisely estimate characteristic length scales of the tremor process: the thickness of the active zone and perhaps also the thickness of the individual structures (shear zones?) responsible for tremor activity. The interpretations of those structures as symptoms of mafic underplating are reasonable, as evidenced by the sometimes layered, scale-like structures outlined by the activity clusters, and their correlation with strong reflectivity and high Vp/Vs ratios.

Although I strongly recommend this work for publication for the previous reasons, I would also suggest a significant remodeling of the text to make clearer the structure of the argument and the progression from results to interpretation. I did my best to pinpoint points of improvement in the comments, but as a general comment, the progression of the argument often feels rushed and oftentimes lacks clarity. I feel it would be beneficial to go through the whole text and work on linking sections together, and add sentences explicitly concluding what the argument gains from observations, small models, and comparison with previous work.

For making reviewing easier, I would like to kindly ask that the authors include line numbers, upload the figures either in vectorized format or higher resolution, format citations either with links to the bibliography or with complete, in-line names/dates, and upload a complete Supplementary Materials file

— the one I could download was incomplete, but I found a preprint of this article online with a Supplementary File, which gave me an idea of what is in there, assuming it was not modified since.

Finally, to comply with Seismica's data and code availability, I would like to encourage the authors to deposit their catalog(s) into public archives (*e.g.* Zenodo).

Main comments

1) I do not know what the Seismica formatting guide specifies, but I feel that including Materials and Methods between the Introduction and the Results sections would significantly improve the clarity of the argument. It would be clearer how events are detected, and exactly what those are (see comments on Materials and Methods below), and emphasize the role that the detection method has in allowing to resolve the spatial organization of tremor.

2) Especially in the detection section of Materials and Methods, I would also encourage the authors to either use simpler language, make some ellipses and refer to previous work/supplementary material for details, or explain more explicitly the sequences of operations performed.

3) It would be great to discuss what are the events you detect in M&M, Results or Discussion, as they seem to me like they are not exactly large, individual events (LFEs) as can be understood. My understanding is that they are detections of coherent P+S energy within tremor in the tremor that can be localized. In my view, they are symptoms of relatively rapid, localized seismic moment release within a relatively slower, more distributed moment release over the tremor locus that produce the full tremor wave field. This is only my perspective on it, my point is that I would appreciate a paragraph or a few sentences describing what this method ends up detecting in the wave field.

4) The “material transfer” concept is introduced very late in the manuscript, but used several times before. It is not an evident phenomenon and needs either to be defined—what material, transferred how, from/to where—when underplating is first mentioned, or left to be discussed at the end.

5) The last section of the discussion seems to serve as a conclusion, but is very light, offering insufficient closure to the argument. It would be good to have the results spelled out explicitly one last time, and a more substantiated generalization to the full Cascadia ETS zone / other ETS zones in the world.

6) A minor comment on formatting: The size of the section and sub-section titles is quite misleading, the latter being larger than the former. Could you please fix this so that it is easier to orient oneself in the text?

Detailed comments

Abstract:

- “localized areas of material transfer” is not clear to me. In your interpretation, it is shear slip that makes the seismic wave, is that right? In that case, tremor wouldn’t the tremor indicate planes of deformation along the underplaying scales of slab? After reading the rest of the manuscript, it is clearer to me what is meant here, but I believe the “material transfer” needs slightly more introduction here, maybe simply by rephrasing this last sentence.

Results:

Tremor layer thickness:

p4: “The kurtosis of a normal distribution is 3. The observation of values significantly greater than 3 implies abnormally broad tails that probably reflect some seismicity outside a narrow shear zone.” A higher kurtosis also implies a more narrow peak. If seismicity was homogeneously distributed over a band of finite thickness, it would probably have lower kurtosis, as your statistical modeling in the SM shows I believe. Your conclusion about the thickness you deduce is not very clear to me: do you suggest a characteristic thickness of the layer that approaches the spread that you observe? Or a very thin layer with distributed seismicity around? I would suggest you rewrite this paragraph to mention the modeling you have done in the supplementary, and explicitly suggest what its conclusion is. It might also be nice to have a model tremor distribution + location uncertainty that can reproduce the observed kurtosis in the supplementary, in addition to the one that shows what the observed distribution is probably not due to.

A figure showing 1) the distribution of tremor across thickness, and 2) a cartoon of the inferred structure when uncertainties are modeled might be welcome in the main text too, it feels like an important result of this paper.

Spatiotemporal progression and moment estimates:

- p4:"Similar behavior is observed for the ETS episodes in 2003 and 2004 (Supplementary Fig. 5)". It would be useful to have colors indicating the progression of activity in time for those figures too, and perhaps also a (time, strike) plot of the activity like the one you include in Figure 3C. It would help the reader get a sense of the systematic pattern of activation of those patches.

- Figure 2: The detected tremor clusters detected here do not appear in the detections of Rubin & Armbruster 2013 (dot:10.1002/2013GC005031), could you please discuss why?

- p4: "we observe a narrow normal distribution of magnitudes Mw = 1.60 ± 0.1, implying a log normal distribution of scalar moments". Maybe it could also imply that the technique used narrows down the events that can be detected to that characteristic magnitude? As this distribution is so narrow, I feel like it needs to be mentioned that it could simply represent uncertainties in moment estimates, and only partly reflect the real distribution of moments of those events.

Discussion

- Figure 4: "CDP" is an unknown/undefined acronym. Could you please define it?

Mafic underplating model for tremor

- p5: "Localized areas where material transfer is occurring within the subduction zone may manifest the distinct tremor patches as seen in Figs. 2 and 3." It is not clear to me what you mean by "material transfer". You mention that LFEs "represent shear failure within mixed brittle ductile deformation" (a few lines above), wouldn't tremor patches be symptoms of persistent shear zones? What kind of material transfer happen through those? Which material/what transfer?

- p6: "[52] argued that exhumed,", the citation needs to be spelled out. Other instances of this are present throughout the text, make sure to spell out in text citation.

- p6: "Although our estimates of slip within tremorgenic volumes based on Kostrov strain significantly exceed those previously reported for tremor". It is not clear if you are referring to an estimate of slip made in this study (I would have missed it in that case) or in a previous one, maybe [16]. Could you make clear the distinction between contributions of the current study and previous work?

- p6: "we interpret the granular and viscous elements of layer 2A to be associated with less altered tracts of metabasalt surrounded by a more intensely hydrated and overpressured matrix, respectively". So clasts would be approximately the size of the patches? Or within the width of the shear zones indicated by the tremor clusters? It would be good to give an indication of the likely sizes in this model, with respect to the model in [16] but also to the observed sizes of ruptures in paleo subduction zones in the field (e.g. Kotowski & Behr 2019, DOI: 10.1130/GES02037.1).

- p6: "Although our estimates of slip within tremorgenic volumes based on Kostrov strain significantly exceed…" It's not obvious to me if those estimates done in this work or previous work. Could you please specify?

Distance thickness calculation of the E-layer

- p7: "in rough agreement with the thickness of the layer 2A pillow basalts" Are you suggesting the full layer is being eroded?

- p7: What is "the Juan de Fuca plate D"?

- The calculations in this paragraph are very relevant, but it feels to me like you do not conclude anything after doing them. It feels like you actually fall short of explicitly interpreting and actually putting numbers on the "material transfer" referred to in the text previously. It would be interesting to discuss the underplating material *flux* inferred here, and perhaps how it compares to uplift rates, as the authors mention that it correlates well with topography.

Tremor as diagnostic of material transfer

- p7: I would like to understand your view— and maybe see it spelled out here in the discussion/ conclusion— on how your interpretation that tremor is evidence of underplating can be linked with slow slip, and large-scale, along strike migrations of tremor/slow slip. Is the geodetically measured slip partly occurring within the underplating region? Would the long migrations of tremor along-strike indicate waves of quasi-simultaneous underplating across the subduction zone?

- p7: "We suggest that the occurrence of tremor in these environments, as in the deep plate boundary of subduction zones, may be diagnostic of granular and/or material transfer in zones of high pore-fluid pressure". It feels like this goes a step further than what your detections demonstrate. The results and development from interpretations needs to be separated: it seems that you prove that there exists an organization of tremor activity (geometrical, in relationship to geophysical measurements) that indicates an underplating process. The development of the mechanisms in the discussion is absolutely relevant in my opinion, but needs to be clearly stated as interpretative, on the basis of previous work.

Materials and Methods:

- I understand that the authors are using a technique mostly developed in previous studies. However, I believe the reader would really benefit from clear and simple explanation of the mentioned details, and justification of why such and such procedure is applied. The event detection is the central piece here: as it allows to detect individual tremor-related events with unmatched precision, the reader needs to understand what a tremor event is, in terms of what is actually detected.

Detection and Location:

- p7: “splitting parameters that best reduce the S-wave particle motions on the two horizontal coordinates to rectilinear motion isolated to a single channel”. This feels very obscure to me… I am not a seismic detection specialist, but I am personally very interested in how the data is processed — at least in simple terms. Could you either simplify the wording here, or add a bit more details?
- p7: “These quantities are used to normalize…”: it feels like the transition from the last sentence to this one packs a lot, at least a lot that I don’t understand. Could you make it explicit what is used to normalize what, what exactly is the procedure? For instance, from what I understand, the S, P delays are used later, to align the 4+1 stations time stamp.
- p7: “Supplementary Fig. 1” is not in the supplementary, could you please include it?
- p8: “A prospective detection is declared if 2 conditions are met relating to thresholds on the values of a) the 4 possible 3-station delay time circuits (i.e. | t i j +t j k-t i k |), and the mean cross correlation coefficient” I understand the second condition — you should probably add a b) here —, however, the first one is obscure, but seems essential to know what’s an event. I believe you should explain what is the delay time circuit, and why it is useful for detection. Is it because it ensures a coherent arrival time for the considered location?
- p8: “first principal component waveform of the aligned S-waveforms” is used to get a clearer, noisefree picture of what the S-wave is? I think the reason for using it could be mentioned here.
- p8: “We employ several thresholds and statistics (described in detail in the Supplementary Note 1) to cull the tremor catalogue to a maximum of 1 detection per time stamp to emphasize tremor hypocentral patterns but minimize scatter arising from false detections.” I feel like this strikes a good balance of detail and justification of the method.

Editor’s Decision and Comments

This is a well-written and well-organized manuscript with potentially transformative implications for underplating processes in subduction zones. As such the manuscript is appropriate for Seismica and well suited for the Cascadia Special Issue. I am returning the manuscript for revision by the authors to address constructive feedback from the reviewers.

The manuscript was reviewed by one seismologist and one geologist, both are familiar with the type of analysis and/or geologic processes discussed in the paper. Both reviewers commend the technical quality and novelty of the study and support its publication and also provide comments for revisions of the manuscript. Reviewer 1 commented on the potentially transformative implications of this work for tremor as a diagnostic of underplating processes in subduction zones. They also emphasize the need for some further attention and clarification of the methods and conclusions. Reviewer 2 noted that this is a well-written and well-organized manuscript that provides valuable insights into deep crustal processes in subduction zones. This reviewer also raises valid geological concerns about terminology and deformation mechanisms, as well as some modeling assumptions.

Reviewers 1 raised some technical issues with the submission: (i) Indeed the Supplement uploaded to Seismica only contains tables, while that in the preprint on Research Square has numerous figures. Please ensure that the supplementary information provided for with the revised manuscript is complete. (ii) I do note that there are in fact line numbers on the PDF of the manuscript, but since they are quite small the reviewer may have missed these. Regarding Data and Code availability: The figshare link to access the catalog and detection software does not seem to work – please remedy this.

From my own reading I would like to see some more clear discussion on the relationship between the LFE template locations and the respective tremor clusters; this was somewhat confusing to me. Also address a grammar problem on line 223.

Response to Editor's and Reviewer's Comments

Editor

This is a well-written and well-organized manuscript with potentially transformative implications for underplating processes in subduction zones. As such the manuscript is appropriate for Seismica and well suited for the Cascadia Special Issue. Based on reviews I have received, your manuscript may be suitable for publication after some revisions to address constructive feedback from the reviewers.

Thank you for your assessment.

The manuscript was reviewed by one seismologist and one geologist, both are familiar with the type of analysis and/or geologic processes discussed in the paper. Both reviewers commend the technical quality and novelty of the study and support its publication and also provide comments for revisions of the manuscript. Reviewer B comments on the potentially transformative implications of this work for tremor as a diagnostic of underplating processes in subduction zones. They also emphasize the need for some further attention and clarification of the methods and conclusions.

Please note that many of Reviewer B's comments/criticisms stem from the fact that, as stated below, they were not able to access the Seismica manuscript and therefore worked off an earlier preprint originally submitted to Nature Geoscience that was made available (by Nature Geoscience) on the "Research Square Archive". The Nature Geoscience submission was of necessity highly compressed due to word limits and adhered to a different format (e.g., "Data and Methods" occur after the main manuscript as ancillary material). Thus, **please note**, that many of Reviewer B's comments and criticisms had already been addressed in the original Seismica submission.

Reviewer A notes that this is a well-written and well-organized manuscript that provides valuable insights into deep crustal processes in subduction zones. This reviewer also raises valid geological concerns about terminology and deformation mechanisms, as well as some modeling assumptions.

Thank you, we have endeavoured to address reviewer A's concerns.

Reviewers B also raised some technical issues with the submission: (i) Indeed the Supplement uploaded to Seismica only contains tables, while that in the preprint on Research Square has numerous figures. Please ensure that the supplementary information provided for with the revised manuscript is complete.

(ii) I do note that there are in fact line numbers on the PDF of the manuscript, but since they are quite small the reviewer may have missed these. Regarding Data and Code availability: The figshare link to access the catalog and detection software does not seem to work – please remedy this.

See comment above. The figshare link has been reactivated.

From my own reading, I would like to see some more clear discussion on the relationship between the LFE template locations and the respective tremor clusters; this was somewhat confusing to me.

In response to your comment above and that by Reviewer A regarding line 113 in the original manuscript (hereafter e.g., OL 113), we have extensively rearranged and modified the discussion of tremor + LFE template relative vs absolute errors (see in particular lines 152-160 in the revised manuscript; hereafter referred to as e.g., RL 152-160

Also address a grammar problem on line 223.

L 223 (now RL 241-244) etc is written in the subjunctive form and appears to be grammatically correct to us(?). We have expanded and punctuated it slightly, so hopefully it reads more clearly now to the editor:

"Moreover, the coherent, coast-parallel distributions of tremor epicenters along both Cascadia (Wech, 2010) and other warm subduction forearcs (e.g., Obara, 2002; Gallego et al., 2013) seem unlikely were tremor associated with sediments, given the variable sediment inputs and subduction styles along these margins."

Reviewer A

The paper by Littel et al. submitted to Seismica highlights the presence of several clusters of tectonic tremors just above the plate interface in the Cascadias subduction zone. It represents a very interesting and careful case study focusing on the distribution of deep events and their meaning in a regional perspective. I enjoyed reading the paper which is well-written, accessible for non-specialists and well organized. I have been quite impressed (surprised?) by the resolution obtained on such remote processes and by the great level of understanding enabled by the observations shown there. Because I am not a seismologist, I mostly have (minor/moderate) remarks on the deep-seated geological processes and their interpretation.

Thank you for your positive remarks.

Some minor adjustments with bibliographic references is suggested as well. I only have one major comment which is on Figure 11: I am not totally convinced with this mass balance calculation. First, I do not understand where the value of D=8000 km comes from.

The reviewer is correct to be confused; the value of D=8000 km in Figure 11 was due to a transcription error in its preparation. The value should have been D=1800 km (which was the value that was (and is) specified in the text; see RL 305-308) and has been corrected in the revised Figure 11. Note that this value agrees still better with the scaling relation of Scholz, as evident in the revised Figure 11. The chosen value of D=1800 km comes from Clowes et al. (1987); see their table 2. It is based on a careful accounting of subduction in the region over the past 40 Myr. We now make specific reference to their table 2 in our citation.

Second, each of the reflective plane in the E-layer (that should be call E-zone instead, because it is not a layer) corresponds to a sequence of ancient thrust faults that are not active anymore and that have different activity ages. At no time in the history of duplex formation were these planes tectonically active together. This is why I find that including all of these paleo-surfaces at once in this calculation is unjustified.

We do not view the E-layer as a "a sequence of ancient thrust faults that are not active anymore and that have different activity ages." Rather, we assume the E-layer (whose naming was coined many years ago and is now in common use) is a ductile layer as evidenced by the absence of seismicity. In our view, the sequence of reflectors represents shear localizations which can all be active at the same time as observed in simple shear experiments in granular layers (Roy et al., 2021):

Roy, A., Roy, N., Saha, P., & Mandal, N. (2021). Factors determining shear-parallel versus low-angle shear band localization in shear deformations: Laboratory experiments and numerical simulations. *Journal of Geophysical Research: Solid Earth*, 126, e2021JB022578. https://doi.org/10.1029/2021JB022578

We have modified section Section 4.2 to emphasize that we consider deformation within the E-layer to be ductile and ongoing, and not the result of a series of ancient thrust faults corresponding to sequential duplexing; see in particular RL212-215.

Line per line comments:

L.20, L.190 and later: I recommend not using the term 'erosion' that is typically used for the downscrapping of the upper plate crust (tectonic erosion; see papers by Scholl, von Huene, Clift or Vannucchi). I would replace it by 'offscrapping' or 'peeling' or whatever other adequate term, if possible already used in the existing literature. term, if possible already used in the existing literature.

We have replaced references to "erosion" and with either "fragmentation", "underplating" or "off-scraping" dependent on the circumstance.

L.74: around here, a brief section on regional geology and geodynamics would be useful to have, in order to better understand the context and why you have chosen this locality for your study.

The rationale for the selection of study area is stated on RL 66-72 and has less to do with geology and more with this region being the type locality for ETS and having very high SNR tremor and a dense source-receiver coverage between 2003-2005 with which to target the tremor source region.

Surface geology is less important inasfar as the motivation for our work. Geology is however brought in later in the manuscript incidentally where it is relevant to the separation of the 2 tremor patches (by the San Juan fault indicated in Figure 6 that separates the Wrangellia terrane from the Pacific Rim / Leech River terrane (RL 167-171), and in discussion of results from the Olympic Peninsula (Section 4.3).

L.113: have you evaluated the role of the chosen velocity model on the robustness of the results presented here? In particular, I am referring to the 39km cluster depth estimate which seems in my understanding exceedingly accurate.

Since the mapping of reflectivity and the final location of hypocenters of tremor, LFEs and earthquakes in Figure 8 all rely on the same 3D velocity model (Savard et al., 2018), we expect that the relative locations of structure and hypocenters, i.e. their precision, are properly represented. Also note that both the full (≥10 P and S picks) and 4S+1P hypocenters of the LFE templates (053,065,070) lie in close proximity and at depths that lie within those of the northern 3 tremor clusters (Figure 5). This point is made on RL 155-158.

L.150: are there any regions with high topography around this area that are devoid of tremor activity?

As we specify later in the paper (RL 244-247 in the new manuscript) our local-scale documentation of tremor cluster association/dissociation with topography/San Juan Fault on southern Vancouver Island are consistent with previous studies suggesting low tremor density below major faults in Cascadia (Wells et al., 2017) as well as margin-scale association of high amplitude tremor and low recurrence interval ETS with high forearc topography (Bassett & Watts, 2015; Brudzinski & Allen, 2007).

Given the generally peppered distribution of tremor (as in Armbruster et al., 2015, their figure 7), it is clear that not all high topographic elevation areas are currently underlain by tremor but nor is this necessarily surprising because underplating is likely to be a time-dependent process that is integrated over geologic time scales. See revisions on RL 212-216, 297-300.

L.174: ‘above the locus OF active tremor activity’? missing word?

Thank you – we have made the correction.

L.181: Do we have any estimation of the relative amount of mafic versus sedimentary material in the studied region of the Cascades subduction margin? In my understanding, the bulk of the Olympus mountains (and also likely on the Vancouver Island) is constituted by low grade metasediments (Brandon & Calderwood (1990) and references therein) with only minor metavolcanic sequences.

The 3-D traveltime tomography study of Savard et al. (2018) is consistent with the lower crustal material in the tremor study region of southern Vancouver being mafic in origin. Strong positive gravity anomalies over the region are also consistent with high density (i.e. mafic) lower crust (Dehler & Clowes, 1992). High velocities from 2-D refraction profiles in this region also led Clowes et al. (1987; see their Figure 10) to hypothesize the presence of mafic underplating as one of 2 possible interpretations.

As for the Olympic Peninsula rocks exposed *at the surface*, the reviewer is correct that rocks are dominantly low-grade metasediments with only minor regions of basaltic flows. However, these rocks probably included large quantities of frontally accreted sediments that were forced and focused into an embayment formed within the static backstop of the Siletz terrane (see Figure 15 of Brandon et al (1998) or Figure 10a of Richter (2007) that the reviewer cites below). The seismic signature of the E-layer in our Figure 9 that generally parallels LFE occurrence and the inferred top of slab along the SHIPS profile is clearly distinct from the reflectivity signature at shallower depths and so need not reflect the same proportions of mafic and sedimentary compositions seen at the surface.

L.183-185: any idea of the plate interface temperature at 39km depth in this part of the SZ? That would be useful to evaluate the likelihood of lithostatic pore fluid pressures and where the main dehydration reactions are expected. The presence of lawsonite in the Brandon & Calderwood (1990) study indicates a low temperature thermal gradient. On the other hand, these authors estimate the underplating depth at 11km, which is quite different from your inferred depth (39km). Any comments on that? As for the referencing, the concept of having lithostatic pore fluid pressures at these depths was proposed long time before the paper you are referring to in this section. See papers by Richard Sibson and field investigations on an exhumed paleoduplex system in Angiboust et al. (2015, G-cubed).

Brandon and Calderwood (1990) describe very different rocks from the Olympic Complex – low-grade metasediments in the prehnite-pumpellyite facies with estimated T = 190 °C at 11 km depth reflecting conditions at some time in the past. We do not think we need to comment on this aspect in our paper.

Various thermal models of the modern Cascadia subduction zone demonstrate the subducting Juan de Fuca plate and the plate interface are relatively warm (compared to most subduction zones) due to young age of JdF plate, insulating sediments atop the plate, and modest convergence rate. Using Gao and Wang's (2014, *Science*) models, the estimated interface temperature at 39 km depth (P = 1.1 GPa) is~530 °C. These conditions lie in the high-P part of the epidote-amphibolite facies, close to the eclogite facies which lies at slightly higher pressure. The idea that (metamorphic) fluid pressures are lithostatic at ~40 km has been well accepted for decades and predates event Sibson's (1988) work, e.g. Fyfe et al. (1978). In response to the reviewer's comments we have updated this passage at RL 200-205 in the revised manuscript.

L.191: there, you may refer to the work from Richter et al. (Journal of the Geological Society, 2007, vol. 164, no 1, p. 203-214.) to support this statement.

The E-layer documented on various Lithoprobe and SHIPS profiles (Nedimovic et al., 2003) as a fairly regular, slab-parallel layer suggesting laterally continuous development over the margin (as integrated over geologic time). Richter et al 2007 does not seem to advocate such a process and so we have not referenced this work.

L.218: Kimura & Ludden is indeed a relevant study to cite. You may also refer to a recent review paper in geosphere focusing on deformation processes in this region (Angiboust et al., 2022).

Immediately after the Kimura & Ludden reference, we now cite the review paper of Angiboust et al. (2022) for extending the Kimura & Ludden result to a broader range of examples in the geologic record.

L.219-221: chlorite and talc are known to flow in a ductile way at very low stress. Plus, if there are such minerals in the Arosa zone, they form only very locally as thin rims around rounded tectonic blocks, due to gradients in chemical composition. So, these chlorite/talc-rich zones (few cms-thick at the maximum) never form interconnected planes able to play a major rheological role on plate interface dynamics at a large scale. Note that talc and chlorite would form after chemical interaction (metasomatism) between a mantle rock and a crustal rock (mafic or sedimentary). If you have no evidence for mantle rocks in these localities, the formation of such Mg-rich phyllosilicates is unlikely. I suggest removing these three lines.

We have removed the citation to French and Condit (2019) and rewritten this passage to accommodate the reviewer's comments on RL 239-241.

L.255: any clue on the likelihood of only offscrapping topographic highs into the duplex edifice? What would cause a deep-seated basaltic layer (isolated from the upper plate by a variably thick sediment layer) to be incorporated inside the deep duplex? See the paper by Bonnet et al. (2019, geosphere) for a Zagros belt case study where such idea is developed. See also the interesting study by Ueda (2005, tectonics).

We note that the work of Bonnet deals with an accreted seamount. It is difficult to envisage that it would be possible to construct a 4-8 km thick mafic layer with larger downdip extent from episodic seamount accretion. We further note that there are not (currently) many seamounts on the JdF plate.

L.264: the year of this paper by Menant et al. is 2019 (scientific reports, vol. 9, no 1, p. 9714.), not 2020 where only the question of topographic effect is addressed.

Thank you, we have corrected the reference.

L.268: Any comments on the potential role of recurrent megathrust ruptures on each underplating event? And on the comminution process? Fragmentation is expected along the megathrust plane so the final product should not be viewed only as the result of ductile dismembering of rigid bodies.

Please note that in Cascadia megathrust rupture is expected to occur predominantly offshore based on geodetic signatures of plate locking (though there may be minor landward incursion in the westernmost Olympic Peninsula). Assuming that there is no significant megathrust overshoot, we expect tremor and slip to represent the primary deformation process at work in the ETS zone (which is downdip of megathrust rupture and exclusively landward of the coast).

L.269: this concept of having an extremely water-overpressurized level was already suggested in a study by van Avendonk et al (2010) in central America (https://doi.org/10.1111/j.1365-246X.2010.04552.x). Such narrow, fluid-saturated level (where cataclasites and hydrofracs are expected) has been documented in the W. Alps in the exhumed record (Angiboust et al., 2015, G-cubed).

Thank you for drawing our attention to the work by van Avendonk et al (2010) which may indeed manifest reflection from E-layer-type structures. The work of Hyndman (1988) is as far as we know the most focused and detailed treatise of high amplitude reflectivity within deep subduction zone forearcs so we have maintained the original referencing. We now include reference to Angiboust et al. (2015) at line RL 204.

L.273: 'ductile': yes, macroscopically ductile. Depending on the thermal gradient (see my question above), it may not be accommodated via crystal plasticity. I am highlighting this point in order to avoid confusion for the reader. At such conditions, pressure solution creep accommodates the bulk part of the slow deformation process (the 'creep' of geophysicists; see the seminal paper by Stöckhert, 2002 https://doi.org/10.1144/GSL.SP.2001.200.01.15).

We now use "macroscopically ductile" instead of "ductile" to avoid any confusion by the reader.

L.286: compare this estimation with the 400-600m range of topographic uplift associated with each underplating event as shown in Menant et al. (2020).

We are not aware of any observations of pulsed uplift events in Cascadia. Quoting from Menant et al.: “Trench-parallel alternation of forearc highs and depressions along active margins worldwide may reflect temporal snapshots of different stages of these surface oscillations“. Thus northern Cascadia might be a snapshot anywhere in this temporal evolution and we do not feel we are in a position to address this issue.

L.299: if there are high pore fluid pressures, there might be fluid-filled opened cracks. What would be the effect on the seismic signal of such objects? How can you distinguish between distributed porosity and potentially fluid-filled cracks, which is something commonly imaged in exhumed metabasaltic bodies exhumed from this depth? (e.g. Bonnet et al., geosphere, 2019).

The work of Peacock et al (2011) which is cited in the manuscript and deals with deep seated structure in the same region as the current study, interprets the E-layer in terms of fluid filled cracks based on laboratory measurements of Nik Christensen. The reductions in S velocity (and associated elevation in Vp/Vs and Poisson’s ratios are consistent with fluid-filled open cracks. Since this work is already cited we do not see the need to cite it further.

Reviewer B

Although the behavior of tectonic tremor on large scale is well described, strong uncertainties in tremor location make it difficult to get insight into its smaller scale (10s km, hours—days) dynamics. The low- amplitude and non-impulsive nature of tremor make it especially hard to locate, especially at depth. This work presents results from a detection method that allows to precisely (±300m) locate hypocenters of individual tremor events at depth. The results reveal a complex 3D organization of activity in overriding planar clusters. Combined with geophysical imaging from previous studies and simple but compelling modeling arguments in the manuscript, the authors interpret the spatial distribution of tremor sources as revealing underplating processes under Vancouver Island. They conclude by suggesting that tremor could be evidence of underplating across the Cascadia tremor zone, but also in the Nankai tremor zone.

The results of this work are impressive: the quality of the detection allows to spatially resolve the organization of tremor activity with a precision that is rarely achieved. It confirms that tremor originates from structures that persist over several slow-slip cycles. It allows to precisely estimate characteristic length scales of the tremor process: the thickness of the active zone and perhaps also the thickness of the individual structures (shear zones?) responsible for tremor activity. The interpretations of those structures as symptoms of mafic underplating are reasonable, as evidenced by the sometimes layered, scale-like structures outlined by the activity clusters, and their correlation with strong reflectivity and high Vp/Vs ratios.

Thank you for your appraisal of our work.

Although I strongly recommend this work for publication for the previous reasons, I would also suggest a significant remodeling of the text to make clearer the structure of the argument and the progression from results to interpretation. I did my best to pinpoint points of improvement in the comments, but as a general comment, the progression of the argument often feels rushed and oftentimes lacks clarity. I feel it would be beneficial to go through the whole text and work on linking sections together, and add sentences explicitly concluding what the argument gains from observations, small models, and comparison with previous work.

For making reviewing easier, I would like to kindly ask that the authors include line numbers, upload the figures either in vectorized format or higher resolution, format citations either with links to the bibliography or with complete, in-line names/dates, and upload a complete Supplementary Materials file — the one I could download was incomplete, but I found a preprint of this article online with a Supplementary File, which gave me an idea of what is in there, assuming it was not modified since.

*** As a general comment, the reviewer indicates that they worked from the version of our study that resides on the Research Square Archive website (see underlined text above). That version was initially submitted to Nature Geoscience (with strict page limits and specific formatting) and was ultimately redirected to Nature Communications, upon which we decided to withdraw the submission. We then reformatted and expanded the manuscript for consideration by Seismica. As a consequence, many of the criticisms levelled by the reviewer in the lines above and in the review that follows have already been addressed in the original version of the Seismica manuscript.

Finally, to comply with Seismica's data and code availability, I would like to encourage the authors to deposit their catalog(s) into public archives (e.g. Zenodo).

As per our response to the editor, the catalogs have been placed into figshare.

Main comments

1) I do not know what the Seismica formatting guide specifies, but I feel that including Materials and Methods between the Introduction and the Results sections would significantly improve the clarity of the argument. It would be clearer how events are detected, and exactly what those are (see comments on Materials and Methods below), and emphasize the role that the detection method has in allowing to resolve the spatial organization of tremor.

This criticism was addressed in the reformatting and expansion of the study for Seismica – see general comment labelled *** above

2) Especially in the detection section of Materials and Methods, I would also encourage the authors to,either use simpler language, make some ellipses and refer to previous work/supplementary material for details, or explain more explicitly the sequences of operations performed.

The location procedure used in our study is an extension of the method introduced by Armbruster et al (2014 but based on work presented in an EOS abstract in 2010) to locate epicenters for long (150 s) windows, and expanded to short (4 s) windows by Rubin and Armbruster (2013). Our extension involves the use of LFE templates to include P-waves in 4 s windows thereby allowing the location of tremor hypocenters. The merit of the general method is its high relative precision that is provided by use of a minimal number of fixed stations for which highly accurate relative delay times can be determined. We have added this latter point in the Introduction on RL 60-62. We have also expanded on the processing in lines RL 90-99, and provided nominal hypocentral errors on lines RL 158-160.

3) It would be great to discuss what are the events you detect in M&M, Results or Discussion, as they seem to me like they are not exactly large, individual events (LFEs) as can be understood. My understanding is that they are detections of coherent P+S energy within tremor in the tremor that can be localized. In my view, they are symptoms of relatively rapid, localized seismic moment release within a relatively slower, more distributed moment release over the tremor locus that produce the full tremor wave field. This is only my perspective on it, my point is that I would appreciate a paragraph or a few sentences describing what this method ends up detecting in the wave field.

The reviewer is correct that the events we detect (like Rubin and Armbruster (2013) before us) are coherent energy within 4 s windows. Occasionally, these windows may contain more impulsive energy corresponding to individual LFEs, but this is not often the case. We now make this point explicit on RL 120-122 in the revised manuscript.

4) The “material transfer” concept is introduced very late in the manuscript, but used several times before. It is not an evident phenomenon and needs either to be defined—what material, transferred how, from/to where—when underplating is first mentioned, or left to be discussed at the end.

We now define “underplating” as “material transfer from subducting to overriding plate” in the introduction on RL 63-65.

5) The last section of the discussion seems to serve as a conclusion, but is very light, offering insufficient closure to the argument. It would be good to have the results spelled

out explicitly one last time, and a more substantiated generalization to the full Cascadia ETS zone / other ETS zones in the world.

In the Seismica manuscript we have included a succinct and itemized "Conclusions" section (RL 314-347), that was missing in the Nature Geoscience / Research Square Archive submission and addresses the reviewer's criticism.

6) A minor comment on formatting: The size of the section and sub-section titles is quite misleading, the latter being larger than the former. Could you please fix this so that it is easier to orient oneself in the text?

The current manuscript section headings have been implemented using the Seismica LaTeX template, and so this formatting issue has been dealt with in the original submission.

Detailed comments

Abstract:

- "localized areas of material transfer" is not clear to me. In your interpretation, it is shear slip that makes the seismic wave, is that right? In that case, tremor wouldn't the tremor indicate planes of deformation along the underplaying scales of slab? After reading the rest of the manuscript, it is clearer to me what is meant here, but I believe the "material transfer" needs slightly more introduction here, maybe simply by rephrasing this last sentence.

The character allowance for the abstract does not allow definition of "material transfer". Instead, we have defined the term in the introduction.

Results:

Tremor layer thickness:

p4: "The kurtosis of a normal distribution is 3. The observation of values significantly greater than 3 implies abnormally broad tails that probably reflect some seismicity outside a narrow shear zone." A higher kurtosis also implies a more narrow peak. If seismicity was homogeneously distributed over a band of finite thickness, it would probably have lower kurtosis, as your statistical modeling in the SM shows I believe. Your conclusion about the thickness you deduce is not very clear to me: do you suggest a characteristic thickness of the layer that approaches the spread that you observe? Or a very thin layer with distributed seismicity around? I would suggest you rewrite this paragraph to mention the modeling you have done in the supplementary, and explicitly suggest what its conclusion is. It might also be nice to have a model tremor distribution + location uncertainty that can reproduce the observed kurtosis in the supplementary, in addition to the one that shows what the observed distribution is probably not due to. A figure showing 1) the distribution of tremor across thickness, and 2) a cartoon of the inferred structure when uncertainties are modeled might be welcome in the main text too, it feels like an important result of this paper.

The Seismica submission did not include the discussion of kurtosis so this comment is no longer relevant, nor do we regard this issue as within the scope of the current manuscript.

Spatiotemporal progression and moment estimates:

- p4:"Similar behavior is observed for the ETS episodes in 2003 and 2004 (Supplementary Fig. 5)". It would be useful to have colors indicating the progression of activity in time for those figures too, and perhaps also a (time, strike) plot of the activity like the one you include in Figure 3C. It would help the

reader get a sense of the systematic pattern of activation of those patches.

We have updated the relevant Seismica submission figure (Figure 3) a requested. Note that in the new plots in Figure 3a,b,c the entire time interval for each year is plotted not just a single day in contrast to Figure 6, since the purpose of the figure is to display the full 4S and 4S+1P data sets.

- Figure 2: The detected tremor clusters detected here do not appear in the detections of Rubin & Armbruster 2013 (dot:10.1002/2013GC005031), could you please discuss why?

Rubin & Armbruster 2013 chose to look at the epicentral rupture propagation patterns of a different, larger cluster which lies to the south of our focus area. Our area is better positioned for purposes of hypocentral (depth) location because it lies more nearly directly below our westernmost stations KLNB/KELB.

- p4: "we observe a narrow normal distribution of magnitudes Mw = 1.60 ± 0.1, implying a log normal distribution of scalar moments". Maybe it could also imply that the technique used narrows down the events that can be detected to that characteristic magnitude? As this distribution is so narrow, I feel like it needs to be mentioned that it could simply represent uncertainties in moment estimates, and only partly reflect the real distribution of moments of those events.

We have included the reference to Sammis & Bostock (2021) on which the magnitude proxy scale is based. The moment distribution here is similar to that reported in Sammis and Bostock, where the ETS magnitude distribution and its significance are discussed in more detail (see their sections 2.2 and 2.3). The reader can therefore refer to that publication for further information. The main point here is that tremor detections obey a magnitude scaling relation similar to LFEs.

Discussion

- Figure 4: “CDP” is an unknown/undefined acronym. Could you please define it?

CDP stands for “Common Depth Point” and is now defined in the caption to figure 4.

Mafic underplating model for tremor

- p5: “Localized areas where material transfer is occurring within the subduction zone may manifest the distinct tremor patches as seen in Figs. 2 and 3.” It is not clear to me what you mean by “material transfer”. You mention that LFEs “represent shear failure within mixed brittle ductile deformation” (a few lines above), wouldn’t tremor patches be symptoms of persistent shear zones? What kind of material transfer happen through those? Which material/what transfer?

As already indicated in Reviewer B’s main point 4), we now indicate what is meant by material transfer in the Introduction section (RL 63-64), namely that it refers to underplating as transfer of upper crustal material from the subducting plate to the overriding plate.

- p6: “[52] argued that exhumed,”, the citation needs to be spelled out. Other instances of this are present throughout the text, make sure to spell out in text citation.

The authors of this reference, namely Kimura and Ludden, are now explicitly spelled out as per Seismica’s formatting guidelines on RL 238

- p6: “Although our estimates of slip within tremorgenic volumes based on Kostrov strain significantly exceed those previously reported for tremor”. It is not clear if you are referring to an estimate of slip made in this study (I would have missed it in that case) or in a previous one, maybe [16]. Could you make clear the distinction between contributions of the current study and previous work?

Thank you for noting this ambiguity; we know specifically indicate that these estimates were made by Sammis and Bostock (2021), on RL 292-293.

- p6: “we interpret the granular and viscous elements of layer 2A to be associated with less altered tracts of metabasalt surrounded by a more intensely hydrated and overpressured matrix, respectively”. So clasts would be approximately the size of the patches? Or within the width of the shear zones indicated by the tremor clusters? It would be good to give an indication of the likely sizes in this model, with respect to the model in [16] but also to the observed sizes of ruptures in paleo subduction zones in the field (e.g. Kotowski & Behr 2019, DOI: 10.1130/GES02037.1).

We now specify the order of slip-surface dimension to be 100 m and refer to Sammis and Bostock (2021) for this estimate (see RL 277)

Regarding “observed sizes of ruptures in paleo subduction zones in the field” we are skeptical that those observations reflect the actual slip surfaces. Exhumed material in paleo subduction zones is usually considered by geologists to be brought to the surface along a low viscosity “subduction channel”. A likely candidate for such a channel is the E-layer. As such exhumed material is more likely to represent material from the E-layer in which case it is unlikely that original tremor/LFE slip surfaces are evident due to extensive viscous-deformational reworking. We prefer not to raise this argument in the present work.

- p6: “Although our estimates of slip within tremorgenic volumes based on Kostrov strain significantly exceed…” It’s not obvious to me if those estimates done in this work or previous work. Could you please specify?

This point has already been addressed 2 points above.

Distance thickness calculation of the E-layer

- p7: “in rough agreement with the thickness of the layer 2A pillow basalts” Are you suggesting the full layer is being eroded?

Yes, locally we are suggesting that the full layer 2A is being fragmented, sheared and underplated to the overriding plate.

- p7: What is “the Juan de Fuca plate D”?

The “D” was a typographical error in the archived version of the manuscript which is not present in the Seismica submission.

- The calculations in this paragraph are very relevant, but it feels to me like you do not conclude anything after doing them. It feels like you actually fall short of explicitly interpreting and actually putting numbers on the “material transfer” referred to in the text previously. It would be interesting to discuss the underplating material flux inferred here, and perhaps how it compares to uplift rates, as the authors mention that it correlates well with topography.

Please see response to Reviewer A regarding their point concerning L 286 and uplift. The calculations Reviewer B is referring to are now included in section 4.6 and their purpose is to demonstrate that E-layer thickness below southern Vancouver Island is commensurate with fault thickness vs fault displacement scaling observed over a broad range of scales. While the reviewer’s suggestion is an interesting one, we consider it as falling outside of the scope of the current work.

Tremor as diagnostic of material transfer

- p7: I would like to understand your view— and maybe see it spelled out here in the discussion/ conclusion— on how your interpretation that tremor is evidence of underplating can be linked with slow slip, and large-scale, along strike migrations of tremor/slow slip. Is the geodetically measured slip partly occurring within the underplating region? Would the long migrations of tremor along-strike indicate waves of quasi-simultaneous underplating across the subduction zone?

This is a good question. In the Seismica manuscript (section 4.5) we expand on what is written in the Research Archive manuscript on how we view tremor and underplating is related to slow slip. In particular, RL 295-300 now read:

“It is likely then that the slow slip of ETS represents ductile shear persisting well into the E-layer at diminishing levels, both where tremor is well expressed and where it is not (e.g., Wech and Bartlow, 2014). The sparse distribution of tremor sources over the broader region when imaged at high resolution (see Armbruster et al., 2014, their Figure 7) suggests that the large areas of fault zone surrounding tremorgenic patches are aseismic because creep is not (currently) inhibited by stuck asperities (i.e. granular jams) associated with material transfer.”

- p7: “We suggest that the occurrence of tremor in these environments, as in the deep plate boundary of subduction zones, may be diagnostic of granular and/or material transfer in zones of high pore-fluid pressure”. It feels like this goes a step further than what your detections demonstrate. The results and development from interpretations needs to be separated: it seems that you prove that there exists an organization of tremor activity (geometrical, in relationship to geophysical measurements) that indicates an underplating process. The development of the mechanisms in the discussion is absolutely relevant in my opinion, but needs to be clearly stated as interpretative, on the basis of previous work.

We have laid out the case for an association between tremor and underplating in southern Vancouver Island as summarized and enumerated in the Conclusions section of the Seismica manuscript (which was not present in the Research Square Archive website and Reviewer B did not access). We present this as a hypothesis (see rewording of point 6 in the Conclusion section) that is supported by several lines of evidence and which can serve as the basis for future studies in other subduction zones and plate boundaries where tremor has been identified.

As far as the extension of this hypothesis to other plate boundary scenarios is concerned (point 7 in the Conclusions), an close examination of tectonic tremor in e.g. southern Vancouver Island and that in Parkfield California reveals that the character of the waveforms in these two disparate environments is essentially indistinguishable and that a common mechanism (be it material transfer across a plate boundary shear zone as proposed here or otherwise) is highly likely and requires no great leap of faith.

Thus we feel that point 7 of the Conclusions is logical and well founded.

Materials and Methods:

- I understand that the authors are using a technique mostly developed in previous studies. However, I believe the reader would really benefit from clear and simple explanation of the mentioned details, and justification of why such and such procedure is applied. The event detection is the central piece here: as it allows to detect individual tremor-related events with unmatched precision, the reader needs to understand what a tremor event is, in terms of what is actually detected.

Please see below regarding significant revision of Data and Methods section.

Detection and Location:

- p7: “splitting parameters that best reduce the S-wave particle motions on the two horizontal coordinates to rectilinear motion isolated to a single channel”. This feels very obscure to me… I am not a seismic detection specialist, but I am personally very interested in how the data is processed — at least in simple terms. Could you either simplify the wording here, or add a bit more details?

We have expanded on the description of the splitting correction normalization and other elements of the signal processing, please see RL 90-100.

- p7: “These quantities are used to normalize…”: it feels like the transition from the last sentence to this one packs a lot, at least a lot that I don’t understand. Could you make it explicit what is used to normalize what, what exactly is the procedure? For instance, from what I understand, the S, P delays are used later, to align the 4+1 stations time stamp.

We have expanded on temporal normalization of S and P channels, please see RL 90-100

- p7: “Supplementary Fig. 1” is not in the supplementary, could you please include it?

This issue again appears to be due to the differences between the Research Square Archive manuscript and the manuscript submitted to Seismica.

- p8: “A prospective detection is declared if 2 conditions are met relating to thresholds on the values of a) the 4 possible 3-station delay time circuits (i.e. | t i j +t j k-t i k |), and the mean cross correlation coefficient” I understand the second condition — you should probably add a b) here —, however, the first one is obscure, but seems essential to know what’s an event. I believe you should explain what is the delay time circuit, and why it is useful for detection. Is it because it ensures a coherent arrival time for the considered location?

Thank you for catching the missing “b)” which we have now included.

We now include reference to VanDecar & Crosson (1990) for details regarding the traveltime circuit constraint.

- p8: “first principal component waveform of the aligned S-waveforms” is used to get a clearer, noise free picture of what the S-wave is? I think the reason for using it could be mentioned here.

Yes, you are correct and we have revised this passage, accordingly. See RL 109-110.

- p8: “We employ several thresholds and statistics (described in detail in the Supplementary Note 1) to cull the tremor catalogue to a maximum of 1 detection per time stamp to emphasize tremor hypocentral patterns but minimize scatter arising from false detections.” I feel like this strikes a good balance of detail and justification of the method.

Thank you.

Version 2: Revised Manuscript

Review #1

Recommendation: See Comments
dear editor

The answers are very brief, sometimes even too short. Nothing was added on the presentation of the regional geological context. Well... these were just suggestions - I let the authors present their work however they like. Overall, these very minor changes do not impact the quality of this very good article. A final word: I find the idea that tremors accompany the comminution of the upper part of the slab very interesting. In this sense, the fracturing process is probably dominant and the deformation faithfully described by a brittle creep law (Brantut et al 2012). The corresponding rock type forming under these conditions would then be foliated cataclasites. This is clearly explained in the article from Oncken et al., 2022 (geosphere) and documented in the Central Alps suture zone. This is not a fundamental point of the paper but refering to this concept would confirm that the authors's intuition is independantly confirmed by natural data. One last remark: L.241, the year of the Angiboust et al paper is not 2015 but 2022 instead (they refer to the geosphere review paper).

Reviewer #2

Recommendation: Accept Submission
First, I would like to apologize for working off the wrong version in my first review, I’m not exactly sure what happened. Hopefully that’s not the case this time.

The author's satisfactorily addressed all my questions and comments. Their modifications, along with the arguments developed Seismica version of the manuscript addressed my concerns. I appreciate the authors' effort of being more specific in the Materials and Methods. This version feels more approachable for the non-specialist. This version of the manuscript also clearly and extensively interprets the tremor catalog. It conveys the results and their implications in a much more effective manner than the previous version I had read. Finally, the conclusion clearly sums up the conclusion of the study.

Except for the two following minor comments, I do not have any substantial criticism on the manuscript in this current state. This work will be an exciting new contribution to the literature and I recommend its publication after consideration of the following minor comments.

Minor comments:

From the response file:

"We have included the reference to Sammis & Bostock (2021) on which the magnitude proxy scale is based. The moment distribution here is similar to that reported in Sammis and Bostock, where the ETS magnitude distribution and its significance are discussed in more detail (see their sections 2.2 and 2.3). The reader can therefore refer to that publication for further information. The main point here is that tremor detections obey a magnitude scaling relation similar to LFEs."

I understand that the analyses in Sammis & Bostock (2021) and previously in Bostock et al. (2015) show a log-normal distribution of seismic moments of LFEs, as presented in the manuscript here. The former also proposes a model to account for this log-normal distribution. However, it feels like this very thin distribution of moments might be a result of some kind of bias emerging from the observation techniques employed here (*e.g.* 4-s time windows), which might limit the size of the events that can be detected. In that case, the observational noise/error would be responsible for the distribution observed here. In other words, can the method used here yield a wider distribution? Could you address this possibility in the text?

Figures:

It would help the reader to label all figure panels consistently using a), b), c) etc (*e.g.* figs 2, 4, 5, 8).

Figure 4 and 5 feel redundant: maybe you could add the Savard hypocenters and the UTM-Y profile of figure 5 to figure 4?

Figure 8: "CDP" is used here but defined in figure 9, could you define it here or spell it out more explicitly on the axes?

From the Data & Code availability section of the manuscript:

"Codes (in Python) used to generate the tremor detection catalog are provided in the figshare repository with the same URL."

I have not been able to find the code on the provided figshare url for the data sets, is it on the same page?

Editor Decision

Accept manuscript

Response to Reviewers' Comments

Dear Professor Hooft, Please find our response to the reviewers' comments in red text below.

Reviewer 1

The answers are very brief, sometimes even too short. Nothing was added on the presentation of the regional geological context. Well... these were just suggestions - I let the authors present their work however they like. Overall, these very minor changes do not impact the quality of this very good article.

A final word: I find the idea that tremors accompany the comminution of the upper part of the slab very interesting. In this sense, the fracturing process is probably dominant and the deformation faithfully described by a brittle creep law (Brantut et al 2012). The corresponding rock type forming under these conditions would then be foliated cataclasites. This is clearly explained in the article from Oncken et al., 2022 (geosphere) and documented in the Central Alps suture zone. This is not a fundamental point of the paper but refering to this concept would confirm that the authors's intuition is independantly confirmed by natural data.

We agree that the work by Oncken et al deserves comparison with the model we are proposing and so have included an extra sentence (***in bold italic***) in the passage that starts at line 289 to read:

"As comminution proceeds, we expect increasing shear strain, ductile deformation, and gradual material transfer/transformation to the E-layer because of decreased density and strength imparted by the release of fluids. ***The dominant rock types manifest in this process would be foliated cataclasites transitioning to mylonites, as has been documented for the inferred plate boundary of the Central Alps suture zone (Oncken et al 2022)****."*

One last remark: L.241, the year of the Angiboust et al paper is not 2015 but 2022 instead (they refer to the geosphere review paper).
We have corrected this mis-attribution.

Reviewer 2

Review #2 of *Tectonic tremor: the chatter of mafic underplating*

First, I would like to apologize for working o] the wrong version in my first review, I'm not exactly sure what happened. Hopefully that's not the case this time.

The author's satisfactorily addressed all my questions and comments. Their modifications, along with the arguments developed Seismica version of the manuscript addressed my concerns. I appreciate the authors' e]ort of being more specific in the Materials and Methods. This version feels more approachable for the non-specialist. This version of the manuscript also clearly and extensively interprets the tremor catalog. It conveys the results and their implications in a much more e]ective manner than the previous version I had read. Finally, the conclusion clearly sums up the conclusion of the study.

Except for the two following minor comments, I do not have any substantial criticism on the manuscript in this current state. This work will be an exciting new contribution to the literature and I recommend its publication after consideration of the following minor comments.

Minor comments:

From the response file:

"We have included the reference to Sammis & Bostock (2021) on which the magnitude proxy scale is based. The moment distribution here is similar to that reported in Sammis and Bostock, where the ETS magnitude distribution and its significance are discussed in more detail (see their sections 2.2 and 2.3). The reader can therefore refer to that publication for further information. The main point here is that tremor detections obey a magnitude scaling relation similar to LFEs."

I understand that the analyses in Sammis & Bostock (2021) and previously in Bostock et al. (2015) show a log-normal distribution of seismic moments of LFEs, as presented in the manuscript here. The former also proposes a model to account for this log-normal distribution. However, it feels like this very thin distribution of moments might be a result of some kind of bias emerging from the observation techniques employed here (*e.g.* 4-s time windows), which might limit the size of the events that can be detected. In that case, the observational noise/error would be responsible for the distribution observed here. In other words, can the method used here yield a wider distribution? Could you address this possibility in the text?

On line 274, we now provide the reader with more explicit guidance on the issue of detection bias is discussed in detail by Sammis and Bostock (2021). The added text is reproduced below in bold italic font. "We will assume that these log-normal distributions are not significantly influenced by catalog incompleteness. Although Sammis and Bostock (2021) were not able to definitively exclude the possibility of detection bias, they did provide evidence supporting catalog completeness based on independent observations of Supino et al. (2020, 2021) and minor diNerences in nighttime vs daytime detections (see section 2.3 of Sammis and Bostock, 2021)."

Figures:

It would help the reader to label all figure panels consistently using a), b), c) etc (*e.g.* figs 2, 4, 5, 8). We have reorganized the figures so that all are now consistently labelled.

Figure 4 and 5 feel redundant: maybe you could add the Savard hypocenters and the UTM-Y profile of figure 5 to figure 4?

After discussion across co-authors, we feel it is important to keep figure 5 separate from figure 4 so that the reader can clearly di]erentiate the two estimates of LFE template hypocenters.

Figure 8: "CDP" is used here but defined in figure 9, could you define it here or spell it out more explicitly on the axes?

Thanks to reviewer 2 for noting this. We now define CDP at its first occurrence in Figure 8 (caption), rather than in Figure 9.

From the Data & Code availability section of the manuscript:

"Codes (in Python) used to generate the tremor detection catalog are provided in the figshare repository with the same URL."

I have not been able to find the code on the provided figshare url for the data sets, is it on the same page?

We are submitting the python codes to the figshare site (only Geena has the website permissions and will do so in the next day or 2). The site is: https://doi.org/10.6084/m9.figshare.c.7947842.v1