Response to Reviewers' Comments

We thank the associate editor and reviewers for their comments, which have helped to improve the manuscript. The reviewers' comments are shown in black text, and our responses are shown in red text. Line numbers refer to the revised manuscript.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

We have acknowledged the reviewers in the Acknowledgments section.

<u>Reviewer A:</u>

Review of Nuyen and Schmidt, 2023, "Along-strike changes in ETS behavior near the slab edge of Southern Cascadia"

This paper performs a systematic study of ETS events in the southernmost Cascadia Subduction Zone (CSZ) from 2016 to 2022 to characterize along-strike variations in slip character. This region in space/period in time has not been previously studied in a systematic fashion. The GNSS time series processing for ETS offsets relies on an algorithm with a combination of standard steps: the algorithm will stack GNSS stations, identify regions and times of tremor, and extract the ETS timing information for further study. 19 events are identified. A cumulative slip plot of all ETS events is produced for southern Cascadia and it shows that 85% of the plate convergence rate is accommodated in ETS events. The cessation of ETS 50 km north of the slab edge may be due to thermal effects on the frictional or hydrological fault properties. The authors discuss literature-based thermal models of the MTJ area, which should increase the temperature of the slab near its southern margin, as an explanation for the cessation of ETS events at the place where it is observed.

This work is relevant and generally well-constructed, but I suggest some major revisions before it is ready to be accepted.

My biggest concern with the interpretation is just that the plate interface might be wrong, either in depth or in lateral extent. The paper bases all its conclusions on the specific assumed fault geometry. What about other plate interfaces? Line 539-542 mentions this concern. I think a comparison of different geometries should be more pronounced in the paper and certainly higher than Figure S24. Understanding the point of the paper does require this figure, and there's room in the main text. I think that some of the figures could be denser and make room for this important part of the story. For example, Figure

3 could be condensed then Figures 2 and 3 could be combined, or there are other options. The authors' reasons for choosing a preferred fault model are valid, and could even be annotated in the figure with "(preferred)" or something.

We agree that it would be worthwhile to see how our results change with different fault geometries and have calculated distributed slip models for several ETS events using the Slab2 model. Results for the June 2020 and August 2016 ETS events are shown below and are now included in the supplement. We chose to model the June 2020 event so that we could provide a comparison for Figure 5 in the manuscript and because it represents one of the most southernmost ETS events in the catalog. Additionally, we chose to model the August 2016 event because it has a large spatial extent and provides insights into how the two resulting models differ along strike. Overall, we find that the Slab2 model does a similar job of fitting the GNSS data and results in similar spatial distributions of slip. The main differences when using the Slab2 versus the McCrory model are that Slab2 produces slightly smaller slip magnitudes and places slip deeper on the megathrust in some locations. Therefore, the Slab2 results generally suggest that ETS accommodates less strain and occurs at deeper depths in southern Cascadia than the McCrory et al. (2012) results. However, the results are consistent in that both slab models produce little to no slip associated with ETS events occurring near the southernmost lateral edge of the Gorda slab. Therefore, we believe that this finding is independent of the fault model used in the inversion. We have added text in Sections 3.2 (lines 394-402), 4 (lines 416-424), and 6.1 (lines 582-591) that acknowledges other slab models in Cascadia and discusses the results using the Slab2 model. We have also added Figure S24 (as Figure 8) to the main text and added the two figures below to the Supplement (Figures S22 and S23).

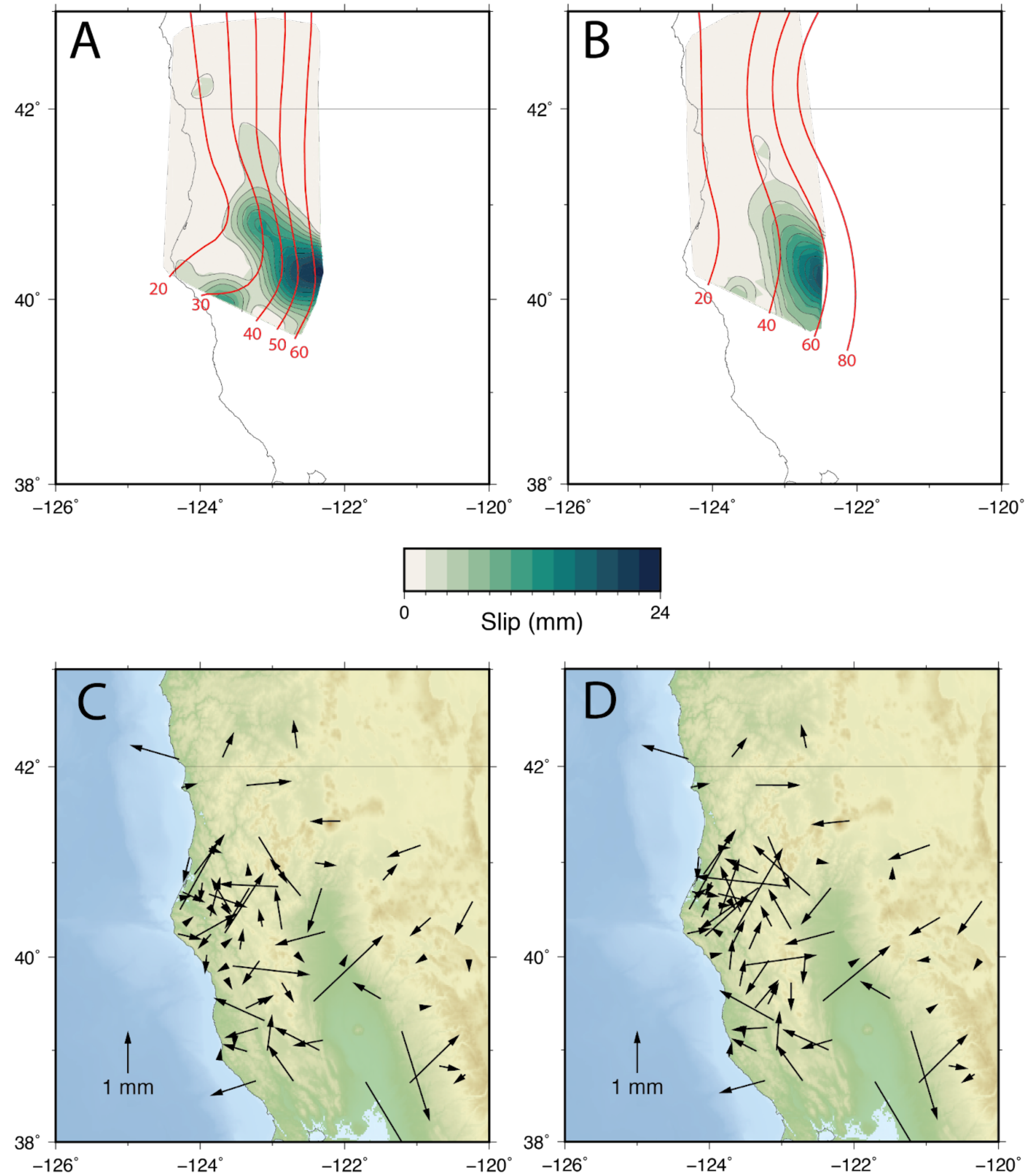

Figure S22. Comparison of slip model results for ETS Event 15 (June 2020) using different slab models. **A** Distributed slip model using McCrory et al. (2012) model with red lines delineating slab contours and black lines with green shading outlining every 2 mm of fault slip. **B** Distributed slip model using Slab2 model from Hayes (2018) with red lines delineating slab contours and black lines with green shading outlining every 2 mm of fault slip. **C** Vector map showing the misfit between the observed and predicted surface displacements resulting from the slip model in panel A. **D** Vector map showing the misfit between the observed and predicted surface displacements resulting from the slip model in panel B.

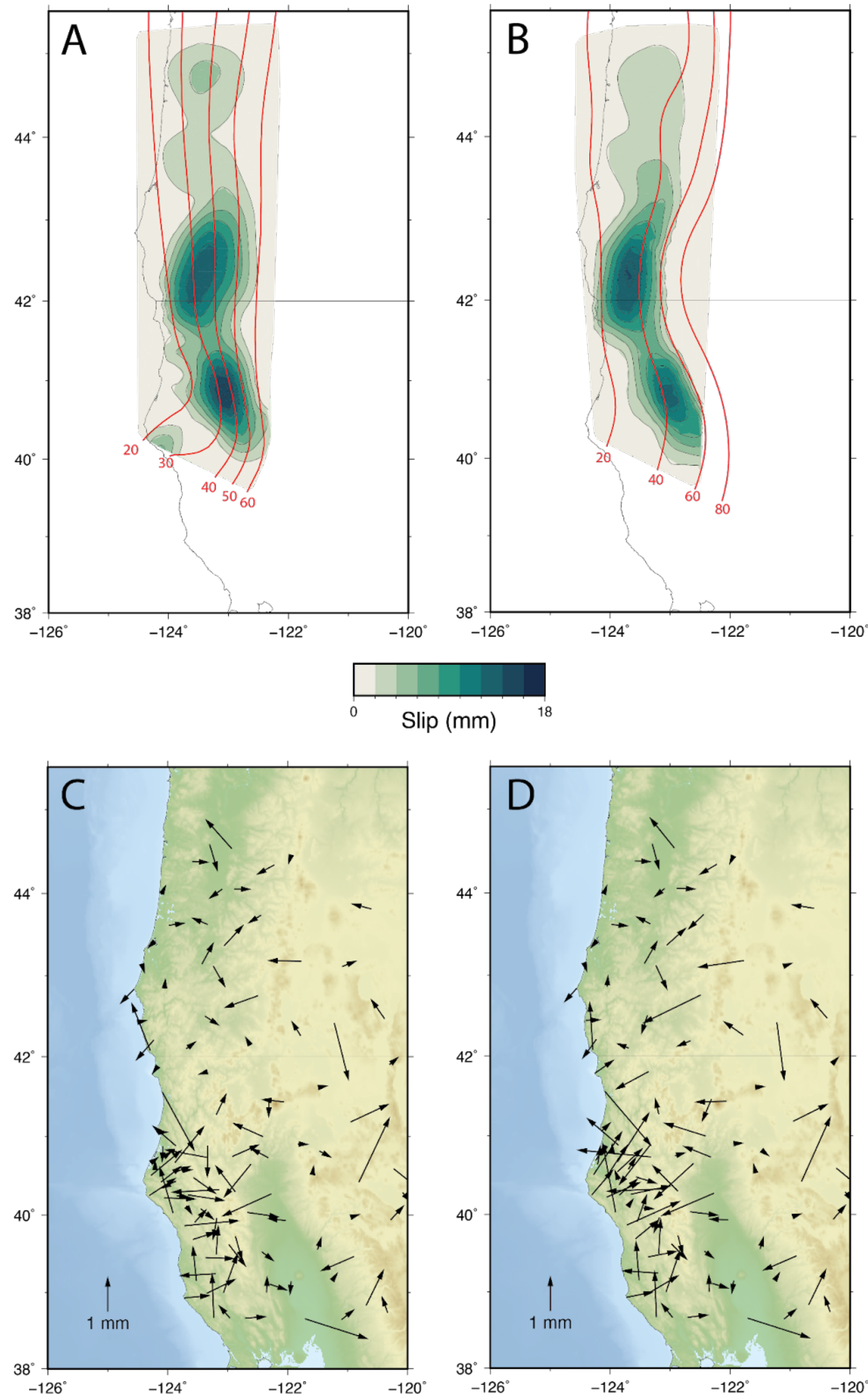

Figure S23. Comparison of distributed slip model results for ETS Event 1 (August 2016) using different slab models. **A** Distributed slip model using McCrory et al. (2012) model with red lines delineating slab contours and black lines with green shading outlining every 2 mm of fault slip. **B** Distributed slip model using Slab2 model from Hayes (2018) with red lines delineating slab contours and black lines with green shading outlining every 2 mm of fault slip. **C** Vector map showing the misfit between the observed and predicted surface displacements resulting from the slip model in panel A. **D** Vector map showing the misfit between the observed and predicted surface displacements resulting from the slip model in panel B.

I was also concerned about the treatment of GNSS uncertainty. The uncertainty in line 372 is probably wrong in several ways. 1) Because a displacement is the difference of the before-and-after positions, the uncertainty of the offset should be the uncertainty of each position added in quadrature. This would tend to raise the uncertainty even more. But on the other hand, 2) Each term in the offset is the average over several days, which will make the uncertainty of each side smaller. I'm not sure this what overall difference this would make in the results. More generally I'm concerned about uncertainties being determined non-rigorously, and then the data being over-fit anyway in Figure 5. What is the chi-squared of each inversion and how do you justify it being over-fit? I'm not suggesting adding error ellipses to every vector plot, because that would be visually overwhelming, but the size of the typical error ellipse in Figure 5d does concern me for over-fitting.

We recognize that the typical method for combining uncertainties involves summing the individual uncertainties in quadrature when the data are independent. However, as mentioned by the reviewer, this method results in uncertainties that are inflated. We felt that this would overestimate the offset uncertainty. Therefore, we opted to use the average approach, as we felt it was a better representation of the uncertainty when using average positions for the offset calculation.

We have calculated the chi-squared values for each inversion and added them to Table S24 in the Supplement for the reader to assess the fit. As you can see in the table, while some inversions have a chi-squared value at or above 1, the majority are just below 1, which would suggest slight overfitting. For the table, we chose to use the chi-squared statistic (i.e. the sum is divided by the number of observations) instead of the reduced chi-squared statistic (i.e. sum is divided by the degrees of freedom). If we had used the reduced chi-squared statistic, our lower values would increase and be closer to 1. However, the degrees of freedom in our inversions are somewhat arbitrary given that spatial smoothing causes neighboring fault elements to be correlated, so we opted to use the chi-squared statistic.

Additionally, we do not use a weighted inversion for our distributed slip models, so these models are not sensitive to exact uncertainties. These uncertainties were only used to constrain the depth limits of our fault model, to assess the relative uncertainty between GNSS stations, and to choose the smoothing weight.

Minor points:

Line 124-125: I think Cape Mendocino and Mendocino Fracture Zone are not labeled on any maps, so a person unfamiliar with the area would not be able to place them.

We have added a label for the Mendocino Fracture Zone, but have not labeled Cape Mendocino to avoid making the figure too busy.

Line 342: This event has displacements to the south but not tremor. It's just mentioned off-hand in the caption, but I think it's one of the most interesting points. Do you have an explanation for this?

As we mentioned in Section 3.1, the tremor monitoring system at the PNSN was updated in 2017 and resulted in more accurate tremor detection in southern Cascadia for later events. Therefore, we suspect that this event (which occurred in August 2016 before the PNSN updates) likely exhibited tremor to the south, but it was not detected by the network. We have added an additional sentence to the caption on lines 342-343 to clarify this.

Line 373: fault slip being zero at the perimeter – does that apply to the bottom edge? Why is the slip inversion in Figure 5 so concentrated along the down-dips edge?

As we mentioned in the following sentence, we do not force slip to be zero along the edges of the slab model south of 40.6° N latitude. Specifically, slip is allowed on both the downdip and lateral edges of the fault. We have added 'down-dip and lateral' on lines 376-377 to make this clearer. This means that slip is allowed along the entire southern edge of the slab model, and on the updip and downdip edges of the slab model that are located south of 40.6° N latitude. Therefore, the slip in Figure 5B is consistent with slip on the edges of the fault model south of 40.6° N latitude.

Throughout: "below 42°N" should really be "south of 42°N" and similar. I've been corrected on this semantic distinction before.

We have changed all instances of "below" to "south of".

Line 482: I think a comparison of slab modeled is warranted earlier than Figure S24.

We have added text in Sections 3.2 (lines 394-402), 4 (lines 416-424), and 6.1 (lines 582-591) that acknowledges other slab models in Cascadia and discusses the results using the Slab2 model. We have also added Figure S24 to the main text as Figure 8.

Line 625: Can you explain why you'd expect shear strength heterogeneity to be a barrier to rupture? It feels like one logical step is missing from this chain.

Shear strength heterogeneity along a fault leads to localized areas with relatively high shear strength. In a given slip event, regions with lower shear strength may slip, but the dynamic stress perturbations induced by the rupture propagation may not be able to overcome the resistance to failure at these localized regions of relatively high shear strength. As a result, these regions will act as natural barriers to sip propagation (see further explanations by Wang and Bilek (Tectoniphysics, 2014)). We have added a sentence on lines 690-691 to clarify.

Line 713: can you be clearer about what deformation pattern you're hypothesizing? if seismic deformation does not take place and ETS does not take place, do you expect constant, velocity-strengthening stable sliding?

We expect the deformation to be aseismic, although fault slip could experience transient behavior at low rupture velocities. We have altered the text on line 793 to indicate that we expect aseismic deformation.

Authors probably should explain the acronyms in the acknowledgements, and PBO is now Earthscope.

We have explained the acronyms and substituted EarthScope for PBO.

Figures:

Figure 1: Please include a legend for the red/yellow/green/cyan dots, the scale of the plate motion vectors.

We have added a legend, plate motion vector scale, and km scale bar to Figure 1.

Please check there is a km scale bar for each map figure.

We have added scale bars to all relevant figures.

Figure 5: Please put the outline of the slab from B onto the maps in A, C, and D. It would help with the context of tremor vs. slow slip regions.

We have added the outline of the slab model to panels A, C, and D in Figure 5.

Line 447 Figure 6: You could just label the contours like the other plots and not include this line.

We have labeled the depth contours and edited the caption for Figure 6.

Figure 7: This figure would also benefit from a legend. If someone were to take a screenshot of this figure and place it in a presentation or review paper about ETS… they would need a lot of contextual information in text unless there's a legend. The legend doesn't have to describe 100% of the symbols all by itself, but even just a few would help.

We have added a legend to Figure 7.

Figure 8: Can you label the seismogenic zone?

We had added a label for the seismogenic zone in Figure 8.

Recommendation: Revisions Required

Reviewer B:

The paper “Along-strike changes in ETS behavior near the slab edge of Southern Cascadia” by Nuyen and Schmidt is a nice piece of literature that outlines how ETS and tremor distribution seems to change as the southern edge of the Cascadia slab is reached beneath northernmost California. This paper performs geodetic modeling of numerous slow slip events (as constrained by the PNSN tremor catalog) to understand how slip magnitude and distribution changes around a bend in the southern Cascadia slab in general, as well as in the co-ETS and inter-ETS periods. They attribute spatial variations into the thermal structure of the margin with contributions from complex slab geometry.

There are 2 major comments I have about the analysis and interpretations in this paper that I strongly suggest should be addressed before publication

I think this paper would benefit from a more thorough approach into how their results would change for different slab models in the area. For instance, they mention that the Delph et al 2021 plate model has a different dip and depth than this model (L485). I am curious how the use of a model with a slab at shallow depths and lower dips might affect the slip magnitudes, and therefore the magnitude estimates for SSEs and depths over which tremor/ETS occurs? This could possibly constrain the likelihood of some of the plausible mechanisms described in the discussion, as well as illuminate how much slip may be accommodated seismically vs. aseismically. It seems within the scope of this study to model slip with other slab model, as slip amplitudes and depth distributions appear to be a significant portion of the discussion. Perhaps it would also be enlightening to model with Slab2, as it is quite different from the Blair/McCrory models and Delph models (which is derived from McCrory). A comparison of the misfits to the geodetic data might help us to understand which model is the best at reproducing the geodetic signal. The depth range of slip is extremely important for rheologists trying to constrain composition and slip mechanisms for ETS and tremor, so it is important to acknowledge where uncertainties exist. Currently, the paper says quite matter-of-factly the depths over which slow slip and tremor occurs, despite this being a product of the slab model rather than any result provided by this paper.

Reviewer A had similar feedback about incorporating other fault models into our analysis. Since Delph et al. 2021 does not provide a new comprehensive slab model (just a few slab contours), we have calculated distributed slip models for several ETS events using the Slab2 model. Please see our response and figures above.

I also think the Discussion section would benefit from a rewording (Secs 5.1 – 5.2). The paper would be significantly strengthened if the authors broke down the different hypotheses covered in this section into their strengths and weaknesses, and integrated their own results to make the case about which one they think is the most compelling. Right now, this seems like a long list of already proposed potential hypotheses, and they do not integrate their results until after a discussion of all the mechanisms. This makes it somewhat unclear how their results provide insights into the dominant controlling mechanisms. Perhaps this would be more clear if they integrated their quite long and detailed 5.1-5.2 sections (which reads more like it should be in the Introduction/background) with their final conclusions stated in Section 5.3.

We appreciate the reviewer's feedback, and can see how a rewording might tighten the discussion. We considered the option of moving some content to the introduction, but decided that this would disrupt the flow of the paper; rather, we felt the discussion of the physical controls is best organized as a stand-alone section. Our goal with this portion of the discussion was to be comprehensive of the various hypotheses, while highlighting where uncertainties remain in our understanding. We do hope that this work inspires future modeling to constrain the processes impacting ETS in southern Cascadia. The synthesis at the end was intended to elevate our main points.

To strengthen this section, we have renumbered sections 5.1-5.3 into 6.0-6.4 to separate it from the earlier discussion of the slip distributions. To help the reader, we have added new introductory paragraphs (formerly Sections 5.1, now Section 6.0; Section 6.3) to summarize our hypotheses and offer our perspective on which hypotheses we believe are the strongest.

Overall, I think this is a nice piece of literature that will be a good contribution to our understanding of the margin, and I believe my recommendations above will significantly strengthen the implications of this study if carried out.

Detailed Comments

L38: The non-technical summary is ok, but seems to have quite different information in it than the Abstract. I think it would benefit the authors to make sure these communicate similar information so that both technical and non-technical readers come away with the same ideas from the paragraphs

We have edited the end of the non-technical summary to try and be more inline with the abstract.

Figure 1: Could be worth putting relative plate velocities on the plate motion vectors. Also, I only see one cyan circle. Should there be more?

We have added a scale for the plate velocity vectors to help gauge their magnitude. As noted in the caption and on lines 187-188, there are two anomalous LFE families that are represented by cyan circles. However, due to the close proximity of these two families, the cyan circles overlap and could be mistaken for one single circle. We note this in the caption.

110-123 I find the geometrical descriptions of the slab very difficult to follow and would encourage the authors to spend more time clearly communicating the geometrical features they think are important. For instance, "planar" and warped are not very descriptive to people unfamiliar with the JdF slab geometry. Instead, you could use “invariant along strike” and “has a convex surface”, or exhibits low curvature between 42-43 and high curvature to the south below the forearc.

We have changed the wording on lines 114-118 to try and make our geometrical descriptions clearer.

L114: I don't think the Guo et al 2021 paper constrains plate geometry. It just embeds it in their model. There, I don't think attributing slab geometry to this paper is appropriate.

We cited Guo et al. since they show cross sections of the velocity structure through the Mendocino Triple Junction, and thus image the down-going plate.

L126: Does Blair differ from those published in the supplemental material of McCrory et al. (2012)? I think that McCrory should be the citation of reference given that the slab model is provided in the supplemental material of that paper. This would allow readers to find the model for themselves, whereas the model cited by Blair is difficult to find, at least in digital form.

The Blair model includes minor revisions to the model published in the supplemental material of McCrory et al. (2012). However, we reached out to Pat McCrory and she recommended we cite the earlier work. Therefore, we have changed all of the Blair et al. (2013) citations to McCrory et al. (2012).

L273: I find it strange to apply a 14 day moving sum to an already 7-day smoothed vector, as there will be temporal leakage due to the smoother. Why not just take the average displacement for a given shorter duration period (i.e., a day if you are worried about daily variations as stated in point 4) and avoid significant temporal smoothing altogether. I'm curious how much your smoothing window length and moving sum window affect the magnitudes of the slip modeled later in this study, and it doesn't seem like those numbers were rigorously justified in this study.

The 14-day moving sum is used to determine the timing of the ETS events, not the final displacements that are used for the slip modeling. Specifically, we find that days with sums exceeding 0.5 mm westward correlate well with days during which a large ETS event is occurring. We experimented with other durations for the moving sum and found that a larger window led to ETS events being extended beyond the visually-estimated start and stop dates. Conversely, a smaller window tended to miss the relatively small displacements at the beginning and end of ETS events, leading to the start and end dates being too late and early, respectively. As mentioned on lines 289-292, we calculate the final displacements for the ETS events using GNSS time series data that has not had smoothing applied to avoid dampening the amplitude of the ETS signals.

L275: Change "-0.5" to 0.5 mm westward. Thresholds are implicitly magnitudes rather than vectors

We have substituted '-0.5' with '0.5 mm westward' on line 275.

Fig 4: The meaning of the horizontal lines should be clarified in 4B. I'm not sure the last sentence is needed, but could rather be merged with the first sentence in the 4B caption (e.g., "time periods used to calculate the surface displacements for each GNSS station in panel (horizontal black lines vertically located by station latitude)")

We have combined the first and last sentences of caption 4B.

Figure 5: It would be helpful is the slab contours were added to 5B (as done in Fig. 6). I also find it strange that you allow large discontinuities in your slip model. In reality, this would lead to a massive concentration of stress along the downdip edge of your model and be pretty nonphysical. Is there a justification for this?

We have added slab depth contours to Figure 5B. In order to test the southern extent of ETS in Cascadia, we allow slip to occur along the edges of the fault model south of 40.6° N latitude. We could avoid slip discontinuities by removing this detail and forcing slip to be zero along all the edges of the fault model. However, this would not allow us to determine whether ETS is not occurring on the southern edge of the slab or if slip is absent in this region due to constraints in the inversion. Additionally, we could avoid slip discontinuities by extending the downdip limit of the fault model. However, we limit the depth extent of the fault model according to our analysis in Section 3.2 and apply these depth limits to all of the inversions for consistency.

L552-559: There are some misstatements here: 1) gravity is sensitive to density variations only (which correlates with rock compositions). Gravity is not sensitive to changes in rock compositions with the same density, so that portion of the sentence should be removed. 2) Gravity is very sensitive to isostatically compensated material. If fact, assuming Airy isostasy, Bouguer gravity is a direct reflection of isostatic compensation. Therefore, you should remove the disclaimer about isostatic compensation. Basically, you could just say gravity is sensitive to density contrasts but is very non-unique.

We have removed the text referring to changes in rock composition and isostatic compensation on lines 618-619.

L643-648: I'm not sure it is valid to link LFEs to tremor/slow slip in your model given that the slab model you use in your studies lies significantly off what Plourde's LFE locations (seems like by a lot based on Plourde et al 2015). I think you should acknowledge somewhere that LFE depths from that paper are drastically shallower than the plate interface from the Blair model (and thus where you interpret that NVT and slow slip occurs). It means that either LFEs are not occurring on the plate interface, LFE locations are erroneous, or the plate interface model is wrong where they occur. I suspect that the interface model has the largest uncertainties… but depending on which it is, it could put the strength of the slip magnitude estimates and depth over which slip takes place into question as well.

L650: Should "lower" be "higher"?

It should be 'lower'. With everything else held constant, we would expect lower temperatures to help facilitate the seismic deformation associated with tremor, while higher temperatures would help facilitate aseismic deformation. We recognize how the choice of lower or higher depends on the reference frame, so we have altered the phrasing on line 729 to clarify.

L658: Has this small, continuous patch been interpreted to result from something besides interface slip? I know some localized families in the area have been interpreted to occur along crustal faults. Also, why could it not also simply be an asperity that concentrates strain and allows more tremor to occur? This patch seems fundamentally different from what is occurring during ETS events.

We agree that this tremor patch may not be due to slip on the megathrust. As noted at the beginning of Section 2.3 on lines 176-178, this patch could also be related to slip on a transform fault within the San Andreas fault system or transform slip on the edge of the Gorda slab. It is also possible that this patch could represent an asperity. In the context of this work, we believe that frequent ETS along a relatively weak portion of the megathrust fault is another valid hypothesis for this near-continuous tremor patch.

Recommendation: Revisions Required