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May 3, 2024

H. Tobin, Editor, *Seismica*

Dear Dr. Harold Tobin,

We thank you and the reviewers for your insightful suggestions for improving our manuscript "Forearc faults in northern Cascadia do not accommodate elastic strain driven by the megathrust seismic cycle". Based on these suggestions, we enclose a revised version of the manuscript, adding the word "Inner forearc" to the original title. The new title is: "**Inner forearc faults in northern Cascadia do not accommodate elastic strain driven by the megathrust seismic cycle**" for your consideration to publish in *Seismica*. Below, we paraphrase or quote the Reviewer's comments in bold, and follow with our response to each.

Reviewer A:

<u>General comments:</u>

Overall, the model methodology is outlined well, however I have a few queries related to the coupling model used (Delano et al. 2017)–what causes the negative dip-slip slip deficit rate in southern Cascadia at depth and is a full margin coupling model needed for this study, since the focus is on northern Cascadia and the upper-plate stress regime in the study region is likely not affected by locking at the southern margin? I do not have access to the supplemental material so the authors may have already addressed these comments there.

Correct, the anomalous negative slip deficit in southern Cascadia is located too far away from the study region to have a noticeable effect on our results. A full margin coupling model may not be necessary for the study. However, we have chosen to use the full margin coupling model as we have no reason to believe it hampers our model. The reason there is a negative dip-slip deficit at the southern Cascadia margin has been speculated on by several publications. Coupling in this region has been estimated using GNSS stations that are heavily influenced by deformation at the Mendocino triple junction and the negative slip deficits are also seen in the coupling model from Saux et al., (2022). Thus, the common suggestion is that complex processes at the triple junction may be influencing the estimation of the coupling distribution here. We have added the following sentence on line286 to add some more context about the negative slip deficit:

“The negative slip deficit here has also been described by other coupling models and it may result from forearc GNSS velocities in this region being significantly influenced by complex interactions with the Mendocino triple junction (e.g., Saux et al., 2022).”

The model results are nicely presented and described in the text, however the color bars used in figures 4, 5 and 6 are slightly misleading. I think the red to blue diverging color bar is great, however one issue I had when first looking at these figures was that the positive and negative slip deficit rates saturate at very different values. For example, in Figure 4(b), the darkest red occurs near to 35 mm/yr, however the equivalent darkest blue occurs at only -7-8 mmy/r. When looking at the figure, this makes it appear that the right-lateral slip deficit rate north of UTM 530000 m N is significantly higher than the left-lateral slip deficit rate just south of the change in obliquity. However, the magnitudes of these rates are actually very similar. This is also confusing when you try to evaluate the rates in terms of convergence obliquity. In northern Cascadia, the obliquity is very small as the authors state on line 87, but the deep blue makes it appear that the obliquity is as high as in southern Cascadia, where there is a high left-lateral slip deficit. I recommend adjusting the saturation points at each end so that the darkest reds and blues saturate at the same slip deficit rate. Figures 5 and 6 - it would also be useful to keep the color bar ranges consistent for the dip-slip and strike-slip results in each model. This would make it easier to compare the partitioned slip rates results side by side.

Thank you for this very good suggestion. These changes will help the reader compare the slip deficits and modelled slip rates more easily. We have now made the color bar saturate at the same slip rates (-30 and 30 mm/yr) for all the meshes shown in Figure 4. For Figure 5, we use a color bar that saturates at -3 and 3 mm/yr and Figure 6 we use -1.5 and 1.5 mm/yr.

The authors do not discuss how the modelled slip rate magnitudes differ from the magnitudes estimated by paleoseismic studies. I suggest adding a discussion on slip rate magnitude since you refer to the slip rates in the introduction. The paleoseismic reverse slip rates are 0.05-0.3 mm/yr and the oblique slip rates are 0.2-1.3 mm/yr along the LRDM (Table 1 of the manuscript). The modelled slip rates are similar in magnitude (0 - ~1.5 mm/yr, with some higher rates for the single fault models), despite the discrepancies in the sense-of-slip. Can you reconcile this in terms of the combined effects of subduction zone earthquake cycle loading and oroclinal bending? Could the combined effects of inter-seismic and co-seismic slip on these upper-plate faults produce the kinematics observed paleoseismically? Could this integrated view of the fault kinematics and slip rate magnitudes explain the paleoseismic data?

Thank you for bringing this up. We have included a new paragraph on line 415 that discusses slip rate magnitudes and how the paleoseismic observations could record the total slip of multiple earthquakes with contrasting slip.

“Despite the inconsistencies between the BEM models and paleoseismic observations in slip sense, most of the modelled absolute slip rates compare relatively well with most of the observed slip rates (Table 1). Model B, and Models D through F estimate absolute slip rates (0 to 1.5 mm/yr) that are similar to the slip rates observed in paleoseismic studies. Given this similarity, we speculate that the paleoseismic slip sense observations could incorporate alternating slip histories resulting from stress reversals during the megathrust cycle (e.g., Hasegawa et al., 2012; Regalla et al., 2017; Cortés-Aranda et al., 2022), and the kinematics observed today are the net sum of these alternating slip sense events. This hypothesis could explain why left-lateral slip sense is observed on the Utsulady Point fault in contrast to right-lateral slip sense or pure dip slip along the rest of the fault system along strike, such as the Devil's Mountain Fault (Fig. 2a; Table 1). Although this possibility exists, the most unambiguous observation of strike-slip displacement are right-laterally offset channels observed in 3D trenching along the Devils Mountain fault (Personius et al., 2014), and the oblique right-lateral slip is consistent with interseismic GNSS and seismicity observations (Fig. 2b; Fig. 3). This consistency between trenching and interseismic observations is indicative that permanent interseismic strain is right-lateral, and is in contrast with all of our interseismic BEM models (Models A, B, D, E). Finally, Models A and C predict maximum slip rates of up to 3 to 3.5 mm/yr at the western end of the LRDM, which have not been observed in paleoseismic investigations. In fact, these highest modelled slip rates occur where the LRDM is thought to be inactive (MacLeod et al., 1977; Fairchild and Cowan, 1982; Groome et al., 2003). Thus this inconsistency may be further evidence that our simple model of elastic deformation resulting from the megathrust cycle cannot be used to explain the observed forearc faulting in northern Cascadia."

There are some inconsistencies in the discussion of the model A results at several places in the discussion section. In lines 388-390 the authors state *"the single-fault interseismic model (Model A) and instrumental seismicity suggest similar reverse right-lateral strain regimes off the west coast of Vancouver Island".* This is then stated to be consistent the seismicity and strain regime in the region. However, in Figure 5a and b (Model A) the motion is reverse left-lateral which would be inconsistent with the right-lateral focal mechanisms shown in Figure 2, but consistent with the SHmax orientation from Balfour et al. (2011). The authors later discuss Model A in lines 397-398 and lines 414 to 416 in terms of reverse left-lateral slip (now consistent with the Figure 5) and go on to say this is consistent with the seismicity. I recommend that the authors review their discussion sections and the comparison between the model results, crustal seismicity and crustal principal stress directions. I would also recommend citing Figure 2 or other data you are referring to when discussing the seismicity.

Thank you for noticing this discrepancy. It is the result of a typo on line 389, where right-lateral should have been "left-lateral". We have fixed this typo and now refer to Fig. 2 when discussing the seismicity here. We also now only note that it is consistent with the S_{Hmax}, which was calculated from all of the focal mechanisms, not just of the larger earthquakes seen on the figure.

Lastly I recommend adjusting the title by adding Inner or Onshore before forearc faults to be consistent with your conclusions.

Good suggestion, we have changed the title accordingly.

<u>Line comments:</u>

Line 52: Describe what is meant by the inner forearc here, when it is first introduced. Currently this is described in line 103-105.

We have added "onshore" to the sentence to clarify.

Line 84: Replace 'an' with 'a'

Corrected.

Lines 90-91: I don't disagree that the obliquity affects the GNSS signal, but the resultant effect of the change in obliquity along strike is very difficult to see in the GNSS data (figure 1). This signal is swamped by the combined effects of subduction, Sierra Nevada-Great Valley block motion (as mentioned) and transform motion associated with the San Andreas plate boundary. I would recommend rewording this paragraph slightly to de-emphasize the obliquity control on the broad spatial patterns shown in Figure 1.

Thank you for this suggestion. We have changed the topic sentence of the paragraph on line 89 to de-emphasize the obliquity control on the GNSS vector pattern.

"Global navigation system satellite (GNSS) surface velocities of the forearc relative to stable NA reflect this change in obliquity and the broader tectonics of the region."

Line 100: I recommend replacing 'reflect the fact that' with 'show' or an equivalent word.

The suggested word change has been made.

Line 101: I think of locking as referring to the mechanical process of locking a fault and coupling as being the geodetically-modelled signal, which can be partial to full. I would avoid using locking and coupling interchangeably.

We have removed the word "locking" from this sentence.

Line 111: Add a reference for this statement on wider coupling in northern Cascadia.

Reference added.

Line 136: The role of Basin and Range extension and San Andreas shear is unlikely to impart a strong (or any) control the upper-plate stress field in the study region. I would make this clearer in this sentence.

This paragraph refers to the Cascadia subduction zone as a whole, and we consider these tectonic processes are very likely to play a role in upper plate deformation along the southern margin. Additionally, we don't mean to imply that we are referring only to right-lateral shear along the San Andreas fault, which is why we don't mention it explicitly in this sentence. Distributed right-lateral shear at the Queen Charlotte triple junction at the northern terminus of Cascadia, may impart stress on the upper plate in northern Cascadia. We have now clarified that Basin and Range extension would likely only have a major effect in the south at line XX.

Line 184: Could the western extent of the LRDM be fully locked down to the subduction interface and thus be the reason for the reduced seismicity?

The western extent of the onshore LRDM is not above the fully locked portion of the subduction zone interface, and in fact, crustal seismicity increases further west, offshore, above the locked zone. There may be a control on crustal seismicity and megathrust locking extent in Cascadia, but we can only speculate on that control and are, therefore, hesitant to mention it in the introduction to the paper.

GNSS strain paragraph: If the LRDM is observed geodetically (and in turn is a block boundary), this suggests that it has a shear zone at depth. This may be important later on when choosing the depth extent of the LRDM in your models.

The LRDM is imposed as a boundary based on geologic studies, not based on it being a large geodetically observed shear zone. It is then kept as a boundary as it allows a good fit with the GNSS velocities. Thus, using geodesy to define a shear zone a depth has not been done. However, the shear zone at depth is highlighted by relocations of seismicity by Li et al., 2018 which show seismicity along the structure to a depth of 28 km (as we now note on line 190).

Lines 228-235: I am not familiar with all of these papers and this is outwith the work presented here, but since GNSS data are used to constrain coupling in the first place, what can the GNSS residuals tell you? Don't the residuals essentially reflect the misfit of the coupling model?

These papers suggest that the residuals show forearc deformation unrelated to subduction zone coupling. However, we agree that this argument can be circular because the coupling is determined from the GNSS vectors in the first place, and the coupling also has large uncertainty associated with it. We note this very argument on line 232.

Line 246: This is beyond the scope of your study, but I wonder how heterogeneities in the upper plate geology across this region would affect your results.

This is a good point as the Leech River fault is an ancient terrane boundary, but as the reviewer acknowledges, this modelling is beyond the scope of this study.

Line 250—253: This sentence has some grammatical errors.

The grammatical errors have been resolved. Thank you for catching them.

Line 267: Why do you use the McCrory et al. (2004) slab model and not the more recent McCrory et al. (2012) slab model?

We use the slab coupling mesh from the Delano et al., (2017) study, which used an updated version of the McCrory et al., (2004) slab model (updated in 2006). Using a different slab model would require creating a new coupling model, which was not the aim of this study. We have changed the citation to McCrory et al., 2006 to reflect the use of the updated model by Delano et al., (2017).

Line 269 - 272: I would add slightly more detail to describe how the GNSS data are treated in the coupling inversion (even if this is also described in Delano et al. 2017), since there are several slightly different

Cascadia coupling models where authors have treated the GNSS data differently. It may also be useful to include a supplemental figure showing the block model and GNSS data used for the coupling model.

We believe that fully describing this already published model would be redundant as it is already described by Delano et al., (2017) and the references therein. Any added detailed description in our manuscript would just be paraphrasing their work. We also already direct the reader to the supplemental material of Delano et al., 2017 on line 290 for more details on the model. However, to address the second part of your comment, we have added details on the number of stations used in the GNSS velocity field (line 274).

"These slip deficit values have been calculated using a geodetically constrained block model that uses a GNSS velocity field consisting of 1717 stations, in conjunction with independently (geologically) calculated relative block motions to predict slip on the block boundaries (Meade and Loveless, 2009; Delano et al., 2017)."

Line 272: What is causing the negative slip deficit for the dip-slip component? Also, if this region has little to no impact on your results in northern Cascadia, is it necessary to include coupling along the entire length of the subduction zone in your model?

See reply to the first general comment above.

Line 297: Is the maximum depth (28 km) below the brittle-ductile transition? See comment for line 101.

The maximum depth is based on earthquake relocations from Li et al., 2018. The brittle-ductile transition is suggested to be deep in northern Cascadia based on that observation and the low heat flow observed on Vancouver Island (see Li et al., 2018).

Figure 4: It might be easier to refer to this figure when reading the paper if you label each of these plots as inter-seismic or co-seismic.

Good suggestion, we have now labelled the plots.

Line 316: I wonder how including faults with contrasting orientations would affect your results.

We are unsure what is meant by contrasting orientations in this instance. Would contrasting refer to opposite dips or completely different strikes? We based the modelled faults on the observed and mapped structures in the region. Although it is outside the scope of our study, it would be interesting to test hypothetical structures with different orientations in a different study.

Lines 317-320: Why do you exclude the western end of the LRF in the multi-fault model BUT include the western and offshore portion in the single fault model? If there is no compelling evidence that the fault extends westward and offshore, then is it reasonable to include it in the single fault model?

We exclude the western portion of the fault in the multi-fault model to test if its removal has a major effect on our results (as noted on line 327), which it doesn't. We have chosen to keep it in the single fault model because the ancient terrane-bounding structure extends offshore (e.g., Macleod et al., 2017), and there is crustal seismicity offshore that indicates if the active LRF did extend offshore, if would be a left-lateral fault (Fig. 2b). Therefore, this model set-up allowed us to test if this seismicity could result from the elastic response of the forearc to subduction zone coupling or forearc rupture (as noted on line 313).

Line 246: It's curious that the highest slip rates are on the western portion of the fault, which is also the portion of the fault that has no or little geologic constraints on faulting (lines 317-320).

We agree. This discrepancy between the geologic observations and the model are one of the reasons we suggest our models do not explain inner forearc faulting in northern Cascadia. We have now included text that begins on line 431 that highlights this point.

"Finally, Models A and C predict maximum slip rates of up to 3 to 3.5 mm/yr at the western end of the LRDM, which have not been observed in paleoseismic investigations. In fact, these highest modelled slip rates occur where the LRDM is thought to be inactive (MacLeod et al., 1977; Fairchild and Cowan, 1982; Groome et al., 2003). Thus this inconsistency may be further evidence that our simple model of elastic deformation resulting from the megathrust cycle cannot be used to explain the observed forearc faulting in northern Cascadia."

Line 361: Grammatical error in sentence.

Corrected.

Line 412-412: Since we are in the inter-seismic period, I question whether you should compare the co-seismic slip results to the current crustal seismicity

We make this comparison between the coseismic slip results and seismicity for completeness as the interseismic coupling models do not produce the kinematics observed in seismicity. By comparing the seismicity with the coseismic slip results, we also rule out that the seismicity could somehow represent a delayed elastic response of the forearc to cosesismic slip on the megathrust.

Line 414: The few focal mechanisms that I see offshore in figure 2 are right-lateral and not left-lateral.

We have edited this paragraph to refer to the SHmax instead of the focal mechanisms (see earlier comments).

Line 502: spelling error Cascadiaelastic

Corrected.

Reviewer B:

First, I am not clear how the authors define the strike-slip component of the interseismic and coseismic models. The "strike" of triangular patches is not always intuitively defined and I have seen some strange results arising from this effect, where the apparent "strike" varies by more than 90 degrees over short

distances on a shallowly dipping mesh. Is the strike defined globally or locally for each patch? How much variation is there among patches? I suggest that plotting the slip direction vectors and the strike or dip orientation for a subset of the fault patches directly on Figure 4, or adding a supplementary figure showing the slip orientations, would make this aspect more clear.

Thank you for noting this ambiguity in our model description. The strike of each patch is defined individually. These individual strike definitions are important to model the change in the orientation of the slab along strike and down dip. Because the slab-mesh is based on the McCrory et al., 2006 slab contours, these contours represent the strike of the corresponding triangular meshes. We have added the following sentence on line 270 to make definition of strike clearer:

"Each TDE of the mesh is defined by its own strike and dip based on the slab contours, and these orientations are used to resolve the strike-slip and dip-slip components of deformation."

Second, I find the description of the BEM model implementation lacking in Section 3.3. Do patches on the LRDM fault (and other crustal faults) interact with themselves to relieve the stresses induced by slip on neighboring fault patches, in addition to the megathrust slip? I assume so, but it is not clearly stated here. Expanding this section somewhat would make this more clear.

Thank you for this suggestion. The patches on the crustal faults do interact with themselves to relieve the stresses. This is why we see lower slip rates in the multi-fault model. We have now added the following text on line 340 to convey this portion of the model description more clearly.

"The model assumes a linear homogeneous crust, and that the crustal faults are traction-free surfaces that relieve all of the stress imposed on them, including the stresses induced by slip on neighboring TDEs on the same or neighboring faults. Therefore, where we model multiple crustal faults, the slip rates are lower where the faults are in close proximity to each other as the strain is distributed. Additionally, slip rates are lower at the edges of the faults where there are no free surfaces."

Finally, I have a question about the validity of separating the estimation of megathrust coupling from the predicted slip on crustal faults, as is being done here. At least conceptually, the coupling model could be biased by the absence of crustal faults and might predict a different sense of strain on those faults than if they were included in the model explicitly. One potential way to address this would be to assess the stresses resulting from other coupling models, like that of Evans et al. (2022) that do include the LRDM and other faults explicitly in their block model formulation. Interestingly, the authors note that the Evans et al. (2022) model actually predicts right-lateral thrust motion on the crustal faults in this region, which is in agreement with the paleoseismic observations… so I would be very curious to see what stresses that model predicts on these crustal faults! At the very least, the authors should include some more discussion of this possibility - block motion is not necessarily inconsistent with their interpretation of oroclinal bending resulting in the observed slip rates, but I think a more nuanced description of this issue would be helpful.

The block model used to estimate coupling distribution along the Cascadia subduction zone includes microplates bounded by crustal faults with 100% coupling. One block boundary is close to and approximates the LRDM (Evans et al., 2022) and therefore the coupling distribution is not biased by the absence of crustal faults. The Evans et al., (2022) coupling distribution, at least in northern Cascadia, is very similar to the Delano et al., (2017) coupling distribution that we use, and would likely produce the same results we see. The prediction of right-lateral slip is based on the block model, where GNSS velocities constrain the relative rotations of microplates, interseismic elastic deformation due to locked faults, and the distribution of coupling along partially locked faults. Slip rates are then calculated using rotation rates at block boundaries. Our model is significantly different in that it uses one of the results of the block model (Cascadia subduction zone coupling) to then estimate elastic stress and forearc fault kinematics.

We have added the following sentence on line 277 to make it clear that the block model used to estimate coupling includes a block boundary close to the LRDM:

“This block model uses potentially active faults as block boundaries, and includes a block boundary that is close to our mapped trace of the LRDM.”

Additionally, in the comparison of our results with the geodetic block model estimations of slip, we include the following paragraph beginning on line 467 to better explain the differences in the block model slip rate estimations and the fault kinematics we estimate:

“Geodetic block models of Cascadia use GNSS velocities to constrain the relative rotations of microplates, interseismic elastic deformation due to locked faults along microplate boundaries, and the distribution of coupling along partially locked faults. Slip rates are calculated using the difference between the rotation rates at the block boundaries (Meade and Loveless, 2009; Evans, 2022). Thus, these models represent an inversion of the GNSS velocities, while our model "drives" forearc deformation using the stress that results from subduction zone coupling. As the block models are inferred to be a prediction of the current deformation being observed at the block boundaries, they are a useful comparison with our modelled fault kinematics.“

Minor comments:

Abstract: LRDM abbreviation is used but not defined in the abstract.

Corrected, however, the abstract was then over the word limit of 200 words so we have had to make additional minor edits to the abstract.

L138 roll -> role

Corrected.

L144: awkward use of the word “using”. Perhaps “through/from paleoseismic records/observations”?

Good suggestion, we have made the suggested edit.

L214 vectors -> values (?)

Changed to:

“the absolute values of the vectors”

L350 mimick -> mimic

Corrected.

Line 389 - model A does not suggest reverse right-lateral slip on the LRDM, as stated here. Perhaps you mean left-lateral?

Thank you, good catch. Seismicity suggests a left-lateral regime on east-west faults offshore. We have made the correction.

L390 missing word, add “indicate” before subduction

We have made the suggested edit.

L445 model -> our models

We have made the suggested edit.

L502 Cascadiaelastic -> Cascadia; soley -> solely

We have made the suggested edit.

Again, we would like to thank our reviewers for their comments and we believe that they have led to changes that have substantially improved our manuscript.

Sincerely,

N. Harrichhausen, K.D. Morell, and C. Regalla

Original complete text of reviews

Reviewer 1:

Summary:

The authors present a well-written thorough investigation of how an upper plate fault system within the northern Cascadia forearc (the Leech River-Devils Mountain fault system) is expected to deform in response to the subduction earthquake cycle: interseismic subduction zone coupling and coseismic rupture. They compare their results (from a boundary element model) to (a) the long-term slip kinematics (from paleoseismic studies); (b) the characteristics of recent seismicity; (b) crustal maximum principal stress directions; and (c) results from block models. Their conclusion is that the observed kinematics of the LRDM (reverse right-lateral) are inconsistent with the modeled behavior due to strain produced by interseismic subduction locking as well as co-seismic rupture on the subduction interface. To explain the observed fault kinematics, the authors suggest the primary driver of motion along this fault system is oroclinal bending in this region.

The conclusions of this manuscript are important for advancing understanding of how the subduction megathrust influences onshore upper-plate faulting in northern Cascadia. This in turn has important implications for seismic hazard analyses.

I recommend that this manuscript be returned to the authors for revisions.

General comments:

Overall, the model methodology is outlined well, however I have a few queries related to the coupling model used (Delano et al. 2017)–what causes the negative dip-slip slip deficit rate in southern Cascadia at depth and is a full margin coupling model needed for this study, since the focus is on northern Cascadia and the upper-plate stress regime in the study region is likely not affected by locking at the southern margin? I do not have access to the supplemental material so the authors may have already addressed these comments there.

The model results are nicely presented and described in the text, however the color bars used in figures 4, 5 and 6 are slightly misleading. I think the red to blue diverging color bar is great, however one issue I had when first looking at these figures was that the positive and negative slip deficit rates saturate at very different values. For example, in Figure 4(b), the darkest red occurs near to 35 mm/yr, however the equivalent darkest blue occurs at only -7-8 mmy/r. When looking at the figure, this makes it appear that the right-lateral slip deficit rate north of UTM 530000 m N is significantly higher than the left-lateral slip deficit rate just south of the change in obliquity. However, the magnitudes of these rates are actually very similar. This is also confusing when you try to evaluate the rates in terms of convergence obliquity. In northern Cascadia, the obliquity is very small as the authors state on line 87, but the deep blue makes it appear that the obliquity is as high as in southern Cascadia, where there is a high left-lateral slip deficit. I recommend adjusting the saturation points at each end so that the darkest reds and blues saturate at the same slip deficit rate. Figures 5 and 6 - it would also be useful to keep the color bar ranges consistent for the dip-slip and strike-slip results in each model. This would make it easier to compare the partitioned slip rates results side by side.

The discussion section of the manuscript (Section 5 onwards) is well organized, and I have two main comments:

- Section 5.1: The authors do not discuss how the modelled slip rate magnitudes differ from the magnitudes estimated by paleoseismic studies. I suggest adding a discussion on slip rate magnitude since you refer to the slip rates in the introduction. The paleoseismic reverse slip rates are 0.05-0.3 mm/yr and the oblique slip rates are 0.2-1.3 mm/yr along the LRDM (Table 1 of the manuscript). The modelled slip rates are similar in magnitude (0 - ~1.5 mm/yr, with some higher rates for the single fault models), despite the discrepancies in the sense-of-slip. Can you reconcile this in terms of the combined effects of subduction zone earthquake cycle loading and oroclinal bending? Could the combined effects of inter-seismic and co-seismic slip on these upper-plate faults produce the kinematics observed paleoseismically? Could this integrated view of the fault kinematics and slip rate magnitudes explain the paleoseismic data?

- There are some inconsistencies in the discussion of the model A results at several places in the discussion section. In lines 388-390 the authors state *"the single-fault interseismic model (Model A) and instrumental seismicity suggest similar reverse right-lateral strain regimes off the west coast of Vancouver Island".* This is then stated to be consistent the seismicity and strain regime in the region. However, in Figure 5a and b (Model A) the motion is reverse left-lateral which would be inconsistent with the right-lateral focal mechanisms shown in Figure 2, but consistent with the SHmax orientation from Balfour et al. (2011). The authors later discuss Model A in lines 397-398 and lines 414 to 416 in terms of reverse left-lateral slip (now consistent with the Figure 5) and go on to say this is consistent with the seismicity. I recommend that the authors review their discussion sections and the comparison between the model results, crustal seismicity and crustal principal stress directions. I would also recommend citing Figure 2 or other data you are referring to when discussing the seismicity.

Lastly I recommend adjusting the title by adding Inner or Onshore before forearc faults to be consistent with your conclusions.

<u>Line comments:</u>

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Line 84: Replace ‘an’ with ‘a’

Lines 90-91: I don’t disagree that the obliquity affects the GNSS signal, but the resultant effect of the change in obliquity along strike is very difficult to see in the GNSS data (figure 1). This signal is swamped by the combined effects of subduction, Sierra Nevada-Great Valley block motion (as mentioned) and transform motion associated with the San Andreas plate boundary. I would recommend rewording this paragraph slightly to de-emphasize the obliquity control on the broad spatial patterns shown in Figure 1.

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GNSSS strain paragraph: If the LRDM is observed geodetically (and in turn is a block boundary), this suggests that it has a shear zone at depth. This may be important later on when choosing the depth extent of the LRDM in your models.

Lines 228-235: I am not familiar with all of these papers and this is outwith the work presented here, but since GNSS data are used to constrain coupling in the first place, what can the GNSS residuals tell you? Don’t the residuals essentially reflect the misfit of the coupling model?

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Line 267: Why do you use the McCrory et al. (2004) slab model and not the more recent McCrory et al. (2012) slab model?

Line 269 - 272: I would add slightly more detail to describe how the GNSS data are treated in the coupling inversion (even if this is also described in Delano et al. 2017), since there are several slightly different Cascadia coupling models where authors have treated the GNSS data differently. It may also be useful to include a supplemental figure showing the block model and GNSS data used for the coupling model.

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Lines 317-320: Why do you exclude the western end of the LRF in the multi-fault model BUT include the western and offshore portion in the single fault model? If there is no compelling evidence that the fault extends westward and offshore, then is it reasonable to include it in the single fault model?

Line 246: It's curious that the highest slip rates are on the western portion of the fault, which is also the portion of the fault that has no or little geologic constraints on faulting (lines 317-320)

Line 361: Grammatical error in sentence.

Line 412-412: Since we are in the inter-seismic period, I question whether you should compare the co-seismic slip results to the current crustal seismicity

Line 414: The few focal mechanisms that I see offshore in figure 2 are right-lateral and not left-lateral.

Line 502: spelling error Cascadiaelastic

Reviewer 2:

I reviewed the contribution titled "Forearc faults in northern Cascadia do not accommodate elastic strain driven by the megathrust seismic cycle" by Harrichhausen et al., submitted to Seismica. The authors use a boundary element model to assess the sense of slip on crustal faults above the Cascadia megathrust that would release the stresses produced by either interseismic coupling or coseismic slip. They conclude that in either case, the slip sense is not consistent with paleoseismic and microseismic observations, except possibly in the outer forearc (offshore Vancouver island). They suggest that instead, the inner forearc likely deforms due to long-term stresses associated with oroclinal bending and extrusion of the Olympic Peninsula. Overall, I am impressed with the quality of this work and feel it should be published after some minor revisions.

First, I am not clear how the authors define the strike-slip component of the interseismic and coseismic models. The “strike” of triangular patches is not always intuitively defined and I have seen some strange results arising from this effect, where the apparent “strike” varies by more than 90 degrees over short distances on a shallowly dipping mesh. Is the strike defined globally or locally for each patch? How much variation is there among patches? I suggest that plotting the slip direction vectors and the strike or dip orientation for a subset of the fault patches directly on Figure 4, or adding a supplementary figure showing the slip orientations, would make this aspect more clear.

Second, I find the description of the BEM model implementation lacking in Section 3.3. Do patches on the LRDM fault (and other crustal faults) interact with themselves to relieve the stresses induced by slip on neighboring fault patches, in addition to the megathrust slip? I assume so, but it is not clearly stated here. Expanding this section somewhat would make this more clear.

Finally, I have a question about the validity of separating the estimation of megathrust coupling from the predicted slip on crustal faults, as is being done here. At least conceptually, the coupling model could be biased by the absence of crustal faults and might predict a different sense of strain on those faults than if they were included in the model explicitly. One potential way to address this would be to assess the stresses resulting from other coupling models, like that of Evans et al. (2022) that do include the LRDM and other faults explicitly in their block model formulation. Interestingly, the authors note that the Evans et al. (2022) model actually predicts right-lateral thrust motion on the crustal faults in this region, which is in agreement with the paleoseismic observations… so I would be very curious to see what stresses that model predicts on these crustal faults! At the very least, the authors should include some more discussion of this possibility - block motion is not necessarily inconsistent with their interpretation of oroclinal bending resulting in the observed slip rates, but I think a more nuanced description of this issue would be helpful.

Minor comments:

Abstract: LRDM abbreviation is used but not defined in the abstract.

L138 roll -> role

L144: awkward use of the word “using”. Perhaps “through/from paleoseismic records/observations”?

L214 vectors -> values (?)

L350 mimick -> mimic

Line 389 - model A does not suggest reverse right-lateral slip on the LRDM, as stated here. Perhaps you mean left-lateral?

L390 missing word, add “indicate” before subduction

L445 model -> our models

L502 Cascadiaelastic -> Cascadia; soley -> solely

I hope these comments are helpful to the authors, and encourage them to contact me with any questions.

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