Dear Dr. Lynch,

Many thanks for submitting the revised version of your manuscript. We have reached a decision regarding your submission to Seismica, "Evidence for an active transtensional Beaufort Range Fault in the northern Cascadia forearc". We will be pleased to formally accept your manuscript for publication in Seismica once we have received some manuscript source files so that we can immediately proceed to the Copyediting stage.

Since Seismica's copyeditors are volunteers, it is best to help them out with their job as early as possible. Below here you will find a line number list of mostly grammar-related comments. Please address and consider my comments/suggestions as you give your manuscript a final read-through, i.e., spelling/grammar check.

I have highlighted several instances of the overused vague terms “these” and “this”, but not all. Such vague language can undermine the strength of your writing/arguments. Please reconsider the remaining unhighlighted instances and ask yourself if the terms can be replaced with more precise language to improve clarity, as the audience (both non-specialist and specialists) will likely become confused with the intended meanings. The clarity onus is the authors, not the readers.

The anonymous reviewer is Alan Nelson. They have confirmed/approved being listed.

Please upload the following files, so that we can proceed to the next stage of the publication process:

- The final, cleaned manuscript using the Seismica template in Microsoft Word, OpenOffice or LaTeX file format (found on the Templates page) with figures included in the text. If using LaTeX, please also include your bibliography *.bib* file containing only the references that your manuscript cites.

- Separate publication-ready figure files in .png format at a minimum of 300 dpi resolution

- Supplementary material should be uploaded as a separate pdf file that will not be formatted. Supplementary material should not be included in the main paper.

Good luck with the final stages, and please don't hesitate to ask if you have any questions.

Cheers and best, -J

Lines 74-77: Please consider if this paragraph could be improved with mention of (1) thick vegetation/dense forest cover and (2) no 1946 surface rupture has previously been documented.

Line 130-132: Please include a statement about the lidar quality here, e.g., (something to effect of) “The lidar dataset was used to identify scarps but, in most instances, the lidar resolution was too low to measure offset of geomorphic features”....this message needs to be made clear for the audience at this point in the manuscript.

Line 134: By "*charcoal*", do you mean "samples"?...Would you have dated something else if it were found, e.g., a leaf fragment?

Line 135: *You wrote "These results are described and illustrated in Section 4."*...Please delete "*these*" and put the rest within parentheses, which will introduce the format used later in this paragraph.

Line 156: "*and these*"...please replace with "which"

Line 157: Replace "*a few of the*" with "3", and parenthesize "Figure 3c, see supplemental Text S2 for details of radiocarbon methods"

Line 161: delete "*associated*"

Line 166: What do you mean by "*These*"; Qt or Qk, or both, or Qhm? Please replace "*these*" with the correct unit name(s).

Line 167: "*five evenly spaced, flat topped terrace treads with steep risers*" Please add a figure reference.

Line 168: Earlier in the paragraph, Qt and Qk each had description sentences. Please consider providing a description sentence for Qhm.

Lines 168-169: Delete *"Unfortunately*". Are you certain the ice contact deposits are completely *barren* of any date-able radiocarbon material, or did you just not find any? Please consider rewording the sentence.

Line 169: Delete "*Nonetheless*"

Line 171: Delete "*although*" and consider replacing "*recognize*" with "acknowledge"

Line 175: "*interpret*"; based on what?...consider providing interpretations post the presentation of the observations/data, i.e., at the end of paragraph...help guide the audience to the best (your) interpretation...

Line 181: Delete "*From these paraglacial deposits,*" and "*dateable*" and un-parenthesize "*BR-9*"

Line 181-183: Combine sentences. Delete the period and "*this sample was located in*" and replace with "from a stratified fan..."

Line 183: Replace "*This*" with "Sample BR-9 yielded..." and after the comma insert "which is" before "*consistent*".

Line 184: delete "*broadly*"

Line 186: What do you mean by "these types"? Do you mean "*clast supported, poorly sorted, stratified sand and gravels*"? Please replace "these types" with a specific description.

Line 195: "*the fans*"...what fans, the alluvial and fluvial fans?..

Lines 195-196: either delete or move to the front of the sentence "*alluvial and fluvial deposits*"

Lines 199-200: please cite a figure

Line 206: delete "*also*"...Was BR-9 not macro? If it was macro, then I suggest not describing BR-8 and BR-42 samples differently.

Line 206: this sentence is repetitive compared to the first sentence, which introduces the paragraph topic well, i.e., units and dated material....combine this sentence 2 with the next sentence 3...which will omit the use of "this".

Line 208-209: "*Sample BR-8 yielded a radiocarbon age of ~6 cal ka BP*"...This sentence is repetitive, not needed, and should be removed, as the information can be directly ascertained from table 1.

Line 211: Delete "*Sample BR-42 yielded a radiocarbon age 0f ~3.5…",* similar to line 208 comment...in the BR-9 instance above this type of statement had a follow up...here you have them standing on their own and therefor just repetitive from what table reads.

Line 211: replace "*Both ages*" with "Samples BR-8 and BR-42"

Line 212: replace "determined from a radiocarbon sample" with "of "BR-9"

Line 212: replace "*and agree with our stratigraphic interpretation*" with "suggesting"

Line 216: please be consistent and use the same term..."the channels" or "the paleo channels"...delete "are" and "d into"

Line 217: delete the "and" and combine this sentence with the next (continue the list), i.e., replace ". These channels" with "and".

Line 218-219: Please re word, the sentence uses "but" twice.

Line 220: "interpret" based on what? Incision patterns? Please consider stating "Based on X"... "these" Please be more specific. Do you mean all mapped paleochannels or only the ones at Site 1?

Line 222: "*These*"...similar to above comment...it is unclear if you're referring to site 2 paleochannels or mapped paleo channels from all sites...

Line 224: Is this paragraph is missing a summary/closing sentence?...maybe an interpretation sentence?...As a reader, I'm left with the feeling of a missed opportunity here.

Line 227: delete the second "*to*", before *"identify*"

Line 228: *"conducted these investigations"* ... please replace with something to the effect of "applied our scarp mapping approach"

Line 229: The Quaternary scarps, section 5, Intro paragraph needs further refinement, regarding the mapping approach presentation/description and tectonic scarp identification. I think many readers will have difficultly following the work-flow-order in its current presentation. Please reconsider your description of this process. It is **very important** that the tectonic scarp identification process is easy to follow for the audience. Perhaps insert a recipe-like-list, after the second sentence of the paragraph. I am envisioning something like your Approach and methods, section 3, paragraph. The approach-recipe-list will (1) serve as a guide for the reader to navigate through the section, (2) likely avoid confusion and subsequent concerns, and (3) help the audience understand your workflow and the logic used to identify tectonic scarps. IMO, such an explicit presentation would have prevented several of the reviewers' concerns.

Figure 4. Please correct the Fig. 4c inset box extent in 4a. Currently, 4c shows much more than the inset box extent suggests, also check the scale bar.

Line 231: Delete "*carefully*" and replace "*these*" with "candidate fault scarps that"

Line 233: Consider adding a closing statement, maybe something to the effect "Post exclusion of non-tectonic forming scarps, the remaining scarps were interpreted to most likely be of tectonic origin."

Line 234: Delete "*Topographic*", when are landforms not topographic?

Line 234: Delete "*that we determined to be*"

Line 238: "*sackungen*" considering reviewers comments, can you cite a reference somewhere in here?

Line 243: "Examples"…how many?…Does this even need to be a standalone sentence? Can you just delete and add the figure reference to the open sentence of the paragraph?

Line 245-250: This is a two-sentence paragraph. Please reconsider the wording of the paragraph's opening sentence as the message is unclear, e.g., the scarps are 100-150 m long AND 2 km along strike? do you mean that the individual scarps are parts of a continuous network for up to 2km?

Line 282: "*fault*"…Please denote/explain where the mapping occurred. i.e., within the GIS and/or in the field…

Line 335: replace "*In addressing"* with "To address"

Line 340: delete "*these*"

Line 341: replace "*these*" with "the" or "our"

Line 342: "these"…what kind of locations? The scarp profiles?

Line 446: replace "if these interpretations of three events are correct" If our three-event interpretation is correct"

Line 475: replace "These" with "Such"

Line 496: replace "These" with "Our"

Response to Reviews on "Evidence for an active transtensional Beaufort Range fault in the northern Cascadia forearc"

Response to the Associate Editor

I have received three reviews of your revised manuscript, which are included below and attached. The reviews indicate that major revisions are still needed before we can consider proceeding with your paper. All three reviewers suggest major revisions and/or reorganization of the manuscript. I am therefore returning the paper to you so that you can make the necessary changes in the manuscript and respond to the reviews. Collectively, the suggestions and concerns provided here aim to help improve the manuscript and, ultimately, see it published.

Considering the significance of this fault, the hazard implications for the regional stakeholders, a more conservative and scrutinized approach is warranted and given the novelty, it necessitates a more convincing case for the community.

Reviewers 1 and 2 re-reviewed the manuscript and think their concerns have not been addressed. Reviewer 3 thinks the revisions are thorough but emphasize a need for major reorganization of the manuscript structure to help improve the flow and impact the work. I agree with all three reviewers, I think a major reorganization will better address Reviewers 1 & 2 concerns, further highlight the robustness of your dataset, reduce repetition, and improve the flow of presentation.

In general, I see and appreciate your efforts on round 1 revisions. For example, I found the reconsidered introduction and attempt at reorganizing some results/discussion topics as helpful improvements. However, I also agree with reviewers 1 & 2 in thinking that in several places your responses and revisions fall short of the mark. To be fair, Reviewer 3 thinks you have adequately addressed the reviewers concerns and that the reviewers may have been overly critical in places. However, I respectfully disagree with reviewer 3 on the "over-critical" part of their opinion. The importance of this fault warrants a comprehensive and critical evaluation and there is still too much unaccounted-for uncertainty.

To varying degrees, the reviewers and I think there is an inadequate level of uncertainty mentioned and/or discussed, e.g., regarding the scarp source topic, the offset measurements, and the 3-event argument. I strongly suggest that you carefully re-evaluate the level of certainty and uncertainty that you present during this round of revisions and in your responses. I urge you to proceed with much caution and reverence, as this work may have major impacts for both specialist and non-specialist discourse communities.

Fortunately, I think a reorganizational approach can address the remaining needs and you pretty much have all the pieces. It's just going to require some exercises in restructuring, cutting-back, rewording, and some figure-and-table tweaks.

P.S. "Real quick before my more formal comments and suggestions:"

I likely lack knowledge and experience, but I have no issue with the structure being compressional or extensional and I think I may disagree with the reviewers 1&2 on this point. IMO lots of things can happen in the upper plate, locally things can be more pure strike-slip or transtentional and I think this is reasonably well substantiated.

Similarly, my understanding is likely incomplete, I am also not concerned about the mapping of the scarps relative to the older thrust fault trace. That is, I am unconcerned if this is a reactivated fault with a different pathway to the surface than the older trace.

Below I provide a more formal list of my major concerns and suggestions.

Associate Editor Comment 0.1 — The manuscript needs a more thorough scarp source assessment in the main text. As you know, you are the first to suggest this fault is active. Therefore, the burden of proof is relatively high and falls on you to convince your peers that the scarps you map are from a seismic source. Currently, most of the reviewers are both unconvinced and skeptical. I agree with Reviewer 1, the case for the fault being active is primary (i.e., has priority) over the offset measurements and kinematic analysis. Having much of this very important "scarp mapping" topic covered within the supplemental section is insufficient. I think it's possible supplemental text S1 can be refined and woven into the main text, potentially by following some of Reviewer 3's structural suggestions. Once you convince the audience or, at least, appease them with a level of acknowledged uncertainty (specifically, Reviewer 1's point regarding McCalpin et al., 1990), the additional work and analysis can be fairly considered.

All the possibilities of the scarps not being seismogenic features need to be systematically assessed and a thorough evaluation of why you feel the observations indicate a seismogenic crustal fault is the best answer needs to be presented within the main text. Moreover, this more thorough presentation (consideration and discussion) around potential scarp sources needs to have a more prominent position within the manuscript structure.

Reviewer 3 suggests a detailed heading and subheading re-organization of the manuscript's structure. For example, they suggest "the reader would have a much fuller understanding of (and greater confidence in) how offsets were measured on carefully interpreted landforms (Methods) if she had read the detailed description of the mapping along the fault zone before the detailed explanation of the offset calculations." Note the prominent position of the mapping and landform analysis, their proposed sections 3-4. Also, please note their alternative title suggestion.

I do not think you must follow all of reviewer 3 manuscript structure suggestions 100% of the time. I just see several of their points and think it could be helpful for the required reorganization, especially helpful for the scarp mapping portion.

Associate Editor Comment 0.2 — Let the lidar data stand on its own (show close ups of all scarps) and characterize the range front.

The reviews that I have received, and your responses, lead me to conclude that you need to more effectively document that this a regional fault along the entire length of the range front. You haven't characterized the range front. The two site figures presented, which I assume are the best representation of scarps and offset, are not enough to fully convince most of the current audience. The two sites are relatively small and quite far apart, on opposite sides of the range front. Therefore, you need to more convincingly argue that results derived from the two sites are representative for the entire structure. Currently, the audience is left to presume that there was not much observed in between the presented sites, (distance of >80km along strike) to suggest the possibility of a seismic source generating any scarps over the proposed multiple earthquake cycles, which fairly prompts valid concerns.

Associate Editor Comment 0.3 — Consider using the structural reorganization suggestion, #1 above and from reviewer 3, as an opportunity to show off the lidar dataset. Presumably, you were at least partly convinced by the lidar data, so show the audience what exactly convinced you. I agree with Reviewer 2, show more scarps, and complement the data, where appropriate, with views from different vantage points, light source orientations, and surface layers (slope, aspect, roughness, etc.). I think supplementing the new scarp related text with more inset figure examples both in the main text and supplemental section will strengthen the active-fault-scarp mapping arguments.

The extent of your lidar data set, or your study area bounds, is not explicitly shown. IMO the entire study area extent needs to be formally delineated (boxed in). Consider including all the insets areas, with nothing else, just all the inset boxes and subsequent figure numbers. I think a new figure in the main text, the previous S1, with inset boxes for several scarp examples that correlate to the reviewer 3 suggested main text sections 3.1-4, will help guide the audience through the new scarp assessment text (i.e., convince them with figures). To be clear, I am suggesting more than what is currently presented in the main text and supplemental section. Maybe show all the fault related scarps the main text, the best examples of non-fault scarps in the main text, and extra/other non-fault scarps in the supplemental.

Associate Editor Comment 0.4 — Provide a detailed explanation of how the piercing lines were drawn and offset measured in the main text and re-evaluate the level of uncertainty included here.

Reviewers 1 & 2 provide alternative interpretations of what is presumably the best offset example, Fig. 3b, because they are skeptical of your measurement and interpretation, which in turn prompts them to question every offset measurement. Why are they skeptical? Why do you not follow the "rule of v's"?

Please explain to us, in your response and in the revised main text, why your way is most correct, accurate, robust, and thoroughly accounts for the appropriate level of uncertainty. Your revisions and responses here need to suppress skepticism.

The offset measurement approach applied here is typically applied in places where it is well understood that the fault is active, and/or it's been trenched. Unfortunately, you don't have that a-priori luxury. Therefore, you are asking the audience to solely rely on your mapping and measurements, which is the reason why your presentation needs to be transparent and comprehensive.

Let the data stand on its own and show it to the audience. I agree with reviewer 1&2, show every offset measurement (likely some in the supplemental) at a scale that the audience can evaluate the measurement, e.g., closer to the scale in Fig 3.b. That is, Figs. 3d, Fig. 5a/c/e, and Fig 6c need be presented at scale were the audience can decern the offset measurement. In doing so, please reconsider the presentation of Figs 5 and 6. Please consider increasing the transparency (perhaps just beyond the transparency of 5b) of the unit colors.

Associate Editor Comment 0.5 — Temper the 3-event argument.

I agree with Reviewers 1&2, the 3-event argument is speculative, missing uncertainty statements, and should be tempered. In you revision be sure to acknowledge and account for the uncertainty described by the reviewers.

Reply:

Dear Editor,

Thank you for your consideration of our paper and for your comments.

Below we describe the major changes to the manuscript summarized into the following main categories (following your guidance):

- Text reorganization and reduction of repetition

We have significantly reorganized the manuscript, following the helpful suggestions of Reviewer 3, who requested that we merge together the previous methods and the results sections into singular sections based on the methods used. Following the Introduction (Section 1) and Background Sections (Section 2), the new Section 3 titled "Approach and Methods" describes each of the methods and results in the following 4 Sections: Mapping of Quaternary stratigraphy and landforms in Section 4; Quaternary Scarps in Section 5; Bedrock Mapping in Section 6; and Quantifying slip across scarps in Section 7. The final Section (Section 8) describes our interpretations and implications of the data presented in the previous sections.

In addition to this major reorganization, we also reduced repeated sections, and expanded sections requested by reviewers, which we describe in more detail below. We hope that these major revisions make our methods and interpretations more clear, and that they also reduce any miscommunications that may have existed in prior drafts.

- The manuscript needs a more thorough scarp source assessment in the main text.

We have made major changes to the manuscript to demonstrate more clearly the rationale and evidence that the identified scarps must be of tectonic origin. These changes include the addition of text in the new Section 5 that presents our methods and interpretations of the topographic scarps. In this section, the first paragraph describes the methods and datasets used in the initial identification of topographic scarps. The second paragraph includes detail about how we ruled out whether or not the identified scarps were tectonic. The next paragraphs in this section 5 present the results of the scarp mapping. The last paragraph of section 5 revisits why we interpret the scarps to be of tectonic origin.

We return to this topic in Section 8, where we present several arguments for why the scarps we have identified must be produced by slip on a surface fault. This section 8 also includes an expanded discussion about why the scarps appear to be discontinuous along-strike, and we present examples from other known active faults with similarly discontinuous scarps on lines 399–408.

Following your suggestion, we have also added examples of non-tectonic scarps into the supplemental materials sections (Figure S4) and referenced them in our new paragraph on how non-tectonic scarps were distinguished in section 5. We have added a sentence in the main text at lines 242–243 and in the caption of Fig. 4 that states *"Examples of non-tectonic landforms are presented in Supporting Information Figure S4."*

More detail on the changes we have made to address this issue are listed farther below under each reviewer's comments.

- Let the lidar data stand on its own (show close ups of all scarps) and characterize the range front.

As we mentioned in our recent correspondence with you, showing close-ups of all of the lidar data unfortunately would not be beneficial to the reader. Although we used the lidar data to identify the field sites, the right lateral slip on the Beaufort Range fault is not clearly visible in the lidar DEMs available for this area. The lidar we use in our study was collected by logging companies for forestry management, and the ground returns are not of the resolution and quality typical for datasets collected for fault mapping purposes. Ground returns are unevenly distributed (e.g., Supporting Information Figure S4), and it is common for ground returns to actually be returns from tree trunks and downed logs on the forest floor. In addition, the lateral distance between ground returns (sometimes only 1 or 2 returns per 10 m) can be wider than the channel troughs or interfluve crests that are laterally offset. Thus, a hillshade or contour map of the lidar DEMs is insufficient to consistently capture the exact position of the laterally offset channel thalwegs and interfluve crests, and due to their low resolution, can yield DEM images that appear 'unconvincing'. Therefore, although we very much appreciate reviewers 1 and 2's concerns that the offset data be robust, convincing and reproducible in the manuscript, the lidar data cannot show this signal.

In order to make these points more clear to the reader, we have made the following adjustments. We expanded our text in Section 5 to clarify that we primarily used the lidar data to select areas to further study. The lidar was most useful in identifying potential scarps that we then visited in the field and surveyed to measure offsets. We also now more directly state in section 7.1 that while the lidar data were helpful for identifying scarps, the data unfortunately are of insufficient quality to map piercing lines and measure fault offsets. In addition, we reference Figure S4 in the Supporting Information, which shows an example of the poor ground point density.

The majority of our interpretations are based on field observations, detailed field mapping, and manually-surveyed topographic field surveys of offset geomorphic piercing lines. To help further address the reviewer concerns, we added topographic survey points that we measured in the field to Figure 3b. This figure now shows an example of the lateral component of an offset geomorphic piercing line, to support our field observations (examples of which are provided in Figure 7). Finally, we have added a new supplemental Figure S9 showing the lidar DEMs with the location of the 3D surveys overlain to support our offset interpretations.

Regarding characterization of the range front, we have made the following changes. First, we now include the lidar footprint in Figure 2b as you suggested. Second, we have amended Figure 2b so that it shows more clearly the entire trace of the fault and better characterizes the range front with respect to our data and our inferred fault trace, including mapped scarps in the ∼20 km between Sites 1 and 2. Third, we include a large figure in the supplemental (Figure S1) that shows the rangefront and all of our mapped scarps in much higher resolution than is possible in the main text.

We discuss this topic in more detail in our responses to the reviewers following their specific comments.

- Provide a detailed explanation of how the piercing lines were drawn and offset measured in the main text and re-evaluate the level of uncertainty included here.

As we described above, it would not be beneficial to the reader to use the lidar to show the offset measurements. In order to demonstrate this more directly, we have added the following text to the new Section 7.1 to explain why we used surveys to calculate displacements instead of relying on the lidar (lines 319–325):

"Although lidar DEMs proved instrumental in identifying scarps, these data were of insufficient quality to delineate piercing lines and measure displacements with confidence. Dense undergrowth prevented the lidar from reaching true ground, and large trees blocked returns for areas up to 10 m across (Figure S8). Uneven return spacing precluded the use of DEM backslipping techniques to quantify displacements (e.g., LaDiCaoz; Zielke and Arrowsmith, 2012). We therefore collected topographic profiles in the field with manual surveying using Nikon XS and Spectra Precision Focus 6 total stations, and used the data to reconstruct fault slip vectors at Sites 1 and 2."

As we have already stated above, although showing the lidar data for each measurement would not be useful, we have instead added figures with all of our surveyed points from 3D profiles. These survey points are shown in the main text in Figure 4b, and all other remaining survey points are now included in a new Figure S9 in the Supplementary material. As requested, we have also increased the transparency of our mapping layers on Figures 5 and 6.

We have also included an expanded description of our surveying methods, slip vector calculations, and uncertainty in the main text Section 7.1 and new Supporting Information Text S3. We include a brief explanation of these methods and their associated uncertainties below.

The majority of the primary topographic data for the slip vector calculations are derived from manually-surveyed total station measurements (n=24), while another five were based on remote lidar data where access in the field was not possible. In the text, we now discuss measurement uncertainty on the XYZ topographic positions surveyed with a total station in the field in Supporting Information Text S3 on lines 70–73. Each surveyed point is the average of three laser pulse returns; each typically has uncertainties of less than one centimeter. This measurement uncertainty is an order of magnitude smaller than any interpretation uncertainty or variability in the ground surface, as we now state in Supporting Information Text S3 lines 70–71. We took the same multiple-user approach with the lidar-derived profiles (n=5), which have additional uncertainty from the uneven distribution of ground returns, which we discuss in Supporting Information Text S3 and show in Figure S8.

The next step in our slip vector analysis was designed to use these high-resolution topographic profiles to estimate the exact position of the thalweg or interfluve in the landscape and any associated uncertainty. We recognize that there can be some ambiguity and uncertainty in the position of the thalweg or interfluve (e.g., Zielke et al., 2010; Scharer et al., 2014) and that post-event processes such as erosion and deposition could alter the measurable earthquake-related offsets (e.g., Reitman et al., 2019). In addressing this uncertainty, we follow an approach similar to that used by Zielke and Scharer. We asked several different users (n=5) in addition to the surveyor in the field (n=1) to select the "best" field-surveyed topographic points to use in the calculation of offset considering several factors, such as whether or not there was erosion on the scarp crest, the presence of deposition against the scarp face, or locations where the thalwegs or interfluve crests were non-linear. Each user therefore selected their own interpretation of the "best" survey points. For each of these 6 "best" interpretations of the channel thalweg or interfluve crest on either side of the scarp, we calculated a linear regression and determined 95% confidence intervals for the parameters of each regression.

The final steps in the slip vector analyses combined the results from the linear regressions of the topographic data with estimates of fault geometry. We performed a Monte Carlo simulation to assess slip vectors and uncertainty. The simulation uses randomly drawn (a) offset thalweg and interfluve regressions from the 95% confidence interval of the regression parameters, and (b) fault plane geometries from the 2σ uncertainty of fault strike and dip determined from the 3-point calculations.

We ran a total of 600 Monte Carlo iterations for each offset profile, with 100 runs for each of the 6 "best" piercing line interpretations at the 35 profiles across the scarp. We report the mean and standard deviation calculated from the suite of Monte Carlo iterations in Table S2, show the uncertainty as 2σ error bars in Figure 9, and now state this more clearly on lines 351–358.

- Temper the 3-event argument.

We have added text to Section 8.2 to address the assumptions that go into our 3-event argument, and explain in more detail the multiple lines of reasoning that each suggest three or more earthquakes. On lines 426–430 and 439–442, we discuss the differential offsets of different age landforms, which is illustrated in Figure 10.

"The differential offset between interfluves, channels, and fluvial terraces suggest the occurrence of at least three earthquakes since the deposition of Qp1 at Site 1 (∼13.6-9.5 ka; Figure S2). At Site

2, differential scarp heights of ∼2 m between those developed in till-mantled hillslopes and younger channels incised into the hillslopes suggest at least two surface-rupturing earthquakes have occurred at Site 2 following the deposition of Qt (∼13.6-11 ka).

"If the average difference we observed in cumulative oblique slip between interfluves and channels (1–2.4 m) is the result of at least one event, we can estimate the minimum number of events necessary to produce the total observed slip in the oldest offset deposits. If one earthquake produces 1–2.4 m of slip, the cumulative oblique-slip offsets of 10–15 m may be the product of three or more earthquakes since ∼11–14 ka."

This argument is based on the differential offsets depicted in Figure 10. We elaborate on this further in lines 431–435:

"While the difference in cumulative vertical separation between landforms of different ages is relatively small, on the order of 1–2 m, this difference is reproducible across 8 out of the 12 measured profiles spanning ∼800 m in distance at Site 1. These 1–2 m differences in offsets are an order of magnitude larger than the absolute measurement uncertainty of the total station primary topographic data (¡1 cm) or the inherent roughness of the forest floor (typically 10s of centimeters but rarely at most 50 cm)."

We further compare these differential offsets to scaling relationships on lines 442-445:

"This inference is corroborated by displacement length scaling relationships. For faults with lengths of 35–100 km, as we map for the BRF, a single earthquake is predicted to result in a slip of 0.4–3 m (Wells and Coppersmith, 1994; Wesnousky, 2008), and thus our total offsets of 10–15 m are consistent with three or more events."

Finally, on lines 459-464, we specify the assumptions inherent in our differential offset interpretations, and summarize each line of evidence for multiple events:

"We note that the above estimates rely on the assumption that landscape adjustment to fault offset occurs at similar rates on interfluves and channels. It is possible that offset magnitudes in channels underestimate total slip due to differential scarp degradation and sediment ponding, and may not solely be the product of slip during different numbers of events. We note, however, that even if we discount the differential offset of interfluves and channels, the total scarp height and oblique slip in interfluves, displacement-length scaling estimates, and differences in offset between Qp1 and Qt depositions, still suggest multiple earthquakes on the BRF since the Late Pleistocene."

We hope that the text above clarifies the major changes that we have made. Below we further respond to each of your and the reviewers comments in turn.

Below I provide more minor comments:

In your Jan10 version there were 79 uses of "this" and 62 uses of "these." In your June 28 version there were 80 uses if "this" and 45 uses of "these." Please address the use of "this" in the fashion that you previously addressed the use of "these."

Reply: Our search shows only 43 uses of "this" in the June 28 version of the text. The remaining 37 uses are in the header "This is a non-peer reviewed manuscript submitted to Seismica" at the top of each page. We have further reduced our usage to 39 instances of "this".

I agree with reviewer 3, refine table 1 to only include pertinent information, e.g., only list the dated material and cut the labels down to a more digestible format. Within the table include descriptions, weights, and dimensions of the dated material.

Reply: Thank you for the suggestion. We have simplified Table 1 following Reviewer 3's suggestions, and added the available descriptive information.

Also, I agree with reviewer 3, the 1946 rupture does not need to be summarized in your section 2.

Reply: We have removed this summary from section 2.

Response to the reviewers

Reviewer 1

Dear Author(s),

I have re-reviewed your revised manuscript "Late Pleistocene to Holocene transtension in the northern Cascadia forearc: Evidence from surface ruptures along the Beaufort Range fault". Unfortunately, I do not think this manuscript is ready for publication for several reasons that I explain below.

Reviewer Comment 1.1 — First, and most important to what I think needs to be reconsidered, is that interpretations are being conflated with data. You have mapped a series of newly identified scarps (Figs. 4a/b) that you are interpreting as fault scarps. Stating in your methods that you 'only mapped faults scarps based on some criteria' and that you 'visited each scarp in the field to confirm,' doesn't convince me that these are indeed fault scarps. My speculation is generally based off the McCalpin et al. (1990) report (which you reference), and where within Appendix A-120 discusses the "Criteria for Determining the Seismic Significance of Sackungen and Other Scarp like Landforms in Mountainous Regions" – a similar setting to yours that provides comparable examples. McCalpin's main point in his discussion is that it can be very difficult to discern between tectonic and hillslope (sackungen/landslide) scarps and it is often never clear – even within trenches and analyzing every geomorphic aspect! The discontinuous nature of the individual scarps along with their relative offset/length ratio and broad/wide distribution across the hill slope makes me skeptical on first-order. Furthermore, there is a long swath (∼100 km) between Sites 1 and 2 where there are few to no scarps mapped. In my opinion, many look like they could be hillslope failure, bedrock scarps, and/or bending moment faults (see next paragraph). However, the scarps are presented as tectonic in the beginning, as data, and later characterized as if they are of fault origin. The main thesis and focus of this paper should ask whether or not these scarps are tectonic. While the kinematic analysis appears sound, it relies on these scarps being tectonic, and to me it's not 100% clear; thus, the broader tectonic implications you discuss, which would have large regional seismic hazard impacts, may be false and all for not. As written, the foundation of this paper is weak.

Reply: Thank you for pointing out that our previous arguments were not as strong as we intended. As we discussed above, we have now reorganized the paper and added text to the new Section 5 where we discuss our reasoning for interpreting the faults as tectonic, and why this is the best interpretation that fits our field observations and map data. In that section, we describe our rationale for how we distinguished between the tectonically-produced scarps that we observed and other non-tectonic features:

"Topographic landforms that we determined to be of non-tectonic origin include logging roads, landslide scars, sackungen, glacially striated, scoured, and plucked surfaces, and scarp-like land-

forms formed by differential erosion. Logging roads typically exhibit a flat base with an oversteepening of the lateral flanks. Landslide head scars or "toes" are typically curvilinear and often associated with hummocky deposits and disrupted topography. Scarps formed by sackungen (e.g., Figure S4b) typically occur parallel to topographic contours near the top of the range (elevations of >1300 m) in parallel linear sets <500 m long. Neither landslide head scars nor sackungen typically extend across multiple hillsides or drainages. Glacially streamlined deposits and glacially-plucked surfaces tend to have an asymmetry axis parallel to the ice transport direction (∼120° azimuth), and the elevation of crests of glacial lineaments typically decreases in that same direction. Scarp-like landforms formed by differential erosion are co-located with steeply dipping bedding planes or changes in lithologic strength across Karmutsen formation flow tops."

We further state: *"Field observations show consistent vertical and lateral offsets of geomorphic piercing lines across scarps at multiple independent sites, regardless of local topographic slopes, elevations above the valley floor, or bedrock versus Quaternary substrate. Mapped scarps clearly project into adjacent scarps across gaps and stepovers, occur in en echelon and parallel arrays, and span several tens of kilometers of strike length."*

Further, we would like to point out that there are only ∼20 km between Sites 1 and 2 (not 100 km), and there actually are scarps mapped in that stretch that we hope are now more clearly visible on Figures 2b and S1. As we mentioned above, we also include new text that describes how discontinuous scarps with similar lengths and gaps are common along faults with low slip rates, and in particular in erosive environments where scarp preservation is expected to be poor and is likely highly dependent on lithology, slope, climate, vegetation, and other factors. Our discussion of these topics on lines 410–417 is below:

"The discontinuous nature of the mapped strands is likely due to the limited preservation potential of scarps that are developed in unconsolidated material, and occur in the steep terrain and wet climate characteristic of the Beaufort Range (e.g., Reitman et al., 2023), and the glacial history limiting preserved deposits to the past ∼14 ka. Similar discontinuous and distributed rupture patterns have been commonly documented elsewhere in the Cascadia forearc (e.g., along the North Olympic, Darrington Devils Mountain, Seattle, Leech River, and Boulder Creek faults, Personius et al., 2014; Sherrod et al., 2013; Nelson et al., 2003; Morell et al., 2017; Schermer et al., 2021), and in historical ruptures in subduction and non-subduction settings (e.g., Yuan et al., 2022; Li et al., 2012; Biasi and Wesnousky, 2016; Koral, 2000; Ainscoe et al., 2019; Bawden 2001; Rodríguez-Padilla et al., 2024)"

We have followed your suggestion in the line comments below to rephrase our methods such that we are assessing the origins of scarps, rather than confirming our lidar-based interpretations.

Reviewer Comment 1.2 — I also see an issue with the logic for redefining the trace of the Beaufort Range fault (BRF) and argument against the possibility of these scarps being fold-accommodation bending moment faults. The prior mapping of Cui et al (2017) define the bedrock trace of the BRF by the contact between the Karmutsen Fm and Nanaimo Group, which appears to mostly be covered by Quaternary – and is thus concealed and could be drawn differently as long as its somewhere between where these two units are mapped. The majority (∼90%) of the scarps mapped (shown in Fig. 4a/b) are within the HW of the Cui et al. (2017) trace. In the paper, you redefined the mapped trace of the BRF based on your scarp mapping and have argued that these scarps cannot be fold-accommodation faults (bending moment faults) because of where this new fault trace is now mapped. THIS IS CIRCULAR REASONING - Of course the scarps lie within both the HW and FW!!... Furthermore, in Fig 4a the new trace intersects mapped bedrock of the

Karmutsen Fm. and where Nanaimo is absent – the fault, as defined in the text (lines 118-119), should be the contact between these two. I see no need to redefine the BRF as no other observations are provide in the text of where the fault is exposed or expressed in the bedrock. So, I see no need to move it. This bedrock fault still exists within the bedrock (Cui et al. 2017), and if these are fault scarps, then they are likely part of the evolution of the BRF thrust system and not something different. I recommend looking at the rupture pattern of the El Asnam 1980 earthquake in Philip and Meghraoui et al. (1983) (see Fig 2 of their paper below). These look like similar patterns to me. Lastly, there is no structural context (axis) to the fold geometry provided other than the steep and opposing dip measurements provided in Fig. 4a (NE edge of map). The axis does not have to be at the top of the BR and the dips suggest a tight fold near the main fault trace to me…

Reply: As we understand the Reviewer's comment, they infer that we did not use bedrock mapping to define the position of the fault contact between Karmutsen Fm and Nanaimo Group. However, we would like to clarify that we did in fact use field outcrops (where available) to define the position of the fault contact between the two bedrock units. Reviewer 1 states that "In the paper, you redefined the mapped trace of the [bedrock] BRF based on your scarp mapping". We clarify that we did not use the position of the bedrock fault to inform scarp mapping or vice versa. Instead, we refined the positions of previously mapped bedrock fault traces (positional differences of meters to 10s of meters) based solely on our own bedrock mapping, and exposures of the bedrock fault and damage zones. We agree that re-mapping the bedrock fault based on scarp mapping and surficial deposits would not be logical.

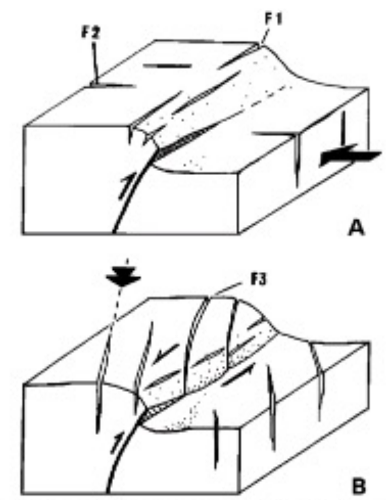


Fig. 2. Sketch view of the surface breaks in the southern part of the fault (zone A). (a) With no horizontal component. (b) With some horizontal components. F_1 extrados cracks, F_2 and F_3 tensile craks due to a secondary extension.

Philip and Meghraoui (1983)

To help clarify these points, we have added text to a new Section 6 detailing our bedrock mapping methods, including text to clarify that we did use fault zone outcrops in our mapping—at Sites 1.1 and 2.1, where we observed the lithologic fault contact between the Karmutsen Fm. and Nanaimo Gp. Elsewhere at both Sites 1 and 2, where the fault contact was not exposed, we were able to locate it within 10s of meters based on exposures of the two lithologies.

Reviewer 1 also notes that our bedrock fault trace exists within the Karmutsen Fm in Figure 4a (now Figure 3a). This fault is the upper of two thrust branches, and thrusts a lower portion of the Karmutsen Fm over a high portion of the Karmutsen Fm. The lower thrust fault branch similarly thrusts Nanaimo-on-Nanaimo, and a sliver of basal Nanaimo deposited on Karmutsen is caught up between the thrust branches. We note that both of these faults were already included in the Cui et al. (2017) mapping. Our mapping of the two bedrock fault strands at Site 1 simply updates the locations of these traces, as we now state more clearly in section 6 (lines 288–289). *"We used bedrock outcrops, and outcrops of the fault zone itself at Sites 1.1 and 2.1, to refine and update the mapping of the location of the Eocene bedrock thrust fault (solid teeth in Figure 3; c.f. Cui et al., 2017)."*

We have added additional text to Section 6 describing our bedrock mapping results, which more explicitly describes the folds in the Nanaimo Gp. We do include fold axes on the stereonets in Figure S6, and have added text to the caption that reflects our belief that the fold axes of the footwall synclines are located near the main fault traces. We agree with Reviewer 1 that the dips at Site 1 suggest a tight syncline near the main fault trace—that is our interpretation as well, as we now state in new section 6. In addition, we note that our prior version of the manuscript from June 28 never stated there was a fold axis near the top of the Beaufort Range, as implied in the reviewer comment above.

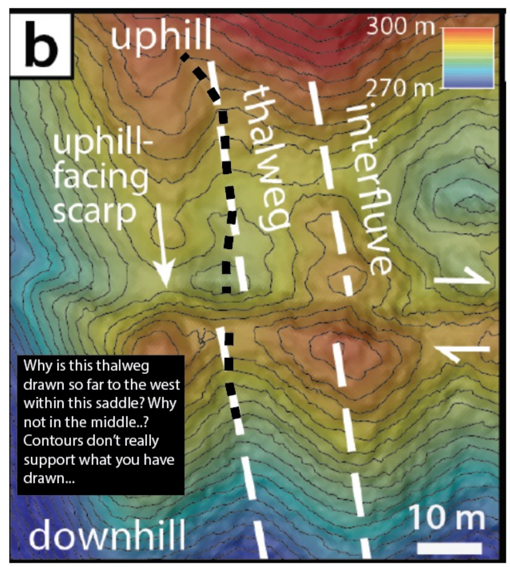
In Section 8 we describe our rationale for why the scarps that we define are not bending moment faults, where we state

"We consider the possibility that the active BRF could be a bending moment fault accommodating extension in the shallow layers of active, thrust fault-related antiforms due to thrust slip on the Eocene fault (e.g., Philip and Meghraoui, 1983). Our field observations, however, preclude this scenario because we found no evidence of antiformal folding of Karmutsen, Nanaimo, or Quaternary formations, nor did we find evidence of thrust-sense offset of Quaternary deposits anywhere along the fault."

We further discuss the two scenarios that we consider more likely:

"Instead, we consider one of the two following scenarios more likely. The active BRF could reactivate the Eocene fault if steeply dipping (60°-80°) BRF faults in the near surface merge with the more gently dipping (45°-60°) Eocene fault at depth. Assuming that the active BRF is right-lateral oblique, this scenario requires that the bedrock thrust fault has changed slip sense since the Eocene, and now accommodates right lateral transtension. Alternatively, the subsurface projections of the steep active BRF and more gentle Eocene bedrock faults could diverge at depth."

Reviewer Comment 1.3 — Secondly, I am skeptical of the RL offset and measurements based off the example you provide in Fig. 3b. There are three issues I see with this: First, the thalweg and interfluve lines are drawn very straight (not real) and are not following the contours. Thus, I'm not confident in the RL offset that are portrayed. I don't see any support in the contours for the thalweg line on the uphill side of and near the scarp in Fig. 3b. Based on the contours shown, it should be drawn through the middle "low point" of the closed contour (depression) just before the scarp. Please see my alternative interpretation below. There is no reason or evidence provided on why it is to the right of the depression. Furthermore, I think the correlative thalweg on the south side of the scarp is needlessly drawn to the left (west..?), where it should be closer to the middle of the saddle. It appears the RL is exaggerated here and makes me skeptical of all the other lateral offsets. Finally, it is not clear to me on how uncertainty was assigned to these measurements. I see there are error bars in Fig. 9 but its unclear how those were assigned. Given that Fig. 3b is only example provided of your horizontal measurement, I question the rest because lateral channel offset is not that apparent to me at the scales provided in Figs. 3d, Fig. 5a/c/e, and Fig 6c.



My channel thalwegs in black.

Reply: We have already partially addressed this point in our responses above. The survey points that we now show on Fig. 4b show exactly where we stepped in the field to outline the piercing points, and where we visually defined the local thalweg or interfluve crest with the aid of at least two field scientists.

Given the limited quality of the lidar data due to gaps in ground returns or false ground returns (as discussed in section 7.1 and shown in Fig. S8), the offsets that we were able to discern easily in the field are not easily conveyed on the lidar maps. Thus, the field-surveyed thalwegs and interfluves do not always visually match up with the "V"s in lidar contours perfectly. Figure 7 shows three field photos of laterally offset channels, which are primary data supporting lateral offset observations. We hope that this field figure, together with Figures S9 and 4b that show our surveyed points for all 3D surveys, and the new text and figures highlighting the poor quality of the lidar, better illustrate these points.

The dashed lines in the previous figure 3b (now Figure 4b) that you highlighted previously were not intended to show our precise thalweg and interfluve crest locations. In order to avoid confusion, we have therefore updated this figure to show the location of our surveyed points.

The uncertainties assigned to the error bars in Figure 9 were determined using the results of Monte Carlo simulations (SCARP-3D), which account for multiple interpretations of the geometry of the offset piercing lines and multiple possible fault geometries. The offsets and error bars in Figure 9 represent the mean and 2σ uncertainty derived from 600 calculations of offset. We refer the reviewer to our response to the editor above, to the edited text in Supporting Information Text S3, and edited text in Main Text Section 7.1 for additional details on how we address uncertainty in piercing points and how that propagates through to our offset measurements.

Reviewer Comment 1.4 — In relation, as I appreciate and understand your response to my concern with comparing scarp heights/offset of the thalwegs with the interfluves to interpret multiple event, I still disagree – even with the far-field projections. The channel profile would change during the scarp forming event as a result of base-level change, triggering progressive aggradation uphill and incision downhill of an uphill facing scarp. Yes, your far-field measurements may help capture these effects, but they are occurring at different rates within the thalweg than the interfluve. If anything, the uncertainty of this effect needs to be considered, but would be difficult to quantify.

Reply: Thank you for your comment. We have tempered this argument with the added text to lines 459–462: *"We note that the above estimates rely on the assumption that landscape adjustment to fault offset occurs at similar rates on interfluves and channels. It is possible that offset magnitudes in channels underestimate total slip due to differential scarp degradation and sediment ponding, and may not solely be the product of slip during different numbers of events. "*

Reviewer Comment 1.5 — So, I've discussed the main issues that I am still seeing with this manuscript, there are other more minor issue that remain and comment on in the text and response letter. These are intriguing scarps that could potentially be tectonic, but I don't think your data supports this to the confidence level you are portraying. There needs to be a clear distinction between data and interpretation and it's not done here.

Reply: We have re-read the manuscript with an eye to differentiating data and interpretation. Some of this has been addressed with our reorganization following Reviewer 3's suggestions (e.g., new interpretations-only section 8). Elsewhere, we have tried to distinguish data from interpretations by clearly prefacing interpretations with phrases like "We interpret..." or "This suggests..." and similar indicators.

I thought I made this point clear in my first review, but it seems it didn't take with this revision or I was not clear. I hope a more objective approach is taken with these scarps in a revised manuscript. I recommend look at some other publications that take such an approach (e.g. Jobe et al., 2024), an offer alternate hypothesis of what they could be and discuss each one with more general implications for each. Please feel free to reach out if you would like to discuss anything further.

Reply: We hope that the edits we have made and describe above sufficiently address uncertainties in scarp sources.

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References

- McCalpin, J.P., 1999, Appendix A, p. A-122 to A-142 in Techniques for identifying faults and determining their origins, U.S. Nuclear Regulatory Commission, NUREG/CR-5503.
- Philip, H. and Meghraoui, M., 1983. Structural analysis and interpretation of the surface deformations of the El Asnam earthquake of October 10, 1980. Tectonics, 2(1), pp.17-49.
- Thompson Jobe, J., R. Briggs, R. Gold, L. Bauer, and C. Collett (2024). Limited Evidence of Late Quaternary Tectonic Surface Deformation in the Eastern Tennessee Seismic Zone, United States, Bull. Seismol. Soc. Am. 114, 1920–1940, doi: 10.1785/0120230094

Line-by-line comments

Reply: We have made most of the small wording and grammatical edits suggested directly within the annotated PDF. We address the line-by-line comments and questions below.

Line 84-88: I have made suggestion here of how to make the thesis of this paper less presumptive:

Here, we present the first field-based tectono-geomorphic data that documents Quaternary slip along the BRF. We map multiple generations of ~~offset~~ late Pleistocene to Holocene deposits **along the BRF**, and measure the displacement of geomorphic piercing lines along >20 km of ~~fault strike~~ **scarps that manifest within these deposits**. These data ~~demonstrate~~ **indicate these are fault scarps, demonstrating** that the BRF is an active fault that has accommodated right-lateral oblique slip rates of ∼0.7-1.3 mm/yr since at least the latest Pleistocene, ~~displacement rates that are~~ **placing the BRF** among the fastest of any active fault in the northern Cascadia forearc.

Reply: Thank you for the suggestion. We have made a combination of your and Reviewer 3's suggested edits to this paragraph.

Line 98: Suggestion: placing an arrow to represent this motion of the Explorer plate would illustrate this well in Fig 1

Reply: Thank you for the suggestion. We have added an arrow to Figure 1 to show Explorer plate motion.

Line 100-103: This is quite a long list. I recommend removing referenced abstracts. I think Wells et al 1998 would suffice alone

Reply: Thank you for the suggestion. We have removed the references here and instead only refer to Figure 1, the caption of which includes relevant references.

Line 111-115: *"We note that the mapped scarps occur adjacent to a bedrock thrust fault, previously named the Beaufort Range fault, that strikes northwest-southeast, following the southwestern topographic range front for >40 km (Figure 2b). In this paper, we refer to the thrust fault as the Eocene bedrock thrust fault, to distinguish it from the active strands we investigate herein (the BRF). We discuss the potential relationships between the active BRF strands and the Eocene bedrock thrust fault in Section 5.2."*

Ok - this is confusing and (I think) were I see some potential misunderstanding of what your trying to do. It seems you are reassigning the name "Beaufort Range fault" from the fault that, as you state, has previously defined within the bedrock, to the scarps you've mapped. Here it seems you are trying to disconnect these scarps from the bedrock fault... Hard to imagine that these scarps are assciated with a completly different fault system. Perhaps a better way to frame the difference you appear to be making is to understand the relationships of these scarp to the predefined bedrock fault that is the BRF.

Also, I again think you are putting the 'cart before the horse' here by assuming these scarps are actual fault scarps. This is just the background and the reader has not yet seen these scarps.

I think one of the main goals of this paper/study, is to, first, show with your data that these are actual fault scarps and not something else. That has not been done yet in this point in the paper. Also, given that you are interpreting these as fault scarps and that the "bedrock fault" dips between 45 to sub-vertical (underlined below), wouldn't these be the faults scarps of the BRF rather than something different?

Reply: In our reorganization, we have placed the mapping and interpretation of fault scarps (new Section 5) before the mapping and interpretation of the bedrock (new Section 6).

We discuss the potential relationship between our mapped fault scarps and the bedrock fault in our new Section 7.1.

Line 121: Suggest not approximating when using less (or greater) than.

Reply: We have made the suggested edit.

Line 121-122: *"Flow tops in basalts and bedding in the Nanaimo Group dip gently ($\sim<20°$) toward the southwest in the hanging wall of the Eocene bedrock thrust fault (Figure 4)."*

Given this description of the Nanaimo Grp and its structural position (FW) - I would be cautious of these scarps being bedrock erosional scarps. See McCalpin 1990 Fig.

Reply: We visited these scarps in the field, and confirmed that they offset Quaternary deposits, as discussed in new Section 5. Therefore, they are not solely the product of bedrock erosion.

Line 156: are there bedrock shear zones?

Reply: Yes. We used the many bedrock shear zones exposed in roadcuts, rivers, etc. to constrain our mapping of the bedrock thrust fault (solid teeth on Figure 3). We have added new Section 6 describing our bedrock mapping methods and bedrock mapping results.

Line 161: Suggest being consistent with your terminology. You already defined fault scarp above. Are there other fault-related landforms your looking for?

Reply: Thank you for the suggestion. We have updated this line to refer to fault scarps.

Line 169-175: *"We visited each accessible remotely-mapped scarp to confirm they were tectonically-generated features (i.e., not related to slumping, etc.). Criteria used to distinguish fault scarps from other features include whether the features are linear; continuous over >50-100 m length scales; cut across topography; and offset hillslopes, abandoned channels, or interfluves (Figure 3b-c). We took care to distinguish potentially fault-related scarps from landforms produced by glacial deposition or scour, differential erosion, anthropogenic disturbance, gravitational failure, in part by comparing scarp orientations with glacial and bedrock mapping, identifying oversteepening of the slope below roads, and mapping curvilinear headscarps and landslide toes"*

With this paragraph, you are telling the reader "trust me, the scarps that I've mapped are indeed 100% fault scarps - I've visited them myself." Prior studies (e.g. McCalpin et al. 1990) have shown the challenges in assigning the origin of scarps in similar settings just as this. I highly recommend you alter the approach of this section to "we mapped and characterize scarps that parallel the BRF to asses their origin.

paper so that data and interpretations are not conflated.

Reply: We have rephrased this sentence as suggested.

Line 183-184: *"Eocene bedrock BRF"* Confusing terminology. Earlier you redefined the previously mapped BRF fault within the bedrock to the "bedrock fault", now your referring to the "bedrock BRF"..? See comment above. Be consistent.

Reply: Thank you for pointing out this oversight. We have corrected this instance to refer to the Eocene bedrock thrust fault, consistent with the other instances throughout the paper.

Figure 4a: Typo - should be Karmutsen Fm.

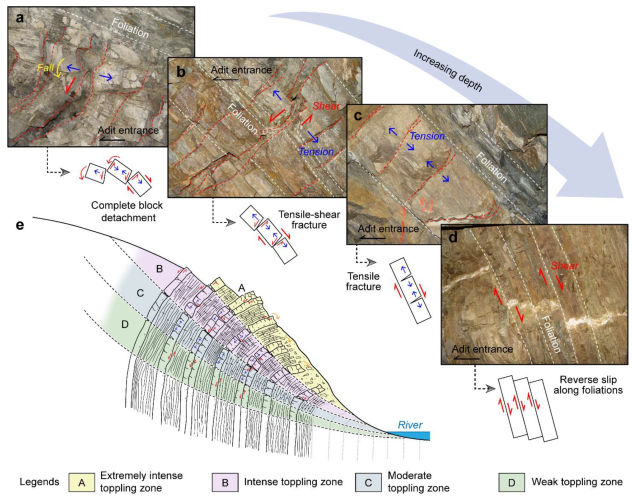
Reply: This is not a typo. At this location, the Nanaimo Gp is thrust over more Nanaimo Gp. NW of this label is the depositional contact of Nanaimo Gp over Karmutsen Fm. We have explained this more thoroughly in Section 6.

Figure 4a: Is there fault exposure here? Why not solid if its a bedrock fault? Shouldnt the fault be the contact between the two bedrock units.

Reply: There is a fault exposure here. The bedrock fault here is not a contact between the two units - it is Karmutsen Fm. thrust over more Karmutsen Fm. - the lithologic contact fault is downstream ~500 m. We show the bedrock fault as a dotted line to indicate that it is covered by the Quaternary deposits we map (i.e., a concealed fault, as is explained in the map legend in Figure 3).

Figure 6: These look like bedrock scarps to me, thus questioning your "fault scarp" mapping.

Reply: We discuss in the text in Section 5 why we interpret these as active tectonic fault scarps. First, and most importantly, our field mapping identifies vertical and lateral offset of Quaternary deposits across these scarps. Therefore, they cannot be the sole product of differential erosion of pre-existing bedrock features. Second, we have evaluated the possibility that they could be "bedrock scarps" formed by a variety of different types of processes and found the field evidence to contradict those alternate hypotheses. For example, the data are not consistent with scarps formed by differential erosion of basalt flow tops or Nanaimo beds. The scarps we map at Site 2.3 are steeply NE-dipping and all but one or two scarps occur within the Karmutsen Fm, which at this site is sub-horizontal or gently SW-dipping. The field data are also inconsistent with the hypothesis that these could be "bedrock scarps" formed by toppling-induced fracture, as in this figure from Huang et al. (2017), because they are underlain by SW-dipping flows.



Huang et al. (2022)

Line 370: *"multi-fault scarp"* poor terminology - unclear. Do you mean a wider zone of fault scarps?

Reply: Thank you for the comment. This sentence is no longer present in our reorganized manuscript.

Line 370: *"emergent"* why the descriptive term here?

Reply: Thank you for the comment. This sentence is no longer present in our reorganized manuscript.

Line 373: *"right-lateral offset"* I question the viability of the presence and measurement quality of lateral offset (see comment above in methodology

Reply: We hope that our additional figures and discussion of our surveying methods give the reviewer more confidence in our interpretation and measurements of lateral offsets.

Line 381: Are you suggesting that gravitational scarps (sackung) cannot have lateral offset across them? I could imaging that horizontal offset could occur across such a feature.

Reply: While it is possible for there to be apparent lateral displacement across sackung, the systematic right-lateral displacement we see of multiple landforms across multiple strands is incredibly unlikely to occur without some lateral component of shear. The simplest and most plausible explanation for our observation is right-lateral slip along a fault.

Line 382-383: *"This sense of displacement is opposite to that predicted for landslide-related failures."* Yes, but there are examples of uphill-facing sackugn scarps (see McCalpin 1990).

Reply: This line is not present in our reorganized manuscript.

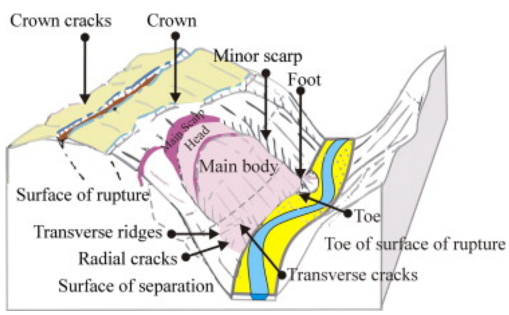
Line 383: *"quasi-linear"* Had to look this up as I have never seen scarps described using this term. Seems like more of a mathematical term to describe a function. Suggest using 'sub parallel to one-another' as this is more geometrical

Reply: We have made the suggested edit.

Line 383: *"several"* can you please define several here. What does your data say? What is the range of your scarp lengths?

Reply: We have rephrased this sentence to read "up to 2 kilometers".

Line 384: *"tend"* key word here - *"curvilinear and discontinuous scarps with limited strike lengths."* what about deep-seated landslides?



Lin et al. (2013)

Reply: From the literature we have reviewed about deep-seated landslides, they typically have curvilinear headscarps with limited strike lengths (e.g., Van Den Eeckhaut et al., 2005; Lin et al., 2013). See Figure 2 from Lin et al. (2013), above.

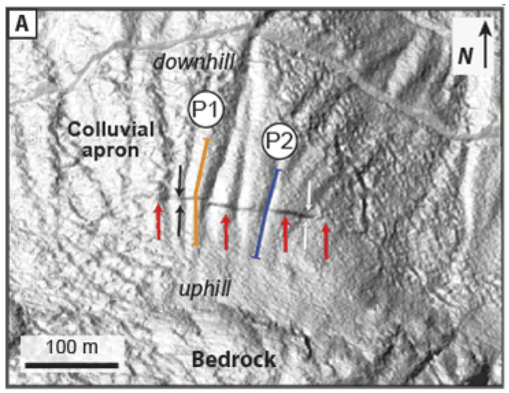
While the "crown cracks" Lin et al. include are relatively straight, they occur within the curvilinear headscarp of the main body of the slide.

Line 463-464: *"The field data and observations provide evidence that the scarps we map along the southwestern flank of the Beaufort Range are tectonic in origin and are associated with an active Beaufort Range fault."* You have already presented these scarps as fault scarp - so why the discussion here?

Reply: In our reorganization of the manuscript, this is now the introduction to our discussion.

Line 465: *"and occur along several tens of kilometers of strike length."* I think this is misleading - Yes, its tens of Km if you add up the distance between Sites 1 and 2, but theres a large gap in-between where there are no scarps. Fault scarps are usually more continuous at such scales.

Reply: We refer the reviewer to our response to the editor above. We now include the following examples of discontinuous fault scarps in the Cascadia forearc on lines 412–417:



Morell et al. (2017)

- the Leech River fault, which has 2-5 km gaps and scarps that are only a few hundred meters long (Morell et al., 2017, below)
- The Boulder Creek/Canyon Creek fault was mapped on the basis of two scarps in Sherrod et al. (2013)
- The Darrington-Devils Mountain fault has >10 km gaps between mapped scarps (e.g., Dragovich and Stanton, 2007; Personius et al., 2014)
- Scarps are extremely limited in the Seattle and Tacoma fault expressions (e.g., Nelson et al., 2003; Sherrod et al., 2004; Angster et al., 2024)
- The North Olympic fault zone has a 14 km gap between the Lake Creek and Sadie Creek faults (Duckworth et al., 2021). The longest gap between scarps we observe on the BRF is also 14 km.

While some authors prefer to make fault maps in which they connect scarps with dashed lines where they infer the fault continues (e.g., Personius et al., 2014), ours is a more conservative scarp map, showing only the mapped scarps. However, we now include our mapped inferred and simplified fault trace on Fig 2 to more clearly show our interpretations.

Line 468: *"fault lengths (>40 km)"* see comment above - these are discontinuous scarps and possibly not one whole fault, so not real evidence - just interpretation.

Reply: We discuss our interpretation of the discontinuous scarps in Section 8.1.

Reviewer 2

Major issues remain unaddressed and so I am recommending the paper be returned to the authors for either major revisions or resubmission after major rewriting. The conclusions and analyses in the paper are not warranted based on the quality of data/observations presented. The evidence of lateral slip in the hillshades, as presented, is extremely weak, yet this remains a central focus of the paper including the bulk of the analyses and conclusions. The uncertainty of lateral offset measurements is not well defined. The tectonic role of this fault remains unexplored and needs to be discussed. The lack of continuity and the limited extent of the mapped fault scarps suggests these may be isolated features related to large scale slope processes (e.g. sackungen) rather than faulting. I discuss these issues in detail in the following list:

Reviewer Comment 2.1 — Offset measurements & uncertainty are not sufficiently characterized. Figure 3b is the only site shown in detail. These piercing lines can be drawn a variety of different ways and the lateral offset could be much less than that presented. This raises question about the interpretation of lateral slip elsewhere along these scarps, which is a central interpretation of the paper. I would like to see a supplemental showing a detailed plot of every lateral slip measurement used along with the uncertainties better indicated (as shown in 3b but including uncertainties in projections). I suggest to use a combination of slope and aspect maps to properly represent the thalwegs & ridges that are used as piercing lines. These lateral displacements should be labeled on Fig 5d. As a huge amount of the paper is based on analysis using these measurements this approach is warranted.

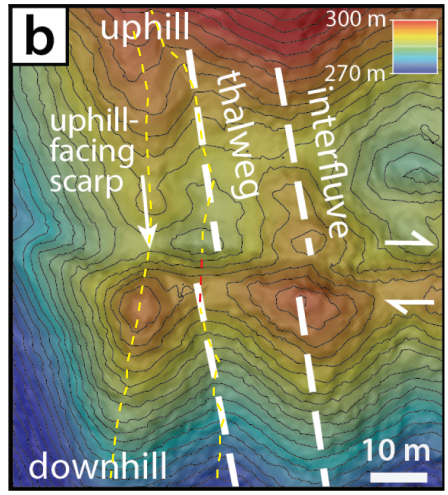
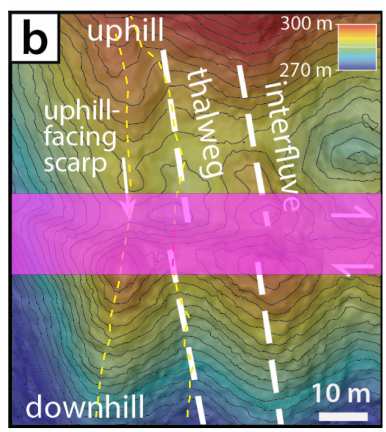


Fig 3b reinterpreted showing much less (none?) lateral offset than the authors propose

I am attaching to the end of this document a reinterpretation of Fig 3b to illustrate this issue in interpretation. In this I have followed the "rule of v's" with the provided contours to try to pick the thalweg/steepest slopes. The result of my reinterpretation shows there can be little or no lateral offset of the channel thalweg. The paper is 38 pages long with 11 figures. Only 3 of these figures (really 2: figs 5c & 6c, as fig 3 is largely duplicated in 5c) show the lidar data that most of the analyses are based on. It should be pointed out that almost any uphill facing escarpment will cause a channel to shift left or right when it intersects the escarpment.

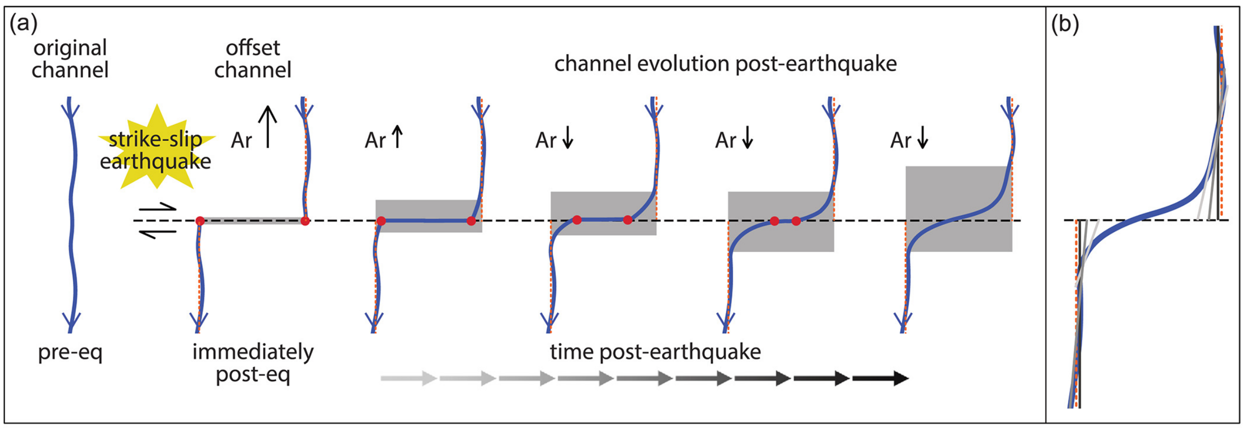
Reply: We have added text to new Section 7.1 to clarify why the lidar of are of insufficient quality to trace piercing lines and quantify lateral offset (see response to Reviewer 1 above).

As we have stated above, our offset measurements are not based on the lidar hillshades. We had included these hillshades for illustrative purposes, but realize now that the presentation may have been misleading. We have now updated Figure 3b to show our surveyed points rather than dashed lines. We would like to refer the reviewer to Figure 7b, which is a photo of the exact channel shown in Figure 3b and shows where we measured ∼2 m of right-lateral displacement.



Reviewer's reinterpretation, without the geomorphic fault zone

We interpret Figure 3b (now Figure 4b) differently from the Reviewer based on several different lines of reasoning. First, as we stated above, our interpretations of right-lateral displacement and offset measurements are based on our field observations and surveys, not on the lidar DEMs, due to the reasons outlined above and in the text. Additionally, the reviewer's reinterpretation appears to rely on this "continuous" thalweg across the scarp. However, our analyses take into account the position of what is termed the "geomorphic fault zone" - where post slip processes modify the geomorphic expression of the fault zone, as defined by Reitman et al. (2019)—see below. As we now state more clearly in the new Supporting Information Text S3, we exclude in our displacement analyses *"points where there may be deposition against an uphill-facing scarp or erosion at a scarp crest."* The multiple geologists' choices of points to include in the profile regression in SCARP-3D result in regressions with varying widths of the geomorphic fault zone. If one excludes the geomorphic fault zone (∼15 m around the scarp, shaded pink) and projects the remaining thalweg into the fault, the reviewer's re-drawn thalweg is right-laterally offset in our interpretation. Finally, we do not disagree that uncertainty in lateral offset and piercing line projections allow for a range of offset values (2.7–3.3 m). These uncertainties (mean and standard deviation of every output from SCARP-3D) are presented in Table S2.



Reitman et al. (2019)

Reviewer Comment 2.2 — The evidence as presented does not provide a compelling justification for the paper's conclusions. Looking at the provided lidar hillshades I struggle to agree with the authors' assertion of compelling & consistent evidence of lateral displacement (and if it isn't compelling and consistent I would argue that the analyses and conclusions are not justified). Why do none of the major drainages have right-lateral displacements in their terraces or channels? Why do some of the major drainages bend left rather than right (e.g. the large channel in Fig 3a situated at the center of the top part of the panel)?

Reply: Our field measurements of lateral offset are focused on minor and abandoned channels which have a higher likelihood to preserve lateral offset. We have documented 14 channels that are right-laterally offset, most by multiple fault strands. We focused our measurements on these smaller channels because the major drainages on the Beaufort Range are extremely active seasonally - and have enough discharge to occasionally move large boulders. Therefore, the larger streams have more than enough power to erode through scarps and straighten out their profiles compared to the slow slip rate on the BRF. We have added this text to lines 435–438.

The "left bend" the reviewer refers to does not occur at the scarps, but rather farther up on the rangefront where it is the margin of a paraglacial alluvial fan, as mapped in Figures 3 and 5d. These larger drainages are also eroding into bedrock and are thus controlled by the numerous bedrock structures within the Karmutsen Fm and the Nanaimo Gp. The signal from active lateral offset in these drainages would be extremely difficult to pick out from the effects that instead result from preferential bedrock erosion.

Reviewer Comment 2.3 — Why are these scarps that purportedly are the result of 3 surface rupturing earthquakes not represented along strike in other similar aged deposits?

Reply: The only units correlative across all the mapped scarps are the Late Pleistocene till (Qt), and we do map scarps across Qt at multiple sites. At Site 1, we do correlate Paraglacial deposits Qp1 (in interfluves) and Qp2 (in channels) across multiple faults. These deposits do show differing amounts of offset. Unfortunately none of the younger terrace deposits mapped are correlative between sites, and they are poorly preserved in most channels, thus precluding offset measurements outside of the ones presented at Site 1.

Reviewer Comment 2.4 — I also disagree with the interpretation that these are all abandoned channels at Site 1. They seem to be large scale rills/gullies, and very largely influenced by a contact in the quaternary deposits mapped on Figs 4 & 5 (Qp1 forms gullies, Qt does not).

Reply: We refer Reviewer 2 to our previous responses to Reviewer 1 regarding the abandoned channels: *"The channels we refer to as "abandoned" are not active today. Rainfall in the study area results in significant infiltration and no appreciable surface runoff."* They also have minimal contributing drainage area, orders of magnitude smaller than the watersheds contributing runoff to the active channels (mapped in blue on Figure 3).

We clarify our use of the term "abandoned channel" in a new Section 4.2.4: Abandoned channels where we describe the channels and explain why we interpret them to be abandoned.

Water does flow in the deeper incised channels that drain the entire rangefront, but we do not observe evidence that water currently flows in the small abandoned channels with minimal catchment area that we utilize as piercing points. We have experienced torrential downpour in the study area and observed no surface runoff flow in abandoned channels. Active or intermittent channels are delineated on Figures 4, 5, and 6. These interpretations are further supported by our observations of thick soils developed within abandoned channel deposits, which are not incised and are not overlain by recently deposited sediment. We further note that our field observations suggest channelization of runoff occurs only within roads and other areas of anthropogenic disturbance.

Reviewer Comment 2.5 — In Figure 8, photos of lateral stream deflections range from 1-2 m, which is roughly 1/3- 1/6 of the vertical scarps presented. This is very different than the 1:1 ratio presented in the paper (and would suggest it is a dip slip rather than a oblique slipping fault).

Reply: The 1–2 m lateral stream displacements shown in Figure 8 (now Figure 7) are displacements across individual fault strands. The cumulative displacement across multiple strands is higher. We base our lateral to dip slip ratios on the cumulative displacements, as slip can partition between different fault strands. We clarify this point on lines 365–367 of the text where we state *"Cumulative oblique slip magnitudes, calculated by summing displacements of a piercing line across multiple parallel strands at a Site, range from 3.6–21.2 m at Site 1 and from 3.3–11.1 m at Site 2, with an average strike slip to dip slip ratio of 1.1:1."*

Reviewer Comment 2.6 — The paper conducts too much analysis using relatively weak data (as presented). This makes the data and resulting conclusions seem much more robust than they (currently) are. There are only 2 real sites shown – with mapped scarp lengths of ∼400 and 600 m, respectively (Figure 9). If at these two sites there is between 5-10 m of lateral slip preserved, why is this fault not more continuous between these two sites? Fig 2 shows that the active scarps are extremely discontinuous, yet the paper interprets that these are the results of repeated surface rupture of a 20-km-long-fault (line 354 - or 35-40 km? line 691). Regardless of the robustness of offset measurements in (1) – does the expression of ~1 km combined total of faulting warrant 10+ pages of kinematic analyses? This seems like an overinterpretation. I think the paper could be greatly shortened as these detailed analyses misrepresent the data as written.

Reply: We discuss the discontinuity of the scarps in response to the editor and reviewer 1 above, and we hope that the changes that we have made in Section 8.1 help clear up this issue.

The reviewer is using Figure 9 to estimate 400–600-m scarp lengths, but there is more than 1 km combined total faulting. Figure 9 shows only where we have offset measurements, not the full extent of mapped scarps. Figure 3 shows where the scarps are mapped at each site, and these scarps can be traced laterally for ~6 km at each site. We hope that the inclusion of the new figures 2b and S1, shows these relationships more clearly.

Another reason why the scarps may be poorly expressed in the landscape is because surficial deposits in this study area are limited to the past ~14 ka, after the last deglaciation. Unlike higher strain-rate regions with older surficial deposits (e.g., the Walker Lane, 7-8 mm/yr with >100 kyr of depositional history), faults in the northern Cascadia forearc tend to have more discontinuous expressions because of the short stratigraphic window within which to accumulate Quaternary offset. We discuss the discontinuous nature of the scarps in more detail on lines 410–419.

Regarding the reference to a 20-km fault, our intended reference to a 20 km distance refers to the distance from Site 1 to Site 2, whereas the previous reference to 35-40 km included additional scarps farther away (see Figure S1). We have rephrased this discussion on lines 251–256 to clarify:

"In total, we identified 127 tectonically produced scarps within the 100-km-long swath we examined using a combination of field and lidar-based observations (Figure S1). First, all scarps were carefully evaluated on lidar DEMs to assess the criteria listed above to interpret tectonic vs. non-tectonic scarps. Then, we visited all scarps that were physically accessible in the field to use additional field observations of geomorphic form and context to assess if they were best interpreted as being of tectonic or non-tectonic origin. Of the entire set of scarps identified from lidar, we visited and mapped 79 as tectonic in the field, spanning a total distance of ~35 km."

Reviewer Comment 2.7 — The limited extent and lack of continuity between the two mapped areas still gives pause that these are actually related to surface rupture of a fault rather than some sort of large-scale shaking, slope, or glacial features. How certain are the authors that these two isolated sections of scarps are not just two large-scale sackungen resulting from shaking on the Cascadia megathrust? This seems very plausible given the observations presented. I would give this alternative hypothesis some credence unless the authors are able to show that these scarps do form a more continuous network.

Reply: We are very certain that these are not just two sackungen. We refer the reviewer to our Supplemental Text S1 (now moved to the main text, Section 5):

"Scarps formed by sackungen typically occur parallel to topographic contours near the top of the range (elevations of >1300 m) in parallel linear sets <500 m long. Neither landslide head scars nor sackungen typically extend across multiple hillsides or drainages... Scarps that we determined to be produced by tectonic processes, in comparison, are ~100–1500 m long, occur in en echelon or parallel sets with spacings of 5–100 m, and are continuous for up to 2 km along strike. They occur

at elevations of ≤500 m above the valley floor and do not occur in the upper ∼700 m of the range. Individual scarps have trends of ∼270–300°, cross topographic contours, and have asymmetric cross-sectional geometries."

We include an example of sackungen in our field area in Figure S4b, showing a parallel set of ridges near the crest of a range. The scarps we map are at the middle/bottom of the range, and include en echelon and splay geometries.

Reviewer Comment 2.8 — What is the tectonic significance of this fault? I disagree with the authors' assertion that this is beyond the scope of the paper. The tectonic significance of a newly mapped active fault is certainly within the scope of this paper, and this does warrant a figure dedicated to your interpretation (I would argue this more useful than the detailed kinematic analysis referred to in (2) above). I agree with the other reviewer – is this a bending moment fault related to folding? I still question the kinematics of a normal fault that is parallel to the subduction zone, and apparently dips into the range front. Why is the vertical-lateral ratio so high (∼1:1 from Fig 9)? Given the apparent confusion both reviewers have – that this fault is a trench-parallel normal/oblique fault – it is very much justified to expect a discussion on this fault's tectonic role/significance.

Reply: The goal of our paper is to show that the Beaufort Range fault is an active fault, with right-lateral transtension. We have expanded our discussion of how broader tectonic drivers in northern Cascadia could affect the BRF in Section 8.6, which will hopefully lead to more research and discussion on the tectonic significance of the BRF in the future.

Reviewer Comment 2.9 — Figure 8h –this is minor– but what is the justification for the interpretation of alternating fault dips? Why would they not all dip to the NE or all to the SW? This seems overly interpretive given the available data. As written in the figure caption this seems that this interpretation is used as evidence of strike-slip faulting. This seems to be a bit of putting the cart before the horse. If the fault is forming a negative flower structure, then surely some of the strands should have a down-to-the-southwest sense of motion?

Reply: We explain how we determine fault dips in Section 7.1:

"No outcrop exposures of fault planes in Quaternary deposits were present in the field area, so we instead modeled the local strikes and dips of fault planes using a modified three-point problem approach. We assumed that the midpoints, or inflection points approximately halfway up a fault scarp, represent the most likely intersections of the fault plane with the surface. We surveyed scarp midpoints at multiple locations along each scarp and determined fault strike and dip through linear regression of a plane through the surveyed scarp midpoints using all survey data along a single, continuous fault strand segment (3–17 points per regression; see Supporting Information file for more details, and Figure 8)."

Our cartoon cross-section in Figure 7h shows how these multiple strands relate to each other. We refer the reviewer to the subsidiary fault strands in Figure 7h that show down-to-the-southwest motion.

Reviewer Comment 2.10 — Relationship between the BRF thrust & the modern fault – I am not sure I follow the authors' response to the other reviewer. The assertion that the modern fault is expressed on both the hanging and foot walls of the bedrock fault seems to be based on the presence of a dotted/inferred fault trace that is used to represent the bedrock fault. In Fig 4 it seems like nearly all of the scarps are expressed in the hanging wall of the BRF (granted at site 1 there are 2 subparallel thrusts mapped).

Reply: The location of the bedrock fault is not mapped as inferred, it is mapped as concealed. This is based on a well-constrained bedrock fault that is exposed in several outcrops but is otherwise buried beneath Quaternary sediments (map symbol of dotted line, solid triangles). More of the scarps at Site 2 are in the hanging wall, but the scarps at Site 1 span the hanging wall and footwall of both sub-parallel strands.

Reviewer Comment 2.11 — Evidence for 3 events is highly speculative given the available data. Why not 6 events? I do not see any clear groupings in the plots of Figure 9 that would suggest precisely 3 events. The paper does not present any paleoseismic data to support this conclusion.

Reply: We do not intend to argue that there are precisely three events. We intend to state that there are probably at least three events, given the relative ages we have available and the amounts of offsets that we observe as recorded in those units. A paleoseismic trench has been completed that elucidates a larger number of events, but is the subject of another paper - therefore, we do not include that data here. We refer the reviewer to our response to the editor above.

Reviewer 3

I agree with the assesment of the paper by the Associate Editor and Reviewers 1 and 2, and I look forward to its publication. Except for a few minor questions and editorial suggestions (below), I am happy with the overall approach, quality of the science and data, figures, references, the scope of the discussion, and (in most parts of the paper) with the overall writing style. Both previous reviews were solid, thoughtful, and detailed, although having studied fault scarps in similar terrain I found some of the reviewers' comments about scarp interpretation to be overly critical. The authors have done a through job of revising the paper in response to those reviews.

Unfortunately, the paper does not have the impact that this study deserves because the paper is still overly long, fails to present some of its important information in the order that the reader needs to best assess what you did, and contains much repetition. As in many other field geology papers, all three of these aspects are mostly the result of the paper's traditional organization of separate Methods, Results, and Discussion sections. Such an organization requires repetition of some of the same information in each of these sections; the authors have done a good job of guiding the reader with introductory paragraphs at the beginning of each of these sections, but, of course, this requires much repetition of the same information in different parts of the paper and makes it pretty long. Some of the concerns raised by Reviewer 1 under their Issue 4 and both reviewers' suggestions for reorganizing parts of the Results and Discussion sections reflect the same problems. Particularly in the Methods section, the reader needs important information that is not presented until later in the Results section. For example, the reader would have a much fuller understanding of (and greater confidence in) how offsets were measured on carefully interpreted landforms (Methods) if she had read the detailed description of the mapping along the fault zone before the detailed explanation of the offset calculations. Note that the information in Table 1 notes and the discussion of mapping and dating deposits in sections 4.1.2 and 4.1.3 makes section 3.3 completely unnecessary.

The authors may feel that deleting the Methods section (as suggested by Reviewer 2) and including an explanation of methods (where needed) as an introduction to various results and interpretation paragraphs will prevent the reader from keeping observations separate from interpretations. However, this paper is written well enough that this is not a problem: the advantages of keeping all the discussion of particular topics together in the same section of text far out way discussing them separately in different parts of the paper. We have struggled with how to organize some of our similar papers; these started out with traditional organizations, but ended up with more effective headings highlighting more concise text with a better flow of ideas (e.g., Nelson et al., 2017; Nelson et al., 2014, Geosphere).

The most effective way to increase the impact of the paper is by reorganizing and shortening it:

Reviewer Comment 3.1 — Reorganize by using more informative headings and subheadings, deleting the Methods section, and by moving the needed text from it into the appropriate parts of a revised Results section (see suggestions for new headings below).

Reply: Thank you for the suggested organization below. We have reorganized the paper mostly following these suggestions. We hope it has improved the flow and the impact of the paper.

Reviewer Comment 3.2 — Most paragraphs are well organized, but some have sentences with unnecessary or unclear words and phrases. I tried rephrasing your introduction as an example of how to reduce repetition (attached docx file). I did not make comments on the rest of the docx file.

Reply: We have made the suggested edits to the introduction, and worked to reduce repetition throughout the manuscript.

Suggested reorganization:

Title: **Post-glacial (<14 ka) scarps along the Beaufort Range fault, central Vancouver Island, record multiple earthquake ruptures during transtension in the northern Cascadia forearc**

1 Introduction
2 Setting of the Beaufort Range fault
2.1 Northern Cascadia forearc
2.2 Eocene Beaufort Range thrust fault *(lines 117-130)*
2.3 Overlying glacial deposits *(lines 131-140)*
(Because it's mentioned in the Intro and well discussed later, the 1946 earthquake does not need to be summarized here.)
3 Mapping and stratigraphy of deposits and landforms (4.1)
(Lines 267-275 not needed because this information is in the Intro)
3.1 Mapping methods *(3.1; slightly condensed from lines 154-184)*
3.2 Ice-contact glacial deposits and landforms *(4.1.1)*
3.3 Paraglacial deposits and landforms *(4.1.2)*
3.4 Post-glacial deposits and landforms *(4.1.3)*
4 Mapping and interpretation of post-glacial fault scarps *(lines 352-406)*
5 Quantifying fault slip across scarps *(3.2)*
(lines 408-414)
5.1 Topographic profiles(?) across offset landforms *(3.2.1)*
5.2 Reconstructing fault slip *(3.2.2)*
5.3 Cumulative slip measurements *(4.3.1)*

5.4 Slip vectors and fault kinematics
(lines 237-252; delete old section 3.3)
(lines 446-460)
6 Post-glacial history of the Beaufort Range fault
(lines 463-479 condensed)
6.1 Kinematics of the Beaufort Range fault and relation to inherited structures *(5.2)*
6.2 Evidence for multiple post-glacial surface-rupturing earthquakes *(5.3)*
6.3 Slip on Beaufort Range fault compared with kinematics of 1946 earthquake *(5.4)*
6.6 Post-glacial slip rates *(5.5)*
7 Implications for strain accommodation in the Cascadia forearc *(5.6)*
8 Conclusions *(6)*

Such an organization should result in a shorter text with a better flow of ideas. It will be apparent with such a reorganized text where you can further condense or delete information that is no longer needed. Yes, reorganizing and rewriting will be a lot of work, but it will definitely increase the impact and flow of the paper.

Problems with words:

Reviewer Comment 3.3 — I find "active" to be a vague and therefore confusing adjective. To some geologists "active" means post-Miocene, to many others it means "late Holocene," and to a smaller number of others it means "the past few hundred years." I suggest deleting "active" from the entire paper (and figures) and replacing it with the appropriate time term (e.g., "Holocene," "late Quaternary," "late Pleistocene and Holocene"). The use of "Quaternary" and especially "Quaternary-active" in this paper confuses me. Aren't all the scarps in this paper "post-glacial (<14 ka)"? If what you mean by "active" in some sentences is "potentially hazardous," I suggest the latter term. I find "modern stream" or "present-day stream" to be clearer than "active stream," especially because you use "active" to mean something else for faults.

Reply: Thank you for the comment. We have defined what we mean by "active fault" on line 94: *"Active faults (i.e., those that have ruptured in the late Pleistocene to Holocene) that accommodate forearc strain. . . "* and edited instances of "Quaternary-active" to simply "active".

We have changed references to "active streams" to "modern streams"

Reviewer Comment 3.4 — In seismology papers it's usually clear what is meant by "event" (originally, it only meant a blip on a paper seismograph). This is frequently not the case in paleoseismology papers where there are commonly several different kinds of erosional or depositional "events." If what you mean is "earthquake," just say "earthquake" rather than "event."

Reply: We have changed instances of "event" to "earthquake"

Reviewer Comment 3.5 — In this paper, is "slip" the same thing as "displacement"? Are both terms needed? If they are, make sure you use adjectives that will clue your reader into what you mean.

Reply: We have gone through the paper to make sure we use "slip" to mean slip on a fault plane, and "displacement" to mean displacement of a surface/landform (which may or may not be the same as the slip on the fault).

Reviewer Comment 3.6 — In section 3.2.1 (lines 191-203), the difference between "profiles," "surveys," and "transects" is confusing, perhaps because you are using different words for the same idea. Did you use a total station to make topographic maps of landforms (10s of meters wide and 100s of meters long), or did you just measure topographic profiles perpendicular to and across scarps, offsetting the profile line where needed?

Reply: We measured topographic profiles along landforms (though not always perpendicular to scarps). We have clarified the language throughout the paper to refer to these only as profiles, or profile segments. We reserve "survey" for points not part of a landform profile (i.e., the points used to calculate S/D), and for the act of surveying.

Reviewer Comment 3.7 — Because "fault "is not capitalized, shouldn't it be "BRf" rather than "BRF"? This is the only phrase in the paper where an acronym is helpful. Write out all other acronyms.

Reply: While "BRf" would be a more accurate reflection of how we style the full name of the fault, we prefer the abbreviation "BRF" for readability. This is commonly used in other papers where the word "fault" is lower case (e.g., Nelson et al., 2017; Morell et al., 2017, and many others).

Reviewer Comment 3.8 — "meters" and "kilometers" are written out unless they are distances with a preceding number.

Reply: Thank you for pointing this out - we have fixed the instances of "m" and "km" without preceding numbers.

Reviewer Comment 3.9 — As geologist readers, we know that this paper is about prehistoric earthquakes, so "paleo-earthquakes" and similar terms are not needed.

Reply: We have changed "paleo-earthquakes" to "earthquakes".

Reviewer Comment 3.10 — "landform" is prefered over "geomorphic feature."

Reply: We have made the suggested edit.

Reviewer Comment 3.11 — Especially where you list multiple adjectives, I find it helpful to hyphenate "upper-plate."

Reply: Thank you for the suggestion. We have hyphenated "upper-plate" where it is used as an adjective.

Reviewer Comment 3.12 — Table 1 is strange because it lists 5 samples for which no analyses were done. This is not information that readers need; please delete them. Most of the sample names are not needed (delete "BR18-0" and "C") making them unnecessarily complicated and too cluttered for figures. If you dropped the "4" from sample 42, all the samples could have single-digit names. Please add how much each dated sample weighed and the dimensions of dated charcoal fragments (mmxmmxmm) to the table. Were the fragments angular, subrounded, stained, etc.?

Reply: We have shortened the sample names to BR-8, BR-9, etc., and removed the undated samples. Unfortunately, we don't have the weights and dimensions of each sample, as they were prepared at PaleoTek Services and submitted directly to Keck by Alice Telka, who has since passed away. We have added the information we've been able to find in the notes she shared with us.

Reviewer Comment 3.13 — The figures are generally excellent; my favorites are Figs. 8 and 9.

Reply: Thank you!

Minor suggestions:

Reviewer Comment 3.14 — The figure parts are labeled with lower case letters, but the captions use upper case. Follow the journal's style for both.

Reply: Thank you for the suggestion. We have changed all figure parts to lower case.

Reviewer Comment 3.15 — Simplify the sample numbers, as suggested for Table 1. Your long numbers are only for your convenience, not your reader's.

Reply: We have simplified our sample numbers to BR-8, BR-9, etc.

Reviewer Comment 3.16 — Although your map units mix landforms with deposits, your well written description of the map units in the text is clear, and so the units don't need to be changed.

Reply: Thank you for pointing this out.

Reviewer Comment 3.17 — "Till" should not be capitalized. And in the text, "glacial" is not needed with "glacial till." Dick Flint (the father of North American Quaternary geology) used to complain that that phrase was very redundant.

Reply: Thank you for the suggestion. We have fixed this. We have also fixed the capitalization of till in the figures and removed extra references to "glacial" in the text.

Reviewer Comment 3.18 — Although it will require a bit more space, separating the parts of figures with a little white space, as you do in Fig. 11, will make them easier to distinguish.

Reply: Thank you for the suggestion. We have separated sub-parts of figures.

Reviewer Comment 3.19 — Delete the frame on Fig. 4.

Reply: We are unsure what the reviewer means by "frame" - if it is the boxes showing the locations of figures 5 and 6, we believe it is necessary to keep those.

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Emersonlynch_2025-01-10_01:08 PM

Hi J, Below is the email that Christine sent to you. Cheers, Emerson

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Dear Jason Padgett,

I am writing because my co-auhtors and I would like to seek your feedback about how we should proceed in our revisions to our Seismica manuscript (manuscript 1163) on the Beaufort Range Fault in Canada.

A few months ago, you returned the manuscript to us and provided several suggestions for how we could make it suitable for publication in Seismica. Thank you for those helpful and detailed comments. We have been able to address most of the suggested revisions and we anticipate resubmission later in January. However, we would like your feedback on two main items before we finalize our revisions.

First, we would like your opinion as to how much expansion of the role of the Beaufort Range Fault in regional tectonic deformation is required, in order for the manuscript to be suitable for publication. Reviewer #2 suggests that we include a more in-depth discussion of the BRF's kinematics and role in regional tectonics. We have written the paper to emphasize the recent activity and kinematics of the Beaufort Range Fault, and we feel that these results are important enough to stand on their own. While we do currently briefly discuss the BRF in the context of other forearc deformation (Section 5.6 of the last revisions), our perspective is that fully addressing the role of the fault in the regional tectonic deformation of the northern Cascadia forearc requires numerous additional analyses beyond the scope of the data included in this paper (for example, regional tectonic syntheses, analysis of geodetic or regional stress data, numeric modelling).

Second, we seek guidance on how to showcase the evidence for right-lateral offsets in our paper. Based on previous feedback from Reviewers 1 and 2, you suggested including lidar data for each offset measurements and providing more substantial evidence for the right-lateral offset. As a reminder, our evidence for a right lateral component of displacement comes from direct field observations of linear geomorphic landforms (interfluves and channels) that we have observed in the field to have consistent right lateral offset on the order of 1-3 m.

Although we used the lidar data to identify the field sites, the right lateral slip on the Beaufort Range fault is unfortunately not clearly visible in the lidar DEMS available for this area. The lidar we use in our study was collected by logging companies for forestry management, and the ground returns are not of the resolution and quality typical for datasets collected for fault mapping purposes. Ground returns are unevenly distributed (e.g., Supporting Information Figure S4), and it is common for ground returns to actually be returns from tree trunks and downed logs on the forest floor. In addition, the lateral distance between ground returns (sometimes only 1 or 2 returns per 10m) can be wider than the channel troughs or interfluve crests that are laterally offset. Thus, a hillshade or contour map of the lidar DEMs is insufficient to consistently capture the exact position of the laterally offset channel thalwegs and interfluve crests, and due to their low resolution, can yield DEMs images that appear 'unconvincing'.

Despite this, we are thoroughly convinced that our offset measurements are robust. The poor quality lidar was the reason behind our decision to physically walk and survey the offset landforms ourselves in the field using a total station, and to perform a statistical analysis of their lateral offset and uncertainty as we present in the paper. We spent 2 six-week field seasons dedicated to capturing this signal by physically walking each interfluve and directly surveying the geometries of offset landforms. We have had a team of ~20 scientists view these offsets in the field with us, including those from the USGS, the Pacific Geoscience Center in Canada, and experienced faculty in regional neotectonics fault mapping and paleoseismic trenching. All of these scientists were convinced, as we were, of the right-lateral component of slip.

Therefore, although we very much appreciate reviewers 1 and 2's concern that the offset data be robust, convincing and reproducible in the manuscript, the lidar data cannot show this signal. In the latest round of revisions, the reviewers request lidar insets of each of the ~60 surveyed sites. We do not feel that including the poor-quality lidar data for all 60 sites improves the paper, and we do not think this is the best use of manuscript space.

In order to try to address this in the last revision, we added several field photos of these landforms in the revision to this article, and we have included all the right lateral offset measurements in the manuscript. However, this proved insufficient for the past reviewers. Thus, we would appreciate further guidance on balancing the reviewers' requests with these constraints.

Many thanks for your thoughts about these issues, and for handling our paper.

jasonpadgett_2025-01-16_03:30pm

Dear Christine and Emerson,

I am currently on parental leave and appreciate your patience.

I am pleased to hear you and the co-authors are working on revisions and planning out the resubmission.

In my letter to the you and the other authors, I provided my thoughts on how one might potentially address the Reviewers' concerns. However, as requested, I will provide my thoughts and opinions to your 2 questions below. In general, and as a spoiler, I think compromise is often the answer.

As a quick reminder, Reviewers 1 and 2 have each signed a review and offered to have further discussion.

- **Regarding the role of the BRF in a regional deformation context**, *as I understand it*: *Reviewer #2 thinks what was previously provided was not enough and you have explained that a complete analysis is beyond the scope of the paper.*

I see your point and do not disagree with you. I cannot be certain of the minimal requirements for publication that Reviewer 2 is suggesting or what the next round of reviewers might think. However, I am certain that neither of the other round 2 reviewers (reviewers #1 & 3) commented on the alleged "missing" section within your manuscript.

I suggest you offer a compromise. Consider adding 3ish lines/statements somewhere in the manuscript, that you can point to in your response letter, that reflect your consideration of the reviewer's comment.

Maybe offer an expansion within the regional setting (section 2). I'm envisioning statements that might provide more detail on the regional tectonic deformation. Perhaps some of the new statements can be written in a way that might encourage the audience to reflect on the possibility of a more comprehensive analysis.

Maybe, also, can a response to the reviewers' comment be woven into a "future direction suggestion" type of statement somewhere in the discussion?

I think it's important to note here, as it ties directly into your question 2 topic and you brought up section 5.6 in your question, that Reviewer 1 wrote in their round 2 review "While the kinematic analysis appears sound, it relies on these scarps being tectonic, and to me it's not 100% clear; thus, the broader tectonic implications you discuss **[your referenced section 5.6]**, which would have large regional seismic hazard impacts may be false and *all for not*."

Based on the reviews, I get a sense that for all the reviewers (1, 2, and 3), the prior "scarps being tectonic" is the primary issue/argument that needs expansion and/or a reorganized presentation.

- **Regarding the presentation of the offset measurements**, *as I understand it*: *The authors used the lidar data to identify field sites where the fault scarps can be traced within a GIS, then they went into the field, and measured offset features by conducting ~58 total station transects (except for Site 2.3, where the lidar is good enough). However, the authors think that the lidar is not precise enough to show the lateral signal consistently, even with the overlying total station transect data. Despite this, the authors and their field visitors are thoroughly convinced that the offset measurements are robust. The authors do not feel that including maps of the 58 offset measurements will improve the paper, nor is it a good use of manuscript space.*

Reviewers 1&2 question the offset measurement in Figure 3 and point to lack of following the "rule of V's", which is the authors described approach in Text S1. This discrepancy leads the reviewers to be skeptical of all offset measurements. Moreover, in their round 2 review, Reviewer 1 remarks "Stating in your methods that you 'only mapped fault scarps based on some criteria' and that you 'visited each scarp in the field to confirm,' doesn't convince me that these are indeed fault scarps."

The offset measurement topic really gets to the crux of what I see as the reviewer's main concern, as I stated above in my Q1 response. Reviewers 1 and 2 are unconvinced by the scarp and offset evidence that have been presented by the authors and, therefore, I think it's reasonable to assume that other audience members/reviewers may have similar impressions. To me, it sounds like a bolstering of the "scarps being tectonic" argument is what the skeptical reviewers are needing/requesting. Therefore, I think, if reviewers need further convincing and want to see more of the data, then convince them more within the text and show them more data/imagery, regardless of the described presentation nuance. IMO, you worked hard to collect the transect data; show it off and let the data speak for itself.

In my letter to the authors, I suggested showing all the data to address and placate the reviewers unconvinced concerns. However, I also understand showing everything might be too much. Therefore, **I suggest here, that the authors offer a compromise**. Find a reasonable balance, i.e., show a couple more scarps and offset transects in the main text and a couple more in the supplemental material (*space in the supplemental should not be a limiting concern*).

Additionally, I think the lidar presentation nuance, your described limitation, must be communicated/explained to the audience. In your message to me you wrote *"In addition, the lateral distance between ground returns (sometimes only 1 or 2 returns per 10m) can be wider than the channel troughs or interfluve crests that are laterally offset."* [A reader may wonder: how often did this occur and to what extent did the artifacts, the resulting triangular facets, interfere with your mapping? That is, do any of the low return areas cross where you have mapped scarps that were not visited in the field?] Your limitation explanation should be in the main text **and** right after supplemental text S1 (paragraph1, sentence 2) that explains "the lidar point clouds contained an average of ~1.2-1.4 ground returns per square meter." Although the lidar-ground return limitation is shown in Fig. S4, the figure is not cited in either the main or supplemental text. Since this is an important issue (especially considering site 2.3; lidar only measurements), I think you should show the ground returns in the offset (and maybe even some scarp) figures/maps.

- Figures S4, S5, S6 or S8 are not cited the main text and they should be.

- Regarding Figure S4, add in the 1 m contours to at least panels B & D. Also, can the scale bar be set to 10 m and the view zoomed in(?), to help the audience understand and see the results of your limitation description?

- Regarding the reviewers questioning offset measurement in Figure 3, would inclusion of the ground returns here help explain/show the authors position of the presentation nuance/lidar-limitation here? I don't think I see any triangular facets here.

As I suggested in my letter, if the hillshade doesn't help your case, can another surface layer offer any improvement? For example, changing the light angle on the hillshade, or a slope map/layer, or an aspect map/layer, or maybe even a roughness layer.

In a more general sense, I wonder how the authors and their field visitors were convinced and if that argument description is thoroughly represented in the manuscript. The authors are introducing a new fault, which complicates a lay-persons understanding of the regional tectonic deformation and it also happens to be mapped pretty-close to some important infrastructure. Therefore, the newly mapped fault will likely have a large impact on the surrounding community members, stakeholders, and discourse communities. The burden of proof is relatively high and its totally on the authors shoulders to provide enough information for the audience to follow the authors, and their filed visitors, logic and rule out non-seismic source possibilities. If walking the offsets was the main convincer for the field folks, then perhaps the next best thing the authors can do, is to show the ground-truthed high-precision total station transect data layered over the poor-quality lidar. I think the audience can understand that data isn't always perfect; let the data stand on its own, state the caveats, state the assumptions, and account for potential error.

Along that line of thought, I think the authors need to account for a reasonable amount of inherent field-based measurement error partly imposed by the duff and dead-fall cover, which is different than the precision of the total station data and part of the reason for the low quality lidar data (correct?).

As a summary, I think the authors should strengthen the "scarps being tectonic" argument by reorganizing the presentation (perhaps like reviewer 3 suggested, and/or/maybe moving some supplemental text into the main text), provide some revised figures of scarps and consistent offset measurements, provide a show-and-tell (figures and text) of the lidar limitation explanation, and account for inherent field-based measurement error. I would think all of that should be much closer to acceptable for the next round reviewers.

I hope these thoughts and opinions are somewhat helpful. I look forward to the revisions.

Response to Reviews on "Late Pleistocene to Holocene transtension on the BRF"

Response to the Associate Editor

I found this paper to be a timely and thought-provoking contribution to ongoing efforts to build and interpret a history of forearc deformation at the northern extent of the Cascadia subduction zone. The topic and specific content are well suited for publication in Seismica, especially the Cascadia special issue. I see the central focus of this work to offer novel observations and interpretations from a complex and insufficiently examined region of the Cascadia margin and, in my opinion, it will be regarded as an important contribution. I have now received two peer-review reports for your manuscript. In general, the reviews are positive, but each review suggests revisions are needed before publication. They have each provided a few major comments and a number of more detailed in-line suggestions. I agree with the reviewers' comments regarding the level of revisions needed to improve the manuscript. I therefore invite you to prepare a revised version of your manuscript that considers all of the reviewers' comments. In preparing your revised manuscript, please respond to each comment/suggestion. I recognize that you may not agree with all the comments, so please indicate and explain your reasoning where necessary. In particular, I agree with both reviewers that the manuscript will benefit from further discussion related to the transtension interpretation and possible alternatives. I also agree with the reviewers that some restructuring will help reduce repetition. Additionally, I caution against the heavy use of demonstrative pronouns, such as "this" and "these," as they may confuse non-specialist audience members.

Dear Editor,

Thank you for your consideration of our paper and for your comments. We have expanded our discussion of the right-lateral component of slip and now report these measurements separately from the oblique measurements in both the Results and Discussion sections, and we now discuss the different interpretations of our kinematic data relative to the Eocene bedrock thrust fault in more detail in Section 5.2. We specifically discuss the "bending-moment fault" hypothesis below in our response to Reviewer 1. We have restructured and edited the paper to reduce repetition and improve flow, following the suggestions of the reviewers. We have also edited the paper to reduce the occurrence of "this" and "these".

We would also like to note that the formatting of the Abstract and Non-technical summary sections in the Seismica latex template do not appear to be compatible with latexdiff and so do not show the tracked-changes. We have only made a few minor wording changes to these sections.

Response to the reviewers

Reviewer 1

The manuscript is generally well-written and well-illustrated; however, I think there are several improvements that could be made and a couple major issues that need additional consideration. . I've made comments throughout the manuscript, for which I apologize are uneven and probably repetitive. Thus, I have summarize the major issues below. Additionally, I think the manuscript would benefit from some restructuring of some sections to help with flow and to reduce repetition - Please refer to Issue #4 and comments in red throughout the manuscript for those suggestion.

Highlight color key for manuscript
Yellow : text comments/suggestions – regular edits
Green : Suggested Supplementary data
Red – Manuscript outline structure suggestion
Purple – Interpretation caution

Major Issues

Issue 1: Interpretation of transtensional faulting on the BRF:

The study focuses on a series of uphill- and downhill-facing scarps along the NW trending and northeast dipping Beaufort Range fault. Most of the scarps manifest within, what are identified/mapped as, paraglacial deposits. Horizontal and offset measurements of geomorphic features associated with the scarps are used to interpret transtensional faulting on the larger BRF. I think other potential hypotheses of the origin of these scarps needs to be address.

First, I think the Intro does a poor job at laying out the contemporary strain field of the northern Cascadia Subduction zone, rather the author focuses on the uncertainties of prior studies based on geodesy and seismology. Yes, the signals are weak and data is relatively less dense, but most (if not all) show the first-order stress and modern strain field is trench parallel compression and, to a lesser amount, north-south intraplate contraction (see figures below). A first-order observation would be the NW trending BRF should be transpressional.

Reply: We appreciate the reviewer's opinion here, however we disagree with their assertion that the contemporary crustal strain field in this region of the northern Cascadia subduction is well characterized, especially over time periods longer than the megathrust seismic cycle. The papers that the reviewer mentions (and that we cite in our paper) rely on very sparse datasets that are mostly concentrated in the southern portion of southern Vancouver Island. Moreover, both of the datasets used in these prior studies only analyze data over decadal time periods, which can be heavily influenced by strain variations related to the decadal megathrust seismic cycle.

We intend to reinforce the observation that small strains associated with active faults are rarely resolvable in the GNSS fields of subduction forearcs like Cascadia that have a large geodetic locking signal. And, several recent publications have documented that the decadal scale decadal GNSS strain field in subduction forearcs does not always predict long term upper plate fault kinematics, (e.g., Delano et al., 2017; Harrichhausen et al.; Herman and Govers, 2020; Regalla et al., 2017). We also note that, while the megathrust locking signal in GNSS may suggest transpression, this paper focuses on presenting data of field observed offsets along the BRF, and not model-based inferences.

We have restructured the introduction to more clearly articulate our intended points: that GNSS and seismicity data alone appear insufficient to fully characterize forearc strain in northern Cascadia over millennial timescales, and that it is therefore critical to use long term paleoseismic and geomorphic records as well. We note that our study is the first of its kind to examine the crustal strain field over millennial time scales in the forearc of Vancouver Island, over time periods longer than the megathrust seismic cycle.

Thus, I think the author should consider the potential of these scarps being bending-moment faults within the hangingwall of the BR thrust fault.

Reply: We thank the reviewer for this suggestion and we have already extensively thought through this possibility. As we outline below, there are no data or field observations to support the interpretation that the scarps we map are associated with thrust sense displacement in the hanging wall of the Eocene bedrock thrust fault.

There are several observations that would support this: 1) Most of the mapped scarps lie north of the inferred trace of the BR thrust fault, putting then within the hanging wall.

Reply: This is not correct. Our mapped scarps are in both the hanging wall and footwall of the bedrock thrust fault, particularly at Site 1 where they extend from the footwall of the lower splay to the hanging wall of the upper splay. This is stated on lines 504-506: "First, the presence of active BRF scarps in both the hanging wall and footwall of the Eocene bedrock thrust faults (Figure 4) suggest that there is not a strong inherited lithologic or mechanical control on the position of the active BRF at the surface."

We also note that the predicted location of extensional bending moment faults would be in the apex of the fault related fold, in the anticlinal hinge, which should be near the top of the Beaufort Range (see model image below from Zhang et al., 2024). Bending related faults that could occur lower on the range front (nearer the position of the scarps we map) would be 'out-of-the-syncline- thrusts associated with contraction in the synclinal hinge. However, we document extension at the base of the range, not contraction, which is inconsistent with this model.

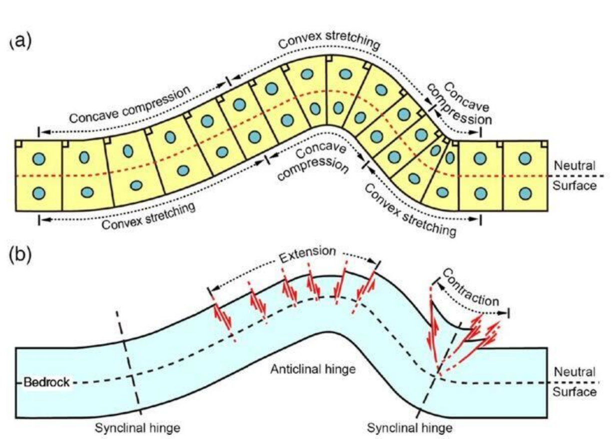

Figure 1: Zhang et al. (2024)

2) as interpreted in the paper, the steep dips suggest normal faulting, 3) in addition to the horizontal offset measurements, the discontinuous and en echelon pattern of the scarps suggest more far-field lateral slip. I think it's possible that oblique slip on the NE-dipping thrust of the BRF could produce these scarps.

Reply: We are glad that the reviewer agrees that the steep dips suggest normal faulting and the discontinuous and en echelon pattern is suggestive of lateral slip. This is exactly what we argue for in our paper.

However, we note that there is no evidence of bending in the hanging wall of the Beaufort Range thrust fault that could produce the faults the reviewer argues for. Flow tops in basalt are gently dipping

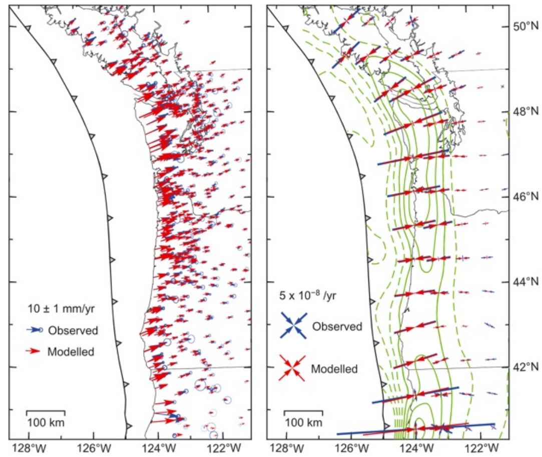

Figure 2: Li et al. (2018b)

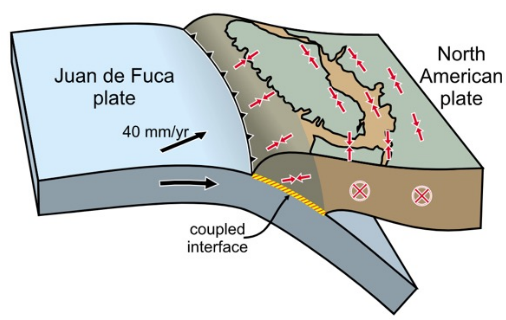

Figure 12. Cartoon representation of the dominant stress orientations (red arrows) in southwest British Columbia. The red crosses in circles represent motion into the page due to the northward push of the Oregon Block, which changes the stress orientation.

Figure 3: Balfour et al. (2011)

everywhere they are mapped and where we have been able to measure them near the thrust fault. In addition, we note, as above, that the scarps also occur in the footwall of the Beaufort Range thrust.

If the Beaufort Range thrust fault is slipping at depth (i.e., the scarps observed at the surface merge with the inherited Eocene bedrock thrust at depth), the slip sense that is geometrically and kinematically compatible with the available data for the BRF is that it slips as a right-lateral transtensional fault, not as a thrust fault. Furthermore, we note that there is no field or lidar based evidence of surface offsets at the exact position of the bedrock thrust fault. Thus, at least at the surface, the bedrock thrust appears not to be active in the Quaternary.

We have elaborated on this point in lines 502-528 in Section 5.2.

Second, this relates to Issues #2 and came to me as I was reading about paraglacial deposits. Paraglacial deposits seem quite ambiguous, but are associated in one-way-or-another to glaciers. Given that most of the scarps are within them; could they be tension cracks due side-hill slumping of the paraglacial unit (see figure below).

1944 C.K. Ballantyne / Quaternary Science Reviews 21 (2002) 1935–2017

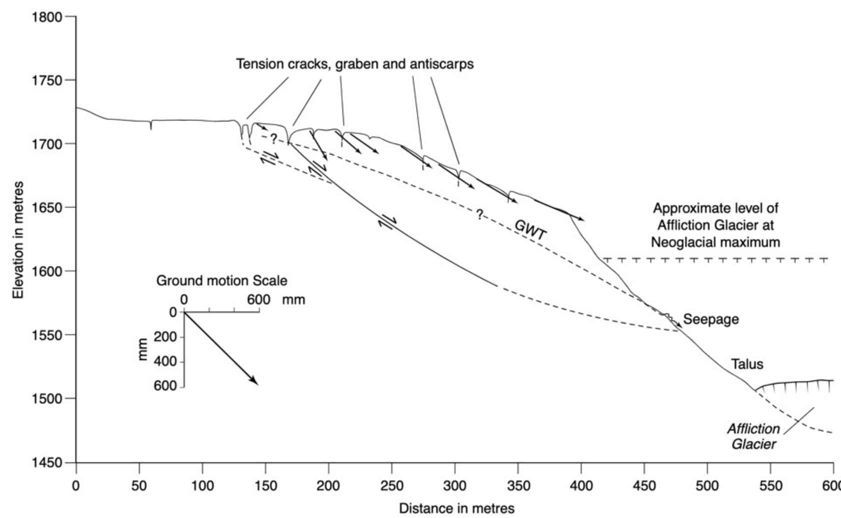

Fig. 4. Cross-section of rock-slope movement caused by downwastage of Affliction Glacier, British Columbia. Adapted from Bovis (1990). The ground motion vectors represent displacement over 4 years (1982–1986). GWT = inferred groundwater table.

Figure 4: Ballantyne (2002)

Reply: Thank you for bringing up this point. We have also carefully considered the origin of these scarps, and describe our rationale for distinguishing tectonic scarps from those created by glacial, gravitational, anthropogenic, or erosional processes in Supporting Information Text S1 and lines 172-176 in the main text. One of the key observations arguing against the hypothesis that the scarps we map are related to tension cracks from side-hill slumping is that they offset post-glacial landforms right-laterally by several meters. This observation, and the discontinuous en echelon patterns that the reviewer themself noted is indicative of lateral displacement, are not consistent with formation by gravitational processes.

Issue 2: Paraglacial deposits (Qp1 and Qp2):

Had to look this one up as its quite an ambiguous term. I either disagree with the term 'abandon channel' or am not understanding the geomorphology associated with the channels incised into the paraglacial deposits Qp1. As described in section 4.2.2, it sounds like the Qp1 deposits merge with the Qt deposits above, at the higher end of the slope. Looking at Fig 3a, I see some of the deeply incised channels continue upslope, into the Qt deposits, as less incised channels and sometimes forming more of a rill pattern. So they look like continuous active channels rather than 'abandon channels' or cutoff from upslope drainages. The potential active nature of these channels relates heavily on the quality of the offset measurements you make within the thalweg (see below).

Reply: Thank you for bringing up this point. We have added an explanatory phrase and a reference to the Ballantyne review paper to lines 280-281 for readers who are unfamiliar with paraglacial environments, which are specific to deglaciation and subsequent hillslope adjustment. The channels we refer to as "abandoned" are not active today. Rainfall in the study area results in significant infiltration and no appreciable surface runoff.

We clarify our use of the term "abandoned channel" on lines 302-305: *"These channels are disconnected from the active streams where rainfall and snowmelt are channelized, and have negligible* *catchment area, but they merge into the heads of Qp2 deposits. This suggests that they were active at the time of deposition of Qp2, but no longer accommodate significant discharge."*

Issue 3: Vertical Offset measurements – There are several issues that need to be address regarding vertical offset measurements:

There is an incorrect use of the term 'scarp height', where I think the author wants or intended to report vertical offset values. They are different (Bucknam et al.,1979) and is improperly used throughout the manuscript. This is most apparent in section 4.3 where 'scarp heights' are reported along with horizontal offsets measurements.

Bucknam, R. C., and R. E. Anderson. 1979. "Estimation of Fault-Scarp Ages from a Scarp-Height–slope-Angle Relationship." Geology 7 (1): 11.

Reply: Thank you for pointing this out. In the intro paragraph to section 4.3, we were referring to our field observations, not our offset measurements. We have moved this text to Section 4.2 to avoid further confusion.

I don't think the vertical offset measurements of the thalweg transects are very robust, and this may be a result of my misunderstanding of the 'abandon channels' I discuss in issue #2 (above). While thalwegs are commonly utilized for horizontal offset measurements, their ability to preserve vertical offset presents higher uncertainties as a result of their susceptibility to erosion from focused 'stream' incision and/or aggradation from hillslope processes of the channel margins. Especially with the uphill facing scarps where sediment will collect at the base of the scarp. The interfluve measurements provide less uncertainty, as scarp erosion is the main point of uncertainty, and appear to be best feature to preserve vertical offset. Both provide a minimum offset measurement, but much less-so using the interfluve. I believe this could explain your smaller thalweg measurements.

Reply: We use far-field projections to calculate offsets from our piercing line surveys (both interfluves and thalwegs). These far field projections exclude points collected near the scarp where there may be some erosion at the scarp crest or deposition against uphill-facing scarps. By excluding these parts of the profile, we are confident that we are not underestimating the vertical offset of our thalweg profiles due to local deposition or erosion near the scarp. We have added text to lines 222-225 in Section 3.2.2 to clarify this point: *"We manually defined the remaining parameters—the point where the fault plane intersected the ground surface, and the individual survey points used to fit linear regressions through the upthrown and downthrown surveyed piercing lines—for each topographic profile, excluding points where there may be deposition against an uphill-facing scarp or erosion at a scarp crest."*

As a result, I see this comparison of the thalweg and interfluve these measurements as a major flaw in the interpretation for multiple earthquakes. Yes, the drainage features are younger that the interfluve surfaces, but seem more active and less reliable. I don't think this is a fair comparison. If you're going to compare these profiles, I suggest you map the channel deposits as something different, to show they are younger. Furthermore, you also compare VS measurements of profiles transecting Qp1 and the one profile across the surface of unit Qt (profile 3). Based on your mapping in figure 5a, the fault trace is inferred across that terrace. So, is there a scarp there? The profile in Fig 10a shows some flattening of the hillslope, but its not 'uphill facing like the others (channel and interfluve profiles), and not as sharp – which I would expect it to be, especially if its 1946. Based on these observations, I question the interpretation of multiple earthquakes.

Reply: We have adjusted our mapping to denote that we are confident in our mapping of the scarp across the Qft2 terrace at Site 1. While this scarp is more subdued and bench-like than the scarps farther west in Site 1, it is along strike from strand Ee, has a clear vertical separation of the terrace surface, and a small boulder is ponded against it. We do not argue that the scarp offsetting Qt is from the 1946 earthquake, only that it is from an earthquake since 3.5 ka.

We also note that given the limited flow that accumulates in the abandoned channels, it would be difficult for the channel to erode across the 6-7-m-high topographic scarps observed at Site 1, where we make our multiple earthquake interpretation. Water depths of that magnitude would also have eroded the adjacent interfluves. We also note that the thalwegs consistently have less right-lateral displacement than the interfluves, which supports our interpretation of multiple earthquakes. We have revised the text on lines 440-444 to reiterate this (*"These same relationships are mimicked in the set of cumulative right-lateral and oblique displacement measurements at 23 interfluves and channels at Site 1 (Figure 9), which show that older interfluves developed in Qp1 consistently have 4 to 10 m more cumulative right-lateral and oblique displacement as compared to younger channels incised into Qp1 (Figure 10c)."*), and added the right-lateral displacement to Figure 10c.

Issue 4: Structural Suggestions:

Mapping/Stratigraphy/C14 Results – I suggest presenting your Q mapping, stratigraphy, and C14 results as the first subsection in Results. There is a lot of reptation between all of these sections that relate to one-another and for the basis of your scarp analysis. This will set up your fault scarp analysis and make it easier to assign ages to the scarps based on the unit they manifest within. Kinematic analysis - The approach is sound, but I think should be presented within the Discussion section and not confused with the actual data of the offset measurements in the results, which needs some work (see issue 3 above). I think doing this earlier on in the discussion will lead you into the interpretation of the scarps considering all possible alternative hypothesis (see issue 1 above), and the question of whether-or-not it's the 1946 scarp.

Reply: We have moved the Quaternary mapping and stratigraphy section to the beginning of the Results. However, we disagree that the kinematic analysis should be presented in the discussion section. These are primary results from our study. As such, we have kept the relevant sections within methods and results.

Line-by-line comments

Reply: We have made most of the small wording and grammatical edits suggested directly within the annotated PDF. We address the line-by-line comments and questions below.

Abstract

Line 17. (northern Cascadia forearc): I think you should be more descriptive as I originally thought of the Puget Sound/Salish Sea region. Suggest: "The deformation field of the forearc at the northern end of the Cascadia Subduction on Vancouver Island is poorly..."

Reply: We have made the suggested edit.

Line 24. (<3-4 ka): Is this a range or are you providing a maximum age? If so, then just put <4 ka

Reply: We have made the suggested edit.

Introduction

Line 45. (energy): ‘moment’ might be a more appropriate term here as most of the remaining items are seismically related

Reply: We disagree, as we are not referring exclusively to earthquakes.

Line 51. (methods): your not really listing methods rather than datasets

Reply: We have deleted the word methods.

Line 54. (In the northern Cascadia forearc of Vancouver Island, many of these methods have yielded incomplete or conflicting results.): I feel the author is using the elevated uncertainty of this region from the realatively weak signals of seismicity and geodesy to embellish a possible transtensional environment. Rather, these references and data within support a tranpressional environment within the forearc on Cascadia, even at the northern end.

Reply: Thanks for pointing this out. However, we do not intend to argue in this paragraph that there is strong existing evidence for a transtensional environment. We are simply pointing out that the existing data are inconclusive as to the stress and strain fields within the forearc. Please see our response to Major Issue #1 for how we have restructured the introduction.

Line 58. (very weak subduction megathrust): unclear - are you saying the locking is ‘weak’?

Reply: This phrase is no longer present in our restructured introduction.

Line 60. (Third, the focal mechanisms of crustal microseismic events suggest equal amounts of strike-slip and normal faulting (Balfour et al., 2011).): But even more thurst faulting!!!

Reply: We have added “on central Vancouver Island” to clarify which focal mechanisms we are referring to. The thrust faulting in Balfour et al. (2011) is primarily offshore and in the Puget Lowland.

Line 64-65. (The northern Cascadia forearc on Vancouver Island is one location where seismicity and Global Navigational Satellite System (GNSS) data provide conflicting information about forearc stress and strain state.): This is very similar to the thesis of the prior paragraph

Reply: We have deleted this sentence to avoid repetition.

Line 66. ($\sim$<1 mm/yr north-south geodetic strain): awkward - need to expand. You need to define the ”northern Cascadia forearc” better and earlier on in this paragraph. Where is this <1mm/yr constrained to? This is not well illustrated in your map (Fig 1). Also, this is contractional strain and shoud clarified

Reply: Thank you for the suggestion. We have edited this paragraph and now refer specifically to Vancouver Island and residual GPS velocities on lines 69-72: *“In this portion of the northern Cascadia forearc, decadal records suggest upper plate strain accumulation is low, with only a few hundred micro-earthquakes recorded in the past $\sim$50 years and residual forearc geodetic velocities of only $\sim$1-2 mm/yr (CSZ; Balfour et al., 2011; Mazzotti et al., 2002; Mulder, 1995).”*

Line 77. (tectonogeomorphic data): not sure this is a real word... suggest using ‘tectonic-geomorphic data’

Reply: We have hyphenated "tectono-geomorphic" to be more consistent with common usage (e.g., Cheng et al., 2024; Regalla et al., 2022; Schoenbohm et al., 2004; Schottenfels and Regalla, 2021; Taylor and Howard, 2000, and many others).

Line 78. (Beaufort Range Fault): should use lower case 'f'. please apply to all. Also, I think this is the first time you name the Beaufort Range fault and, whereby, you refer to it by BRF –¿ you need (BRF) here

Reply: We have added the initials here. This was the only instance of capital F in Beaufort Range fault.

Line 79. (a well-preserved set of): suggest switching this around: 'a set of well-preserved'

Reply: This phrase is no longer present in our restructured introduction.

Background

Line 104. (initially formed in the Eocene as a bedrock fault that placed the. . .): You state that it is an Ecoene-age fault later in this paragraph what the thermochron. Suggest making this more active: "The BRF juxtapositions Late Tirassic..."

Reply: We have made the suggested edit.

Line 113-114. (However, several researchers proposed that the Beaufort Range fault may have hosted the 1946 M 7.3 Vancouver Island earthquake.): This sentence needs a citation as is. Suggest combining this with previous sentence to set up the rest of the paragraph that describes these prior proposals.

Reply: We have combined these two sentences and added relevant citations in the new Section 2.3.

Methods

Line 138-139. (20-100 m above the valley floor, or 500-870 m below the range crest): not needed - or choose one

Reply: We have deleted this phrase.

Line 140. (Our initial observations): I think you're 'putting the cart before the horse here'. You haven't got to the observations point yet. Suggest making this sentence more active and saying something like: "A network of V-shaped drainage channels and interfluves appear right-laterally offset across the ∼? m scarps." Then on your observations you can make the offset measurements

Reply: These are paleochannels, as discussed above. We have clarified our use of this term on line 305, and rephrased in the methods to specifically refer to abandoned channels.

Line 155-158. (Identifying fault-related deformation (e.g., fault scarps) in dateable Quaternary sediments is essential for characterizing the slip history of active faults (e.g., Van Der Woerd et al., 2002; Zinke et al., 2017; Hatem et al., 2017; Regalla et al., 2022), but dense temperate rainforest limits exposures and accessibility of offset Quaternary deposits in this study area.): Not needed

Reply: We disagree with the reviewer that this is not needed. These statements highlight the importance of lidar data in the methods we use in our analysis. In fact the lack of lidar, until recently, is the reason no fault ruptures have been identified to date in this portion of the forearc, despite the occurrence of the 1946 earthquake.

Line 164–167. (Lidar point cloud data were collected by Terra Remote Sensing, and TimberWest and Island Timberlands logging companies provided ground returns. The lidar point clouds contained an average of 1.2-1.4 ground returns per square meter. We gridded these data into a 0.5 m DEMand generated topographic derivatives such as hillshade, standard deviation, and slope maps to aid in mapping.): This would be better placed as Supplementary information

Reply: We thank the reviewer for this suggestion and have added this information to Supporting Information Text S1.

Line 171-175. (Criteria used to distinguish fault scarps from other features include whether the features are linear; continuous over >50-100 m length scales; cut across topography; and offset hillslopes, abandoned channels, or interfluves (Figure 3b-c). We took care to distinguish potentially fault-related scarps from landforms produced by glacial deposition or scour, anthropogenic disturbance, gravitational failure, or differential erosion (see Supporting Information Text S1 and Figure S2).): Also would be better places as Supplementary info

Reply: These two sentences summarize Supporting Information Text S1, so we have kept them here.

Line 185. (abandoned): Are these really abandoned channels? They look like active drainage channels that occur where there appears to be enhance local incision (uplift..) on the alluvial slope. In figure 3b I see a channel thats beheaded by the scarp. Suggest rephrasing

Reply: These are abandoned channels, as discussed above.

Line 187-189. (Unfortunately, we were unable to use methods that rely on continuous lidar data to estimate fault slip (e.g., LadiCaoz; Zielke and Arrowsmith, 2012), because available lidar DEMs contained non-uniform return spacing and included some false ground returns (Figure S4).): Not needed or suggest rephrasing so sentance doesn't start out with "unfortunately" Be Positive!

Reply: We have deleted the word "unfortunately".

Line 196-201. (For each profile, total station survey data were collected every ∼0.5-1 m, to a distance of >20 m uphill and downhill of each fault scarp (Figure 7). Along survey transects where a geomorphic piercing line extended for less than 20 m (e.g., between closely-spaced fault strands), we collected a minimum of 3 survey points, with an average of 11 points. We complemented these ground surface elevation profile with six additional topographic profiles extracted from lidar DEMs in a portion of Site 2.3 where thick forest cover and uneven topography prevented total station surveys of offset abandoned channels.): Too detailed for manuscript

Reply: We disagree with the reviewer that the methods that we employ in our study are too detailed for our manuscript. These data are key to our overall study and we see no need to exclude them.

Section 3.2.2. (Reconstructing oblique fault displacement): In my opinion - I think this section and the following section (3.2.3 Inversion for fault kinematic) would be better placed within the discussion section as they are both analysis of the topographic data you collected. Keeping the data (mapping, ages, and topographic profiles) separate would help the reader delineate data from interpretation, which is what these analysis are of the fault kinematics.

Reply: We disagree, as we consider the kinematic inversions to be a primary result. We report only the results in Section 4.3.2 - average strike and dip of fault planes, average trend and plunge of slip vectors, and the orientations of fault planes and P- and T-axes from the inversions. We feel it is important to discuss the data inputs and assumptions in the methods section before presenting these results.

Line 207. (range of elevations (∼4-12 m elevation range)): unclear of what this means. Why is there a range on scarp midpoints? Also, there should be a '∼' with a range

Reply: There is a range of scarp midpoint elevations because the scarps cut across topography. We have rephrased this section to clarify: *"We surveyed scarp midpoints at multiple locations along each scarp and determined fault strike and dip through linear regression of a plane through the surveyed scarp midpoints using all surveyed data along a single continuous fault strand segment (3-17 points per regression). More variation in scarp midpoint elevations can reduce uncertainty on fault dip; we surveyed midpoints within a ∼4-12 m elevation range for each scarp segment."* There is a "∼" with the range.

Line 250-257. (We focused our sampling on detrital charcoal as charcoal is present in many deposits on Vancouver Island, has previously been used to evaluate late Pleistocene to Holocene unit ages (e.g., Clague et al., 1980; Morell et al., 2018; Harrichhausen et al., 2021), and because luminescence techniques have not yielded reliable ages for late Pleistocene to Holocene deposits on Vancouver Island (e.g., Graham, 2017; Morell et al., 2018). We collected samples of macroscopic (macro) charcoal (>0.5 cm) where fragments were visible in outcrops of Quaternary deposits. If no macro charcoal was readily visible in an outcrop, we collected 1-2 L of bulk sediment and sieved the samples to extract any dateable macro charcoal present. For all sample sites, we completed detailed unit descriptions and noted the sample's stratigraphic position within the deposit (Figure S3).): Supplementary

Reply: We thank the reviewer for this suggestion and have moved this text to the new Supporting Information Text S2.

Line 261-266. (Charcoal samples were cleaned and processed at Paleotec Services, Ottawa, Ontario, Canada. Macroscopic charcoal pieces were extracted from bulk sediment samples by flotation and wet sieving in warm tap water using nested sieves of 0.85 mm and 0.425 mm. All material greater than 0.425 mm was examined using a binocular microscope, and any isolated charcoal pieces were shaved of any adhering sediment. The largest shaved fragment from each sample was further sliced into smaller fragments to look for the presence of fine modern rootlet penetration and/or fungal contamination, including mycorrhizae, and rejected if contaminants were present.): Supplementary

Reply: We thank the reviewer for this suggestion and have moved this text to the new Supporting Information Text S2.

Line 274-276. (Radiocarbon ages (reported following Stuiver and Polach, 1977) were calibrated using the INTCAL20 calibration curve (Reimer et al., 2020) and OxCal v4.4 (Bronk Ramsey, 1995, 2021)): You could place this in the Table caption

Reply: This information was already in the table caption, so we have removed it here.

Results

Section 4. (Results): I suggest restructuring this section a bit to help with repetition and length. Suggest starting withn Quaternary mapping and stratigraphy first (4.1) and then the Quaternary fault scarps (4.2). To me this make more sense and sets you up to easily describe the scarps and age of slip. Which will then be immediately followed by your offset measurements and slip calculations.

Reply: We have made the suggested edit and have made adjustments to the section to reduce repetition. For example, we have combined the sections on unit descriptions and geochronology.

Section 4.1. (Quaternary fault scarps): Suggest making this subsection 4.2

Reply: We have made the suggested edit.

Please use and apply proper fault scarp terminology following Wallace et al. (1977) and Bucknam et al., (1979)

Reply: We have changed all references to how "tall" a scarp is to how "high" it is.

Line 289. (multiple ages of Quaternary deposits): not established yet.,..

Reply: This has been established with the suggested restructuring.

Line 302. (steep): whats steep?

Reply: We have deleted this sentence as it was repeating information from the prior sentence.

Line 302. (well-preserved free faces, such as strand Ew at Site 1.2 (Figure 8b-c)): this is a bit interpretive. What else tell you its a actual free face? Not clear in Fig 8b-c where exactly that is. Furthermore, the fault interpreted below should connect into it.. If it is a free face, that would be the surface to measure a dip for the fault! suggest saying its a possible free face

Reply: We have deleted this sentence as it was repeating information from the prior sentence.

Line 310-311. (associated with steep NE-dipping fault planes that produce "valley-side up" displacement): I recall you saying there weren't any bedrock faults... Are these other previously mapped proximal bedrock fault planes..?

Reply: These are referring to the active fault planes we calculate later in this section. We have deleted this phrase to avoid confusion.

Section 4.2. (Quaternary mapping and stratigraphy): Suggest placing this section along with radiocarbon results, as your first subheading under Results so you can more easily talk about the fault scarps and their associated age based on what unit they manifest in

Reply: We have made the suggested edit.

Line 353. (heads): ???.. peaks?

Reply: We have edited this sentence to refer to the noses of these deposits.

Line 357-358. (Qp1 is incised by a series of abandoned channels. These channels are disconnected from active streams but merge into the heads of Qp2 deposits, suggesting that they were active at the time of deposition of Qp2.): As I mentioned above, I don't know if I would call these abandon channels. Looking at Fig 3a, I see some of these channels continue upslope forming more of a rill pattern. I guess the obvious question is, when it rains, which it does a lot there, where does the water flow uphill of these 'abandoned channels'? Does it all get captured by the deeper incised channels that continue through? For example on Fig 3a, where does surface flow go from the "400m" contour label? Looks to me it would go straight down and pick one of the "abandoned channels". But I haven't been there. I agree with the interpretation that they were mainly formed during Qp2, but are they truely abandoned? Maybe just oversized..? If they are occasionally occupied by surface runoff flow, then I suspect erosion is going on, thus making vertical offset measurements on these features is not very robust. Furthermore, I would suspect that subsequent slumping within the abandon channel would affect scarp measurements.

Reply: Yes, the water flows in the deeper incised channels that drain the entire rangefront, and not these very small abandoned channels with minimal catchment area. Active or intermittent channels are delineated on Figures 4, 5, and 6. We have experienced torrential downpour in the study area and observed no surface runoff flow where there is any soil development - active rilling occurs only within roads and other areas of anthropogenic disturbance. We have also not observed any evidence of slumping within abandoned channels. We clarify our interpretations and observations on lines 302-305 of the text: *"These channels are disconnected from the active streams where rainfall and snowmelt are channelized, and have negligible catchment area, but they merge into the heads of Qp2 deposits. This suggests that they were active at the time of deposition of Qp2, but no longer accommodate significant discharge."*

Line 361-362. (We interpret these abandoned channels to have formed as the result of fluvial and debris flow scouring and filling associated with the deposition of Qp2): Ok - I agree

Reply: Great.

Section 4.2.4. (Radiocarbon results and inferred unit ages): Suggest combining this with your stratigraphy section and include the C14 results with the unit descriptions

Reply: We have made the suggested edit.

Line 385-387. (We note that the interpretation of detrital charcoal radiocarbon dates can be challenging due to vertical mixing during bioturbation or soil creep, recycling of older charcoal into younger deposits, and bias from younger carbon (e.g., roots) included in older charcoal.): This is a general understanding all to say that detrital charcoal ages represent a maximum age of that deposit.

Reply: We have deleted this sentence in the suggested restructuring.

Line 406-408. (Qft yielded a charcoal sample (BR18-42C) sieved from bulk sediment collected from a stream cut exposure of stratified pebbles and cobbles, located <10 m downhill from mapped fault strand Ee (Figure 4a, Figure 3). This sample yielded a radiocarbon age of 3.5 cal ka BP (Table 1).): See purple comment below

Reply: See response to comment below.

Line 411-412. (If we assume that these samples reflect deposit ages, and are not significantly altered by recycling, bioturbation, or inclusion of younger carbon, these data suggest the following as possible brackets on the ages of mapped deposits): Or you can more simply say these radiocarbon results represent maximum ages to their associated deposits...

Reply: We refer to these as age approximations, not maxima, because of the possibility of the introduction of anomalously young charcoal.

Line 414. (Qft deposits are <4 ka): I would be carefull with this generalization for all terrace deposits amoungst the drainage channels - likely a signal of mass transport deposition upstream, which could be random throughout the drainage headwaters... The one age you have for Qft is a good maximum for this deposit, but would be hesitant to apply to other drainages. If anything, Qft is younger than the associated Qaf

Reply: We have added text to clarify this point: "Qft deposits are younger than Qaf, locally constrained to <4 ka."

Line 415. (age approximations): Or max ages

Reply: See comment above.

Section 4.3. (Fault offset measurements): I'm a little confused with this section. I'm finding it difficult to keep track of your vertical and horizontal measurements. First you state 'scarp heights', which are not offset measurements. Not sure if the correct term is applied here. If you do mean scarp height, suggest using this terminology in the section describing the scarps rather than the measurement section. I think this is a result of incorrect terminology - please refer to comment above following proper scarp terminology. Second, I don't quite get why you are measuring vertical offset of channel thalwegs over the interfluve crest? A VS measurement on the interfluve crest would provide a minimum offset value with a max age. The thalweg is younger than interfluve (obviously), but how much younger? You say they are abandon, meaning no deposition is occurring, but no ages constrain this abandonment. I think this paragraph needs a lot of work as its once of the main dataset that you use to estimate your fault kinematics. Focus on reporting that actual offset measurements more clearly. I suggest your vertical measurements be of the interfluve crest and younger terrace (Qft) surfaces (for reasons I mentioned above in methods) and lateral measurements of channel thalwegs.

Reply: See comments above on scarp heights. We have rearranged Sections 4.2 and 4.3 to clearly separate field observations from offset measurements. We are not choosing to measure thalweg offset over interfluve offset, but rather measuring both. It is true that we have no age constraints on the abandonment of the channels developed in Qp1. That is why we have used these data to constrain the number of events, but not the timing of those events. We have rewritten the first paragraph (now two paragraphs) of Section 4.3.1 to detail the dip slip, strike slip, and oblique slip across individual strands and the cumulative dip slip, strike slip, and oblique slip summed across multiple strands. We do not measure vertical and lateral from separate sources because we are specifically interested in the obliquity of the BRF. Combining vertical measurements from some landforms and lateral measurements from others would be inappropriate as they formed at different times, and therefore do not necessarily reflect the cumulative slip of the same number of events.

Line 419. (scarp heights): Why scarp heights? shouldn't you be estimating vertical offset here?

Reply: This is in reference to field observations. We have moved this text to Section 4.2 to more clearly separate field observations from measurements.

Line 420-421. (These observations suggest cumulative displacements of several meters across multiple fault strands.): I don't understand the evidence for this yet. you have scarp height measurement, but no vertical measurements, and some lateral... Suggest removing this as you discuss cumulative displacements below

Reply: We have made the suggested edit.

Line 421. (scarp heights): confusing - unsure if you reporting the correct value here....

Reply: See response to comment on line 419 above.

Line 423-426. (Our field observations and survey data also show that scarp heights in older deposits and landforms, including the interfluves developed in Qp1 at Site 1 and the till-mantled hillslopes at Site 2, have larger vertical displacements than the younger channels incised into these deposits.): This is an important point, but poorly shown in Fig 9 as is

Reply: Thank you for pointing this out. We have edited figure 9 to clearly show the different deposits in addition to the different landforms (thalweg vs. interfluve).

Line 426: I think this paragraph needs a lot of work as its once of the main dataset that you use to estimate your fault kinematics - confusing with scarp height values. I think you should focus on reporting that actual offset measurements more clearly.I suggest your vertical measurements be of the interfluve crest and younger terrace (Qft) surfaces (for reasons I mentioned above in methods) and lateral measurements of channel thalwegs.

Reply: See comments above.

Line 426. (Figure 9b): Where do I find vertical displacements in 9b...? Do you mean 9a..?

Reply: We have corrected the text to reference Figure 10.

Line 428. (one to three strands): the active fault zone..?

Reply: We are referring to the number of strands across which we have surveyed, which may not reflect cumulative displacement across the entire active fault zone.

Line 429. (Figures 5, 9): Units not shown in either Figure.. See suggestion for Fig 5

Reply: We have added the mapping to figures 5 and 6.

Line 434. (due to limited exposure and preservation): I don't understand what you mean by exposure. Doe you mean piercing points?

Reply: We have edited this sentence to say "limited access and preservation".

Line 439. (younger abandoned channel shows only 4.7 m of vertical separation): I don't think this is a good example greater amounts of offset within older deposits 1) From what I can tell in Fig 4 and 5, these profiles are solely within unit Qp1. I don't see another unit delineated within the drainage. 2) yes, the drainage is a younger geomorphic feature than the surface of the interfluve, but theres active erosion going on. Why couldn't your lower offset value be a product of that? Please refer to comment above about vertical separation measurements in channels vs interfluves.

Reply: Yes, the interfluves and channels are both developed in unit Qp1. However, that does not mean that these landforms are the same age as the deposit. Rather, the channels are erosional features that formed after deposition of Qp1, during deposition of Qp2. Another unit is not required to establish differential offset of different age landforms. We have added the phrase "incised into Qp1 during the formation of Qp2" to clarify this point.

We disagree that there is active erosion in these channels. Seasonal rainfall and snowmelt is confined to the active and intermittent streams shown on Figures 4 and 5. The catchment area for each of these channel forms is only 10s m2.

Line 439-440. (A younger Qft fluvial terrace, which crosses adjacent fault strand Ee): Based on this description - it looks like you are taking this measurement from profile 3..? (please be clear). On fig 5a you have an inferred fault line → I don't understand this. If there is a scarp, and this VS measurement is true, then YES, this would be a great example of your thesis of the paragraph.

Reply: Thank you for the suggestion. We have added a reference to profile 3. We have also corrected Figure 5a to reflect our confidence in this scarp.

Line 440-441. (Similarly, at Site 2, the till-mantled hillslope typically has larger vertical separation than channels incised into till): Again - not a good offset comparison

Reply: We are unsure why the reviewer feels that this is not a good offset comparison.

Line 441-446. (For example, profile 28 at Site 2 in Qt shows 4.1 m of vertical separation across strand Q, whereas profile 33 along a younger channel incised into Qt shows only 2.9 m of vertical separation (Figures 9, 10b). These same relationships are mimicked in the set of cumulative 3D displacement measurements at 23 interfluves and channels at Site 1 (Figure 9), which show that older interfluve developed in Qp1 consistently have 4 to 10m more cumulative oblique displacement as compared to young channels incised into Qp1 (Figure 10c).): No bueno

Reply: We would like to point out that the inclusion of the comment "no bueno" by the reviewer is not only inappropriate but it also is not constructive. Based on this comment, we do not know why the reviewer highlighted these sentences and why they disapprove of them.

Section 4.3.2. (Slip vectors and fault kinematics): Suggest you place this section in Discussion

Reply: We disagree - see comment above about Section 3.2.3.

Discussion

Line 466-468. (Mapped scarps form en echelon steps, and parallel arrays exhibit geometries common in strike-slip fault systems and pull-apart basins, especially in immature faults with little cumulative offset): awkward

Reply: We have rearranged this sentence to be less awkward.

Line 468. (Mapped scarps form en echelon steps, and parallel arrays exhibit geometries common in strike-slip fault systems and pull-apart basins, especially in immature faults with little cumulative offset (e.g., Hatem et al., 2017; van Wijk et al., 2017)): suggest adding reference below - its a much older idea than 2017. Wesnousky, Steven G. 1988. "Seismological and Structural Evolution of Strike-Slip Faults." Nature 335 (6188): 340–43.

Reply: We have added the suggested citation.

Line 469. (The magnitudes of displacement): please state here in parentheses so I don't have to go up and find these values

Reply: We have made the suggested edit.

Section 5.2. (Kinematics of the BRF and relationship to inherited structures): I think more consideration is needed for the interpretation of large-scale transtensional faulting. While the pattern and characteristics of surface faulting does appear to reflect transtension, the possibility of these fault scarps being bending-moment faults within the hangingwall of the BRF need to be address as it may be a possibility here.

Reply: We disagree strongly with the reviewer's assertion that our mapped scarps relate to an origin as bending-moment faults. We have made our hypotheses on the kinematics of the active fault based on our data. Our data show that the lithologic bedrock fault trace separating the Nanaimo Grp sediment from the Karmutsen basalts is not an active structure today, and that mapped active scarps occur in both the hanging wall and footwall of inherited thrust faults (inconsistent with the bending hypothesis). In addition, our field observations and prior published mapping confirm there is no anticline in the hanging wall of the thrust. Therefore, our data do not reflect secondary motion related to thrust fault reactivation. We have expanded Section 5.2 of the text to detail the evidence that the inherited thrust fault is not active in the Quaternary, and therefore not a source of these scarps.

Line 501-502. (both the hanging wall and footwall of inherited thrust faults (Figure 4) suggest that there is not a strong inherited lithologic or mechanical control on the position of the active BRF at the surface.): What if the main fault trace of the BRF is spatially inaccurate? Very few scarps lie south of the inferred trace. Why couldn't these scarps be secondary fold-accommodation faults - possibly bending-moment faults?

Reply: The main fault trace is not mapped inaccurately. Our observations and those of others allow us to directly map the lithologies and the fault strands in the locations as shown in Figure 4. We have updated and refined the previous mapping of the bedrock thrust fault in this paper (see Figure 4, "this study"), and we have added text to the methods section to clarify this. We observe scarps in the hanging wall and footwall of the bedrock thrust fault, including at Site 1 where there are two main strands of the bedrock fault.

Line 508-509. (These data suggest that it is possible that the active BRF may reflect the formation of a new fault): So a new fault that slips opposite to the 'Older structure' (the BRF) - this inferres a change/rotation of stress field. When and why?

Reply: We have added text describing the changes to the CSZ since the Eocene: *"The Cascadia forearc stress field has changed significantly since the Eocene, with the cessation of terrane accretion, northwards migration of the northern terminus of the CSZ since ∼15 Ma (Madsen et al., 2006), and formation of the Explorer microplate ∼4 Ma (Audet et al., 2008). Paleomagnetic data and geodetic rotations also show that the forearc has been rotating since ∼18 Ma (Finley et al., 2017)."*

We do not know when or why this change occurred, but the data suggest that it is a valid hypothesis.

Section 5.3. (Evidence for multiple surface-rupturing late Pleistocene to Holocene earthquakes): I think more consideration is needed for this section - please see comments above about comparing VS measurements from interfluves and thlwegs

Reply: Please see our responses to the above comments about interfluve and thalweg vertical separations.

Line 515-517. (The strongest evidence for multiple events comes from the differential scarp heights and cumulative slip magnitudes calculated for offset landforms of different ages at Sites 1 and 2 (Figure 10).): I disagree - See comments above

Reply: Please see our responses to the above comments about interfluve and thalweg vertical separations.

Line 520-521. (The differential offset between interfluves, channels, and fluvial terraces indicates the occurrence of at least three events since the deposition of Qp1 at Site 1 (∼13.6-9.5 ka).): I think 1 for sure and likely 2 earthquakes since. The thalweg vertical measurements should not be considered. The VS measurement across the Qft surface is questionable since you've mapped it as an inferred trace. The rather large offsets (>∼5m) are suggestive of the second. Other than this, I'm having a hard time seeing this.

Reply: We have updated the figures to denote a confident fault trace at the Qft2 terrace. Please see our responses to the above comments about interfluve and thalweg vertical separations. We also note that the thalwegs consistently have less right-lateral displacement than the interfluves. We have added the right-lateral to Figure 10c to better show this.

Line 524. (simplifying assumption): bsed on what..? Empirical scaling relationships..?

Reply: Yes, based on empirical scaling relationships. We have added clarifying text: *"Furthermore, if we make the simplifying assumption that a single event produces ∼1-3 m of oblique slip, based on the average difference in cumulative oblique displacement between interfluves and channels, empirical scaling relationships (e.g., Wells and Coppersmith, 1994) suggest the cumulative oblique slip of ∼10-15 m across the BRF may be the product of more than three events since ∼11-14 ka."*

Line 529. (before the abandonment of channels incised into Qp1): Ok - when was this?

Reply: As stated above, we do not have precise age controls on channel abandonment.

Line 561-565. (An earthquake of a similar magnitude today would pose significant hazard not only to Port Alberni, but also to the nearby communities of Nanaimo, Parksville, Qualicum Beach, and Courtenay (Figure 2a). Failure of dams on Comox Lake and Elsie Lake could lead to flooding of communities downstream (Figure 2b), as well as impacts on power availability, as nearby power stations supply 11% of the electricity generated on Vancouver Island (BC Hydro, 2023).): Not needed - a lot of speculation. Let the ground motion modelers deal with that

Reply: We have simplified this sentence to only list infrastructure nearby: *"An earthquake of a similar magnitude today would pose significant hazard not only to Port Alberni, but also to the nearby communities of Nanaimo, Parksville, Qualicum Beach, and Courtenay, and dams on Comox Lake and Elsie Lake (Figure 2b)."*

Section 5.4. (Estimate of Late Pleistocene to Holocene slip rates): Suggest revisiting after addressing concerns above

Reply: We have edited this section to separate out dip slip and strike slip rates from oblique slip rates, as suggested by Reviewer 2.

Line 587. (we prefer the slip rate of 0.7-1.3 mm/yr from the more conservative open-ended method): Agree - the 'closed-interval' is quite uncertain, maybe even speculative.

Reply: Okay.

Figures

Figure 1/Lines 89-90. (Nootka fault zone): Not on map

Reply: We have deleted the sentence that references Nootka.

Figure 4. Suggest using a brighter and different color for samples. Too similar to Qt

Reply: We have made the sample symbol white.

Figure 5. I think having the unit mapping here would be more useful. . .

Reply: We have added the mapping to figures 5 and 6.

Figure 8 caption. (Tall): State the scarp height instead

Reply: We have edited the caption to include scarp height.

Figure 9. Would be useful to put a diagram of the relative age of the colors so I don't have to scroll back up to Fig 4 for the colors. Or am I wrong - are the colors associated just with strand letter..? Unclear.
Colors need more description in caption or as a picture
Suggest adding some age reference to these offset measurements
Line 438. (Qp1): Suggest adding this label to the figure

Reply: We have edited this figure to clarify the use of colors and shapes in each panel. We have also added the mapped units to the figure.

Figure 10. Would be useful to reader to state age in figure

Reply: There isn't room to add additional text to Figure 10, but we have added the ages to the caption.

Reviewer 2

In this paper Lynch et al. present new observations to suggest that the BRF is an active oblique-slip fault. They use lidar data combined with field studies and several radiocarbon ages to propose a slip rate and event history for the Holocene. The work is an important and timely contribution to our understanding of upper plate faulting in Cascadia.

The work should be published following major revisions. I have a few concerns with the interpretations and presentation, ranging from minor to major, along with a number of line comments. I also think the paper is rather long given the sparse amount of data available and limited number of fault observations. Some effort could be made to reorganize and simplify the paper as well as to reduce the number of redundant/repetitive statements (e.g. glacial history/methods/dating could be reorganized & combined into later sections). Overall, it is a good effort with a good attention to detail and should be a valuable contribution once these issues are addressed. The authors are free to contact me with any questions/discussion.

-Ian Pierce, ian@nevada.unr.edu

Major questions/comments

How long has this fault been active? Why is there such a much larger vertical than horizontal component of slip? If this is reflective of the long term, then how do you explain the mountain's geomorphology?

Reply: The fault has been active at least since deglaciation. Because of the lack of surficial deposits prior to this time, we are unable to constrain the maximum age further. We do discuss possible explanations for the mountain's geomorphology in Section 5.2: *"The NE-side-down slip sense we determine for the Quaternary-active BRF produces a "range-side down" sense of motion. This result suggests that the high elevations and steep topography associated with the southwestern flank of the Beaufort Range were not formed by transtensional slip along the active BRF. Instead, the steep range front may be the product of differential erosion of the softer Cretaceous Nanaimo Gp sediments that underlie the Alberni Valley, relative to the more resistant Karmutsen Fm basalts that underlie the range crest (Muller and Carson, 1969), consistent with typical glacial valley morphology. Or, it may imply that the net range-side-down (NE-side down) motion integrated over 100s kyr to Myr across the active BRF may be small relative to the amount of Eocene NE-side-up thrust fault displacement. The small cumulative magnitude of NE-side-down motion could indicate that transtension across the BRF is a relatively young phenomenon, and has not accrued a large magnitude of vertical displacement."*

Why is the morphology so different at site 1 vs site 2?

Reply: We are uncertain what the reviewer is referring to here. But the bedrock geology and Quaternary deposit stratigraphy are different at site 1 and site 2, which likely leads to major differences in range front geomorphology, and differences in how faults rupture toward the surface, thus impacting scarp morphology.

I am concerned that the confidence in the fault is overstated. There are only two very short (∼1.5 & 0.5 km long) sections of scarps interpreted to relate to a 40+ km long fault. I have not seen compelling evidence for lateral displacement. More care/effort should be shown to demonstrate clear lateral displacement along with uncertainty. This will require more detailed figures showing clear lateral displacements. Without this, it takes away from the interpretation that this a fault and not a landslide/glacial deposit. With the proximity to Cascadia (and the local M7.3 earthquake) I would expect this area to have some very large landslides. Likewise, some of the features seem to be possibly consistent with valley margin glacial morainal deposits. Furthermore, many of the scarps, especially in Site 1, appear to be aligned/along a prominent contact in q-units that is mapped on Fig 3. This contact is evident in the hillshade as there is a clear contrast in material properties.

Reply: There are more than a few km of scarps - Figure 4 shows two ∼6 km segments, and additional scarps are shown in Supporting Information Figure S1. In total we map ∼20 km of scarps over a distance of ∼40 km.

We have updated Figure 2 to show our detailed mapping rather than a simplified line. We describe how we distinguish tectonic scarps from gravitational failure in Supporting Information Text S1. None of the deposits we map are moraines, which are composed of glacial till. The exposures of paraglacial deposits Qp1 and Qp2, and the post-glacial deposits are quite distinct from glacial till.

In response to the reviewers comment that "they have not seen compelling evidence for lateral offset", the paper presents 54 field-surveyed right laterally offset geomorphic piercings lines each averaging several meters of lateral offset. To help make this lateral offset clearer we have added several field photos of offset channels (Figure 8a-c) and a plot of the right lateral component of offset from surveyed piercing lines (Figure 9b).

In response to the reviewer's comment that scarps at Site 1 appear to be aligned/along a Quaternary unit contact in Figure 3, we note that A) There is no geologic mapping in Figure 3 so we assume they refer to Figure 4. B) The faults are not at the contact between Quaternary deposits - they occur 50-100 m away from the nearest contacts. We note that Figure 4 is a regional map and we refer the reviewer to the updated Figure 5 (the more detailed map at this site) that clearly shows the scarps are not along Quaternary unit contacts.

The kinematic inversion method is an interesting, new (to me), and useful way to get at oblique slip. However, this seems to mask the evidence of lateral slip that is really required for such a steep dipping fault. As currently presented, I worry this method is overinterpreting the available data. I would like to see the lateral & vertical displacements separated throughout the manuscript and reported along with the net oblique displacement. This would help the presentation a lot to reduce uncertainty. Labeling displacements on the figures would also be helpful.

Reply: We thank the reviewer for the suggestion. We have rewritten Section 4.3.1 to more clearly lay out the dip slip, strike slip, and oblique slip of individual strands and cumulative across multiple strands. We have also added the dip slip and strike slip components to Figure 9.

Are there other examples of faults with a near vertical dip, inverted scarp morphology (e.g. uphill facing), and such a high (apparent) ratio of vertical:horizontal? This is concerning as the long term morphology of the mountain front is the opposite of the short term scarp morphology interpreted to be a result of repeated faulting. This is addressed a bit, but is somewhat glossed over despite being a major conflict.

Reply: Yes, this occurs elsewhere in the northern Cascadia forearc along the Leech River fault (e.g., Harrichhausen et al., 2021; Morell et al., 2017, 2018), the North Olympic Fault Zone (e.g., Nelson et al., 2017; Schermer et al., 2021)), and the Boulder Creek fault (e.g., Sherrod et al., 2013). Similar morphologies have also been described along portions of the San Andreas fault (e.g., Lindvall et al., 2002; Prentice et al., 1999), the Andes (e.g., Tibaldi and Ferrari, 1992), in Tibet (e.g., Murphy and Burgess, 2006), in Iran (e.g., Ritz et al., 2006)Ritz et al., 2006, and the Eastern Tennessee Seismic Zone (e.g., Thompson Jobe et al., 2024).

We were similarly puzzled with the relationship of the topography with the fault slip sense when we first discovered the fault kinematics and their relationships to topography. When we were considering why the data could to be in apparent conflict, we turned to the observation that, in most subduction forearcs, long term displacement rates along forearc faults are slow, such that their role in generating local topography can be small (e.g., Regalla et al., 2017). Exceptions are in locations where collision is occurring with subducted seamounts or oceanic plateaus (e.g., Sitchler et al., 2007), but such a collision is not occurring today in Cascadia. We also considered that inversion of fault slip can be quite common (e.g., Farías et al., 2011; Loveless et al., 2010; Regalla et al., 2017), especially for regions with long-lived inherited faults such as forearcs.

I am quite surprised that the dip is so steep (nearly vertical) to produce so much vertical motion. Alternately, if the dip is shallower, then it does match the bedrock fault quite well (which makes sense)- but in this case then it doesn't match the M7.3 earthquake as well.

Reply: Fault strands dip ∼60°-88°, which is the expected range for normal (60°) to transform faults (90°) in Andersonian mechanics. This is observed elsewhere at numerous locations. Large amounts of vertical offset can also occur along strike slip systems with local transtension (i.e. negative flower structure). The large magnitudes of vertical offset occur at site 1, in a location with en echelon scarps that we interpret as a negative flower structure.

Better presentation of the M7.3 triangulation results could be given, including showing them on one of the figures (Fig 2?) and also including the uncertainties. This is an interesting and important datapoint relevant to the story.

Reply: The M7.3 focal mechanism is shown on Figure 2 in both panels, but there isn't space to add the triangulation results. We have added the uncertainties to the new Section 2.3.

Are there other [primarily] extensional faults in the forearc? I am only really familiar with southern Cascadia where they are all thrusts. If not, why is this one special? If so, how does this fault compare to those?

Reply: We are not aware of any other active extensional faults in the forearc of northern Cascadia. However, normal faults in forearcs are relatively common (see northern Chile). Given our already long paper, we feel a robust discussion of this is outside the scope of our current paper.

What is the kinematic role of this fault in the CSZ? This could work for a discussion section along with a schematic figure.

Reply: Such a discussion is interesting, but outside the scope of our current paper, and would make the paper even longer. We have added a summary of possible tectonic drivers to Section 5.6: *"A change in strain field may be related to spatial variations in principal stress orientations and magnitudes in the upper plate that locally promote transtension along the BRF. While the data presented here are not sufficient to determine the causes of this potential change in the upper plate deformation field, there are several possibilities, including oroclinal bending (Finley et al., 2019; Harrichhausen et al., 2021; Johnston and Acton, 2003), spatial changes in plate tractions, convergence rate, or obliquity (Li et al., 2018; McCaffrey et al., 2013; Wang, 2000; Wells et al., 1998), interaction with the Explorer plate and Queen Charlotte fault (Audet et al., 2008; Braunmiller and Nábělek, 2002), or the "escape" of forearc crustal blocks related to north-directed shear from southern Cascadia (Nelson et al., 2017)."*

Line Comments

Abstract

Line 16-17. Phrasing the paper motivation in terms of the 'deformation field of the forearc' is a little weird. I suggest to reframe it maybe in terms of 'upper plate faulting' or similar.

Reply: We have made the suggested edit.

Line 17. 'poorly constrained' is a vague statement, I suggest to rephrase it to better articulate what is meant.

Reply: We have made the suggested edit.

Line 20. documented … at the time? Evidence of Quaternary activity is referring to the fault not the earthquake presumably? It is a bit disconnected in this sentence.

Reply: We have made the suggested edit.

Line 24. Kinematic slip inversion? This is a new term for me. Is there a way to simplify this concept for readers unfamiliar with this?

Reply: Given the word limit, we do not know how to simplify this discussion in the abstract. However, we have added *"Kinematic inversions use fault orientations and slip vector data to invert for, or reconstruct, the associated stress state (e.g., P- and T-axes)."* to the main text on lines 238-239.

Line 26. Usage of ; breaks up the natural flow of the text, and makes it a bit awkward to read. I suggest to use less of these.

Reply: We have removed the semi-colon.

Line 27. decades? Presumably that is via GPS → just state that here.

Reply: We do mean GPS, but we are referring in this case to the time scales.

Non-technical summary

Line 37. The 1946 earthquake had offsets → in the abstract you said there was no surface rupture identified?

Reply: Thank you for pointing this out. We have edited this sentence to say motion rather than offsets.

Line 40. typo: 'observe suggest'

Reply: We have rephrased this sentence to be more clear.

Introduction

Line 45-52. This is an OK motivation, but I think could be improved a bit. Studying every fault in the system is important to understand how stress and strain are distributed. Having the first words of the paper 'quantifying the stress & strain rates' is a bit of a round about to get there. I think maybe it is OK as is, but I would consider rewriting this paragraph to emphasize that really it is important to study these smaller faults to determine how much deformation they accommodate vs. what we can attribute to the megathrust. I think that is the key you are getting at but it isn't as clear as it could be.

Reply: Our introduction focuses on the importance of both long- and short-term data for understanding the strain and strain rate field of the forearc. We have restructured and rewritten parts of the introduction to clarify this.

Line 52. 'earthquakes on upper plate faults' isn't really a method, so this sentence reads a bit weird.

Reply: We have edited this sentence following Reviewer 1's suggestion.

Line 57-59. this seems like a really important point, are there any references more recent than 1995?

Reply: We have added recent citations.

Line 64. 'Is one location where' is an awkward phrase.

Reply: This phrase is no longer present in our restructured introduction.

Line 65-67. phrasing is awkward

Reply: We have edited this sentence following Reviewer 1's suggestion.

Line 71. What is the uncertainty on 7.3? That predates moment magnitude scale. I think this is a unnecessarily bold statement when there is the 1992 Mw7.2 earthquake. Has anyone digitized the 1946 records and reprocessed / relocated this event?

Reply: The M 7.3 for the 1946 event comes from Rogers and Hasegawa's relocation in 1978, as cited in this sentence. We are not sure to what 1992 Mw7.2 event the reviewer is referring.

Line 77. tectonogeomorphic – just say tectonic geomorphology

Reply: See response to Reviewer 1 above.

Line 81. 0.7-1.3 mm/yr isn't really 'highly active' in a plate boundary setting. It is ~2.5% of the strain you report in the next paragraph.

Reply: We have removed the word "highly" following Reviewer 1's suggestion.

Line 80-84. This is an area you could reduce the length of the paper. The roadmap overview is good, but no need to summarize the entire paper again in the intro. That is already done in the abstract.

Reply: Thank you for the suggestion. We have shortened this paragraph.

Line 82-83. pedantic, but 1mm/yr is pretty low-strain still, so this conclusion isn't very precise in wording.

Reply: This phrase is no longer present in our restructured introduction.

Line 83. 'low rates of geodesy' what does this mean? Geodesy is just the study of the shape of the earth. Low deformation rates based on GNSS?

Reply: This phrase is no longer present in our restructured introduction.

Line 84. transtension instead of transpression is surprising to me given the near parallel strike with the CSZ. Is it accommodating hanging wall flexure/anticlinal folding? Might be good to stick a sentence in the intro about this.

Reply: We have decided to leave this for the discussion.

Background

Line 88-89. these distances aren't necessary when you have Fig 1- could shorten the paper here.

Reply: We have made the suggested edit.

Section 2.1. missing a bit of background on how these faults work. Could be good to give some background on the rates, magnitudes, deformation style etc. of the other upper plate faults. Maybe a bit more on Cascadia too. How far are we from the Queen Charlotte fault? Does this fault's obliquity relate to that?

Reply: We have added information on the number of earthquakes and range of slip rates on these forearc faults, as well as the location relative to the Queen Charlotte fault to lines 97-104: *"The BRF is positioned ∼60 km south of the projected northern terminus of the Juan de Fuca slab, where subduction decreases as the Explorer microplate moves north in response to the right-lateral Queen Charlotte fault (Figure 1). Active faults that accommodate forearc strain have been recognized along most of the Cascadia subduction zone in the United States and southernmost Vancouver Island (e.g., Figure 1; Bennett et al., 2017; Brocher et al., 2001; Goldfinger et al., 1992; Harrichhausen et al., 2021, 2023; Horst et al., 2021; Kelsey et al., 2008; Liberty et al., 2003; Morell et al., 2017, 2018; Personius et al., 2003; Schermer et al., 2021; Sherrod et al., 2004; Wells et al., 2020). These faults have each hosted at least one to five earthquakes in the Quaternary, and have slip rate estimates ranging from ∼0.1 mm/yr to ∼1.5 mm/yr."*

Line 107. if it dips 45 it is a reverse fault not a thrust?

Reply: Yes (near the surface), however we refer to it as a thrust fault to be consistent with previous literature.

Line 113. awkward phrasing

Reply: We have rephrased this sentence.

Line 114. citation needed ('several researchers')

Reply: We have added citations following Reviewer 1's suggestion.

Line 115. Depth <30 km could be clarified (shallower or deeper than 30?) How deep is this relative to the subduction interface?

Reply: The "<" symbol means shallower than 30 km. We have clarified that this is 10 km above the subducting slab.

Line 116. focal mechanism solutions → focal mechanism?

Reply: We refer to the multiple possible solutions for the one earthquake.

Line 118. citation missing (geodetic surveys)

Reply: Thank you for pointing this out. We have added the missing citation.

Line 119. that is a lot of slip to be missing!

Reply: It is! It is also a densely forested area with limited access, especially in 1946.

Line 119. clarify if it is oblique compression or oblique tension

Reply: We have separated out the right-lateral and normal slip.

Line 125. no more recent references? These predate cosmogenic dating

Reply: These ages were determined using radiocarbon. We are not aware of any cosmogenic dates constraining local deglaciation ages on Vancouver Island. We have added a reference to a more recent review paper summarizing the evidence for glaciation in SW BC (Clague and Ward, 2011).

Line 129. this date overlaps with your phase 1 date from line 125

Reply: We have rephrased this paragraph to clarify that there were two types of glaciation - the Cordilleran Ice Sheet and a valley glacier. The valley glacier occupied Alberni Valley during the end of the Fraser stage.

Methods

Line 134-136. first sentence is very wordy

Line 136. DEMS → DEMs

Line 137. sets of what? I suggest rephrase here to en echelon arrays comprised of 1-6 subparallel fault scarps ... or similar. How high are these scarps?

Line 141. is that vertical or horizontal displacement? Need to clarify

Reply: We have deleted the introduction to the methods.

Line 143. I think you need a zoomed out Fig 3A to properly show how these channels relate to the glacial geomorphology if you want to make the claim that they are post-glacial. A geomorphic map showing the extent and ages of glacial deposits is also needed to make this claim. (I see this now in Fig 4 but it should be referenced here). As currently presented it is not clear that these channels postdate the glacial activity. Looking at the hillshade in the supplement, I do not see clear evidence of glaciation along this margin of the range. Valleys are generally V shaped and quite steep, suggesting that there were not cirque glaciers emanating from the W side of the range (there are U valleys on the E side). The alternative is if the entire Alberni valley were glaciated, but the extent of ice and resetting of these drainages on the mountain side is unclear as presented.

Reply: Most of the Alberni Valley surficial geology was not mapped prior to this study, so we cannot refer to previous maps. The entire Alberni Valley was glaciated, as stated on lines 135-136: *"During the retreat of the ice sheet, the Alberni Valley was occupied by a southeastward flowing valley glacier"*. A zoomed-out Fig. 3a is now included in Figure 5. Kame terraces, up to 40-m-thick glacial till, streamlined bedrock surfaces, all indicate that the area was extensively glaciated in the Pleistocene. We discuss this in Section 2.2.

Line 134-143. this isn't really methods, this is the result of your mapping. Some reorganization of this section could help save words.

Line 141-151. this isn't really methods either, reads more like a summary. If you need a methods section (you might get away with just deleting it completely) then try to leave the results out.

Reply: We have deleted the introduction to the methods.

Line 153. where is the bedrock mapping shown? I don't see it on the e-supp.

Reply: Bedrock mapping is included in Figure 4 with surficial mapping.

Line 156. Somewhat weird selection of citations, there are earlier/more seminal papers showing the need for dating offset landforms.

Reply: Thank you for pointing this out. We have updated the citations.

Line 162. unclear if it is airborne lidar, but I assume so. State that in the text.

Reply: Thank you for pointing this out. We have added "airborne".

Line 165-166. it is inappropriate to grid a 1.2 pixel/m pointcloud to a 0.5 m/pixel DEM (4 cells/sq m).

Reply: We have rephrased this sentence (now in the Supporting Information) to clarify that this was the average cell size after triangulation.

Line 205. I think a 3 point problem approach is inappropriate at this scale. You show that these scarps are echelon shears, that implies they are reidels forming some level of local flower structure. The shallow fault dip is somewhat meaningless here relative to the deeper (even shallowly deep-like 20-100 m where these reidels might merge).

Reply: The shallow fault dip is important when calculating offsets, as we need to pinpoint where the profiles intersect with the fault.

Line 233. I have never seen this approach used before with an active fault study, and I am concerned that this approach is over-interpreting fairly vague geomorphic observations. None of the shown lateral displacements so far are compelling. The lateral & vertical displacement measurements should be separated more clearly in the text and following figures.

Reply: This method has been used before, including in New Zealand (Claypool et al., 2002), Walker Lane (Sturmer and Faulds, 2018), Korea (Kim et al., 2023), Chile (Machuca et al., 2021), Italy (Stemberk et al., 2019), and with surface rupture data from the 1999 Chi-Chi earthquake (Blenkinsop, 2006). We disagree that we are over-interpreting vague geomorphic observations. Our inversions are based on fifty individual slip vector calculations, and are one of several lines of evidence for right-lateral transtension.

We have also separated the lateral and vertical displacements on Figure 9 and in the text of Section 4.3.1.

Results

Line 282. I do not think that numerous right-lateral displacements have been shown so far based on what has been presented.

Reply: We disagree. We show right-lateral displacement at 54 different locations (Figure 9; Table S2). To help make the right lateral offsets even clearer, we added several field photos of offset channels

(Figure 5a-c) and a plot of the right lateral component of offset from surveyed piercing lines (Figure 9b).

Line 309-318. there is a bit too much interpretation driven motivation here. E.g. you do not know the dip that these scarps are associated with (or at least have not presented it so far).

Reply: We have removed the reference to fault dip.

Line 311-312. see my comment before on Fig 3 – but what about a shallow block sliding type failure? That could form an uphill facing scarp at the top of the block, and could explain some of these features – especially if the material is a loose glacial deposit sliding atop a competent bedrock unit.

Reply: See response to Reviewer 1's comments about side-hill slumping. Primary observations arguing against this hypothesis relate to the right-lateral displacements measured.

Line 318. What about glacial depositional features? E.g. a valley marginal moraine?

Reply: Moraines do not form horsetail splays, nor do they right-laterally offset drainages incised into them. Moraines are composed of glacial till, inconsistent with our observations of offset paraglacial deposits (Qp1 or Qp2). We also explain how we rule out scarp formation by non tectonic processes in Supporting Information Text S1.

Line 331-333. A figure to show photos of these bedrock fault exposures would be helpful here. A map too.

Reply: We have added references to Figures S1 and S7, which show locations and stereonets of the bedrock fault exposures. We have also added a new figure, Figure S5, which shows the bedrock fault exposure at Site 2.2.

Section 4.2. A figure here might be helpful to show the landscape evolution in different stages/time steps. I would like to see a figure showing the interpreted ice extent and then subsequent unit development. Figure 4c looks really nice and could be modified to do this. It would help the reader to understand the relationship between the glacial deposits and faulting.

Reply: Thank you for the suggestion. We have made the suggested figure. It is now Supporting Information Figure S8.

Section 4.2. this section would go better before the description of all of the interpretation methods of the fault geometry calculations.

Reply: We have moved Section 4.2 to the beginning of Results (now section 4.1).

Radiocarbon ages. put the sample type (e.g. charcoal/bulk) next to all mentions of sample name & age on figures (and in text).

Reply: We feel this is not necessary to distinguish between the samples as the dated material was charcoal for each instance in figures and text. The details of whether this was macro charcoal plucked from an exposure or sieved from a bulk sediment sample are presented in Table 1. Field photos of the sample sites are in Supplemental Figure S3.

Line 420. This implies a vertical to lateral rate of 3:1. This seems difficult to do with a vertical fault plane.

Reply: In this sentence we refer to a range of scarp heights and apparent right-lateral offsets observed in the field, not individual calculated displacements. We have moved this text to Section 4.2 to clearly separate our field observations from calculated displacements. We also note that the active fault planes are not vertical - we use mean dips of 72-77° in our calculations.

Line 448. But on line 420 you suggested 3:1 ratio.

Reply: We did not. See above comment.

Line 454. Here you say 5 m but on line 420 you said maximum SS is 2 m?

Reply: See above comment.

Discussion

Line 465. I have some questions with the interpretation so far, so I might suggest not to state they are unequivocal.

Reply: We have deleted the word "unequivocal".

Line 467. I agree the echelon scarps suggests strike slip

Reply: Great

Line 469. I am a bit confused with the total fault length. You have shown 2 short several km segments, and interpret a longer fault, but this longer fault doesn't match the full length of the bedrock fault shown on Figure 2. Why?

Reply: We are simply reporting the results as we have them, and as are possible with the existing lidar data. We can speculate that the active BRF may take advantage of preexisting planes of weakness along the inherited bedrock structure, but it is not reactivating the bedrock fault, so we would not necessarily expect the full lengths to match.

Line 469-471. This hasn't really been shown. I am not sure what you are arguing here. Which magnitudes and lengths? The uncertainty bars on these scaling relationships are very large.

Reply: We have added details per Reviewer 1's suggestions: *"The magnitudes of displacement (∼1-3 m per event) and total fault lengths (>40 km)..."*

Line 483. The paper hasn't demonstrated any convincing RL offsets so far.

Reply: We disagree, as discussed above.

Line 491. These are really important and required assumptions if the kinematic interpretations are true, but they are a bit unsatisfying. I do not think the differential erosion model is reasonable, but then why are there no longer term displacement features along this fault? It seems unlikely that this is a brand new fault with no pre-glacial expression.

Reply: We do not argue that this is a brand-new fault. However, the lack of preserved deposits and landforms that predate this most recent glaciation precludes us from making any statements about its activity prior to glaciation. We believe it is highly probable that there are no longer- term displacement features along this fault because there was a continental ice sheet modifying the landscape.

Line 500. This is a bit strange too. Why is the fault in such a close proximity at the surface but has a completely different subsurface geometry? That doesn't make a ton of sense to me.

Reply: This is not uncommon in this study area, with recent examples being the Leech River fault (e.g., Harrichhausen et al., 2021; Li et al., 2018a; Morell et al., 2018) and the North Olympic fault zone (e.g., Schermer et al., 2021).

Line 503-504. but these ranges overlap, so it could be the same?

Reply: We do not think that 1° of overlap (45°-70° vs 70°-90°) is significant.

Line 515-516. the multiple event evidence could be shown more clearly with a figure showing a site with clear multiple generations of scarps & progressive geomorphic features. Maybe include the hillshade maps along with Fig 10 to show these sites on a single figure.

Reply: We have edited Figure 9 to show mapped units in addition to different geomorphic figures. We have also added the geologic mapping to Figures 5 and 6, per Reviewer 1's suggestion.

Line 549-550. but glacial unloading of the valley bottom would be consistent with that popping up? The fault expression seems primarily to be vertical compared to strike-slip.

Reply: Extensional faults have a vertical σ_1. While the BRF is not purely extensional, it does have a large component of extension, as pointed out by the reviewer. As stated in the paper, a reduction in vertical stress (σ_1) reduces the deviatoric stress (σ_1 - σ_3), making failure less likely. This is a simplification, but a more detailed treatment of the precise principal stress axes of the BRF and the timescales of vertical and horizontal stress changes from glacial isostatic adjustment would be the subject of another paper. Additionally, we point out that the continued ruptures of the BRF into the late Holocene is inconsistent with the timing expected from GIA-induced seismicity.

Line 581. It would be good to separate the oblique(net) from the horizontal and vertical components here.

Reply: Thank you for the suggestion. We have added the horizontal and vertical components to Sections 4.3.1 and 5.5 of the paper.

Line 612. state how much slip

Reply: We have added "~1-2.5 m" to the sentence.

Line 615. slip at the surface or at depth?

Reply: We have added "at 0-5 km depth" to the sentence.

Section 5.6. a figure showing the kinematic significance of this fault would be helpful.

Reply: In this section we are speculating on some possible hypotheses but a robust analysis of this is outside of the scope of this paper.

Figures

Fig 1. Star I presume is the 1946 earthquake? Label it on the fig. I'm pretty sure I have seen more active faults than you have plotted in BC – but maybe I am wrong here.

Reply: The only documented active fault in BC not already on Figure 1 is the X̲EOLX̲ELEK Elk Lake fault (Harrichhausen et al., 2023), which was published after this paper was submitted. We have added it to Figure 1 and labeled the star. We note there are very few active faults identified in BC to date (in the Introduction and geologic background), hence adding to the significance of the findings in this paper.

Fig 2. You have thrust teeth on the BRF but state in text it is transtensional.

Reply: Figure 2 shows bedrock faulting in grey and identifies the bedrock Eocene thrust fault of the Cowichan fold and thrust system. The dotted symbol of the thrust fault is to indicate that it is buried under Quaternary deposits and does not offset them (i.e. has not had offset in the Quaternary). The active BRF is transtensional, and is shown by the red and orange lines in Figure 2b.

Is it possible to keep N up in Fig 2? It doesn't seem like it saves you a ton of space to rotate it.

Reply: North is rotated $\sim$40° in Figure 2, making it possible to show both panels on one page.

A. Some strike & dips, and other structural data, if available, would be useful to show on the map.

Reply: We have added some strikes & dips to Figure 2.

B. Do you have an easy explanation for the difference between the two faults? I am wondering if the bedrock trace is just less precise than your more modern trace, and the disagreement is just a result of the digitization of the bedrock map. In this case I would suggest to combine the traces. If the difference is real on the ground, there should be some discussion of this.

Reply: The difference is real on the ground and we have confirmed the two faults' positions in the field. Figure 4 shows our updated precise mapping of the bedrock trace from this study (solid teeth) relative to the active fault. This is not just a 'poor digitization' of the bedrock fault from existing maps. We discuss the location of the active fault relative to the bedrock fault in section 5.2.

I also think you should include your detailed fault mapping on here. You can use a thinner line weight than the dashed red currently shown. I don't think the reader should have to refer to the supplement to find this. (in hindsight I see this is on Fig 4 – maybe refer to that in this caption).

Reply: Thank you for the suggestion. We have updated Figure 2 to show the detailed mapping. We have also added a reference to Figure 4 in the caption.

A comparison between the dashed line in Fig 2 and the very limited extent of Q-scarps on the detailed fault map in the supplement is concerning as the dashed line leads the reader to expect the fault is well expressed along this entire length. What makes you interpret the other parts of the dashed line as 'the active trace' as indicated by the symbology on Fig 2? This should be mentioned in the text-¿ maybe something like "the fault is only well expressed in Quaternary deposits in 2 very short (several km max?) locations.

Reply: We have now edited this figure to no longer show an interpolated extent of the active faults but the actual extent mapped (as in the supplement). The detailed mapping we have added to Figure 2 shows that the Quaternary scarps, while discontinuous along strike, do extend throughout this entire length. It is not uncommon for strike-slip faults to have discontinuous expression, particularly in an area with mostly young (Holocene) deposits, and an area that experiences extensive hillslope erosion.

Figure 3. Indicate in caption which figure the locations are on (Fig 2b). Indicate hillshade illumination angle either on figures or in the caption.

Reply: Thank you for the suggestion. We have added a reference to locations on Figure 2b in the caption. We have added the hillshade azimuth and altitude to the caption.

3a. What is the downhill facing scarp just above the 'Fig 3c' label? It appears to follow the 300m elevation contour. Together with the uphill facing scarp it almost gives the appearance of a graben, which could be consistent with a shallow sliding block type slope failure. Alterately, is it possible that this uphill scarp is the margin of a moraine pasted on the mountain slope? The texture of the slope below this uphill scarp is much different than that above, suggesting it could be compositionally different. It is clearly more erodible than the uphill unit.

Reply: The downhill-facing lineament on Figure 3a is a small bedrock escarpment. The slope below this escarpment is a different material - paraglacial debris cones as opposed to the till over bedrock upslope (see Figure 4). The uphill facing scarps in figure 3c are not moraines because they right-laterally offset post-glacial channels, and they are not composed of till.

3b. I think I could draw those channels at a slightly different angle and there wouldn't be a clear right-lateral displacement. They only need to be rotated 5 degrees for this RL offset to disappear. Adding contours to this plot would help justify your placement of thalweg & interfluve lines.

Reply: Thank you for the suggestion. We have added 1 m contours to this figure. We also add that we have confirmed the right lateral displacements at each site by visual inspection in the field, and by the survey data of >50 laterally offset piercing lines presented in this paper.

Also – curious it is transtensional yet has uphill facing scarps? Very unusual geometry for a fault presumably dipping into the mountain range. I would expect this is pure strike-slip given the overall morphology here. But if it is strike-slip, where are the lateral offsets?

Reply: We are confused about the comment "where are the lateral offsets," because we measure lateral offsets as reported in Section 4.3.1 and Figure 9. Moreover, such a fault expression is not uncommon for oblique faults; see our response to the comment on uphill-facing scarps above.

How high are these scarps? Some profiles of representative scarps might be useful.

Reply: We have added scarp height to the caption. A representative profile is already shown in Figure 8.

Figure 4. I suggest to rotate these so that N is up and just make each one a full page figure.

Reply: The 40° rotation is consistent across all of our lidar & mapping figures so that we can better fit the ∼310° striking fault. We would prefer to keep this consistent so as not to confuse the reader.

Do you have a photo showing the Qt – till mantling bedrock? Some of your interpretations hinge on this and it isn't well established so far despite being a very widespread unit.

Reply: We have added an annotated photomosaic of till over a bedrock shear zone in the new supplemental Figure S5.

Indicate ages next to sample names on Fig 4.

Reply: We have added the ages to panel a.

Fig 8. What do you use to justify the different dips and vertical senses of motion indicated here? All of the scarps dip the same way, so I might expect that they have similar dips & senses of motion.

Reply: The dips and senses of motion in Figure 8b are from our surveys. We have added this information to the caption.

8c caption. angle of repose? of loose sand? I am not sure this is relevant, so just suggest to delete.

Reply: We have deleted this phrase.

Figure 9. Please separate vertical from horizontal measurements on separate plots prior to combining them.

Reply: We have revised Figure 9 to include separate panels for vertical and lateral displacements.

Supplemental

Figure S1. Scale bar is missing from the supplement map

Reply: Thank you for pointing this out. We have added the scale bar to Figure S1.

Fig S2: E. I am not sure I would call this a sag pond. It looks more like a shutter ridge that has created a dam. A sag pond is more like a negative flower structure and I don't see that in the gross morphology. Alternately it looks like it could be an eroded bedding plane.

Reply: First, there is no channel to dam at this site, and so the feature cannot be classified as a shutter ridge. Second, we discuss in Supporting Information Text S1 how we ruled out bedrock erosion. We also note that the bedrock unit here is basalt, and therefore this feature cannot be an eroded bedding plane. Furthermore, flow tops in basalt are gently dipping here, not steeply dipping, as would be needed for this feature to be related to differential erosion.

Figure S3. What figure shows these locations?

Reply: Figure 4 shows the locations of the dated samples. We have also added a reference to Figure 4 in the Figure S3 caption.

Need to add the unit abbreviations to the caption.

Reply: We have added unit abbreviations to the caption.

Indicate the ages along with the sample names on the photos.

Reply: We have added the ages of dated samples to the photos.

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