Review report

Patton et al., 20th to 21st Century Relative Sea and Land Level Changes in Northern California: Tectonic Land Level Changes and their Contribution to Sea-Level Rise, Humboldt Bay Region, Northern California, TEKTONIKA, 2023.

This document contains:

1/ Pages 2-8: 1st round decision letters by the executive and associate editors, followed by the 2 reviews. Decision sent to authors was "revise".

2/ Pages 9-18: The detailed author's answer to these letters and reviews.

3/ Pages 19-21: The second decision letter based on the revised manuscript and the above author's answer, followed by final author's answer. Decision was "minor technical revision before acceptance." Editor's final acceptance letter follows.

1/ ——————————————————

1st round decision letters by the executive and associate editors, followed by the 2 reviews.

Dear Jason,

We have received two reviews regarding your paper submitted to Tektonika. They are both positive but ask for some revision and clarification (in particular on the figures) before the manuscript becomes acceptable for publication. You will find below these two reviews as well as the recommendation by the associated editor, Jack Williams.

One of the reviewers pointed out to us, editors, that two "unpublished" non peer reviewed reports already presented some of the results. Please take into consideration the associate editor's recommendation about this issue.

I also note that you haven't specifically stated how and where the data used in the paper are accessible. Please add a section "Data availability" at the end of the text. See point 1.7 in тектonіка code of conduct (https://tektonika.online/index.php/home/code_of_conduct)

Your figures and tables are presently raster images embedded in the manuscript. In addition to the complete manuscript file, I suggest you submit a separate PDF file for each figure and table, ideally with vector figures and text tables, not raster. This will help for the production of the final paper.

Submit your revised manuscript via тектonіка web site: under your manuscript's record you'll find a box named "revisions" with a way to upload your new files. Please also submit a detailed rebuttal letter explaining how you took into account reviewer's and editorial recommendations, and an additional manuscript version with the changes outlined.

Waiting for the revised version of your paper. Thanks for submitting to тектonіка.

Best regards

Robin Lacassin (тектonіка editor)

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Associate Editor recommendation:

We have now received two reviews regarding the submission to тектonіка, "20th to 21st Century Relative Sea and Land Level Changes in Northern California: Tectonic Land Level Changes and their Contribution to Sea-Level Rise, Humboldt Bay Region, Northern California."

Based on these considerations, my recommendation is that revisions are requested prior to being accepted by тектonіка. In doing so, I note both reviewers agree that this study presents a novel and robust dataset that suggest deformation associated with the Cascadia Subduction Zone and upper crustal faults can help explain ‘anomalous’ sea-level records in northern California. Hence, it will make an interesting and worthy contribution to тектonіка.

When making their revisions, the authors should pay particularly attention to making the figures more legible and incorporating previous published work on this region.

Please also note that one of the reviewers noted that some these results has been previously presented in the following reports:

Patton, J., Williams, T., Anderson, J., & Leroy, T. (2017). Tectonic land level changes and their contribution to sea-level rise, Humboldt Bay region, Northern California. https://digitalcommons.humboldt.edu/cgi/viewcontent.cgi?article=1023&context=hsuslri_local

Patt on, J. R., Williams, T. B., Anderson, J., Burgette, R., & Leroy, T. (2014). Tectonic land level changes and their contribution to sea-level rise, Humboldt Bay region, Northern California: 2014 status update. Prepared for US Fish and Wildlife Service Coastal Program. Cascadia GeoSciences, McKinleyville, CA. http://www.hbv.cascadiageo.org/HumBayVert/reports/USFWS/20160322/pdf-final/status_report_CG_HBV_USFWS_fall_2014_small_20141231.pdf

I do not see this as a problem per se for тектonіка, and ask only that the authors: (1) confirm these are reports, and not previously peer-reviewed scientific articles, and (2) reference these articles in this manuscript, and demonstrate how this new work builds on, and strengthens the previous studies.

I look forward to seeing the revised version of this paper

Jack Williams (тектonіка associate editor)

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Reviewer 1 Comments

For author and editor

In this article, Patton et al propose to explore potential reasons for an apparent discrepancy in relative sea level measurements in northern California. Sea level measurements from tide gauges are not all consistent along the coast and, by comparing vertical motion derived from leveling data, GNSS data and tide gauge data (relative sea level measurements corrected from various sources), they conclude that the variability observed in measurements is most likely of tectonic origin and relate it to the deformation associated with the Cascadia subduction zone. The outcome of the paper is a very nice dataset of vertical motion in the region.

In general, the paper is nicely written and although some of the figures could be clarified, they convey adequately the message of the paper. I haven't found significant flaws in the method description and the results are nice. I have a few suggestions to improve the manuscript and, in particular, the scope of the paper. I think these comments could be addressed pending minor to moderate revisions.

Main comments:

The objective of the paper does not appear so clearly from the introduction. Maybe I didn't get it right, but it seems that the initial motivation was to understand the cause of the discrepancy between measurements at North Spit compared to further north and that the study concludes that, actually, there is a simple explanation (i.e. the subduction zone). So the objective of the paper is to explain this discrepancy or to provide a unified dataset? or both? or is the unified dataset a consequence of the study. This should be clarified in the introduction. Maybe a simple sentence stating "we show that, with a collection of data over the whole region, these measurements are consistent and explain why" would do the job.

There is no explanation regarding how the different datasets are put in a same reference frame (or I missed it, which would mean it is unclear). Especially, the motion derived from leveling is relative while GNSS provides something attached to ITRF08. Is this relative motion solved using only the motion of monument #60 as written in the text? How about error propagation in the leveling measurements (errors should increase away from that point)? I think there should be a standalone paragraph on that specific topic.

Map on figure 4 is really cool! Maybe increase the size of markers on the inset focusing on the Humbolt bay.

Figure 5 does not show nicely what figure 4 shows brilliantly. I would suggest to modify two points. First, the markers should be bigger, with thicker error bars and colored to ease their identification. Second, I am not certain the quantity that actually matters is longitude, but rather distance to the trench. Given the latitudinal spread of the data and the twisted coastline, the pattern of deformation as a function of distance to the trench might highlight a simple elastic deformation pattern, to first order, especially since the subduction is locked offshore there.

In the discussion section, I would encourage the authors to derive a simple elastic dislocation model to identify, if this signal were the signature of locking of the megathrust, at what depth one would expect to find the transition from a locked to aseismic megathrust. Equations can be found in Segall's book on earthquake and volcano deformation.

One very important point that the authors overlook somehow is the consistency between datasets acquired over different time windows. GNSS data were acquired over the last decades, tide gauge data and leveling data were acquired over the last century and they are consistent with each other. There is very few places in the world where we have records of deformation during the interseismic period in subduction zones over such long periods of time and the fact that, to first order, there is no variation in surface deformation rates is remarkable. This point should be emphasized and discussed. For instance, some visco-elastic models of the earthquake cycle suggest that interseismic strain might change gradually over time, which is not what we see here.

Minor comments:

Line 55: Is there uncertainties associated with the global estimate?

What motivated the choice of the green background of Figure 1? I have to say that it is almost painful.

Figure 2 is a nice beautiful map but it is slightly difficult to read. Grey symbols over a grey background is not an ideal choice. Some labels are really small and might not be needed (hence they look heavy on the figure). All faults do not necessarily need to be labeled. We probably do not need roads (all readers of Tektonika might not be used to the geography of the US highway system). If you want to keep the road, the color of highways should be consistent between the legend and the map and similar on both maps. Maybe adding some color would help identifying which marker is which type of data.

In overall, I think it is a very nice paper and I hope my comments will help improving this (already very interesting) manuscript. I haven't checked for typos and style issues but I am willing to in a next round of reviews if need be.

Romain Jolivet, PhD

Ecole normale supérieure, Paris, France

Reviewer 2 Comments

For author and editor

SUMMARY OF THE PAPER

The manuscript by Patton et al. presents a synthetic analysis of available tide gage, leveling and GNSS data along the coast of northern California. The area is affected by a number of natural and man-induced processes that make it vulnerable to coastal subsidence. In particular, the interference of sea-level rise and tectonic processes leads to a complex pattern of coastal subsidence in the Humboldt Bay area, the primary focus of this study. Characterizing this subsidence requires using complementary geodetic techniques, which is the approach taken by Patton et al.

Using long datasets gathered at a few tide gage sites, the authors estimate the relative sea level (RSL) rate, and access the local vertical land motion (VLM) rate by subtracting previously published velocities of absolute sea level rise. They also subtract the glacial isostatic adjustment (GIA) to remove the contribution of continental-scale processes (>1000km), so as to be able to focus on land motion of local-to-regional origin, which are caused mainly by tectonic processes. Less-well constrained tide gages are used to assess the relative sea level by differencing their record against those of better-constrained tide gages, which gives access to their own VLM rate. Independent analysis of leveling acquired along coastal roads brings redundancy, which allows for confirming that spatial variations along the coast of VLM rates are robust and consistent within the dataset. Finally, the estimated VLM rates are complemented by GNSS-derived estimations extracted from USGS public databases.

After processing these data, the authors argue that the dominant source of RSL, in addition to the global (nearly uniform) sea level rise, are (a) at broad scale, interseismic loading of the Cascadia megathrust and (b) locally, interseismic loading of crustal (shallow) faults, in particular the Little Salmon fault.

GENERAL COMMENTS

The paper is generally well written, and the methodology is sound and well explained. The consistency between the various datasets is remarkable, and clearly demonstrates the existence of a coastal subsidence pattern in Northern California peaking in the area of the Humboldt Bay, and likely of tectonic origin.

On the downside, the figures could be much improved, as I struggled to find the location of most stations referenced in the text and in the tables. I provide a few suggestions below.

More importantly, the references provided in the text are somehow outdated, and an important paper published in 2018, that also estimates the rate of subsidence in the area using tide gages and GNSS, is not cited in the present manuscript (Montillet, Melbourne and Szeliga, 2018, JGR Ocean, "GPS Vertical Land Motion Corrections to Sea-Level Rise Estimates in the Pacific Northwest"). This paper reports estimates of VLM rates using part of the dataset used by Patton et al., which may (or may not) be in agreement with the estimates of Patton et al. A more recent paper from the same group appears to include updated estimates (He, Montillet et al., 2022, Remote Sensing, MDPI). A comparison with these previous works, and generally speaking, a more thorough review of previous attempts to constrain VLM in the area, as well as interseismic strain accumulation, and a refreshing of the figures, would certainly improve the present manuscript.

Finally, although the estimated VLM rates reported in this manuscript (from tide gages, leveling and GNSS) form a generally coherent result, one specific treatment of the results would deserve a more careful analysis. Namely, the interpretation of a present-day accumulation of interseismic deformation at the location of the Little Salmon fault (LSF) may not be fully convincing to the reader (please see Figure 6). Indeed, the apparent "step" of ~ 2 mm/yr at the location of the LSF would require that the fault should slip interseismically up to the surface ("creep"), or at least very close to the surface. However, the authors do not mention this straightforward interpretation, and neither do they discuss its implications nor its possible consistency with independent evidence.

Furthermore, a number of stations along the "North Spit", exhibiting a behavior departing from the simple "step" pattern across the Little Salmon fault, are ignored in the analysis. In section 2.2.2, the authors mention these discarded points: "We note some leveling data that are from sites on the North Spit and plot these as white circles because they are also to the west of the highway 101 data (they show a larger negative rate).". Further justification would be required to convince the reader that these points should indeed be considered as outliers, even if their uncertainty (as far as

I can judge from the supplementary material) is similar to that of other points that have been considered as valid for the interpretation. Considering all the datapoints together (valid or "outliers"), one may reasonably wonder whether the local subsidence pattern reported by Patton et al. may be actually enhanced by a local sediment compaction process, in addition to the tectonic process (whether due to a shallow fault or interseismic loading of the megathrust). Even if the authors do mention these non-tectonic processes, the justification brought by the authors to discard them as negligible warrants further justification.

Overall, this manuscript brings a number of important conclusions, that could be better supported by a more careful discussion of the assumptions, improved figures and a more thorough discussion of its implications in the context of pre-existing state of knowledge.

Raphael Grandin

LINE-BY-LINE COMMENTS

line 26: missing point (typo).

line 41: please replace "This" by "Present-day", to better distinguish deglaciation after the LGM from today's accelerated ice melting.

line 49: "Pacific Northwest": please define for non-US readers.

line 49: sea level rise is estimated to 2.28 ± 0.03 mm/yr from a study of 2009, but other studies estimate different rates. For instance, Montillet et al. (2018) report a sea level rise at 1.99 mm/yr, which is well outside the confidence interval used here. A short discussion about the differences between published estimates could be useful to better constrain the uncertainties.

line 57: it took me some time to spot the location of "Crescent city" in Figure 1. This figure needs to be greatly improved to help the reader to quickly locate the area of interest. Presently, it is a bit cluttered and text fonts are generally too small.

line 60: the references reported here are rather old (1994 and 2006). More importantly, this does not look like a fair depiction of the existing state of knowledge regarding the unusually large RSL rate tide gage recorded at Humboldt Bay tide gage. For instance, Komar, Allan and Ruggiero (2011, Journal of Coastal Research) mention explicitly that this tide gage is affected by tectonic motion.

Figure 1: the area of interest of this study is too small in this figure, or at least should be identified by a rectangle, using the limits in Figure 2. Also, the green layout is questionable. Finally, the plate kinematics boundary conditions in the vicinity of the triple junction, and the interseismic velocity field as deduced from GNSS could be displayed here, to facilitate the understanding of the local tectonic regime. Please see for instance the recent paper by McKenzie & Furlong (2021, Geoscience Letters).

Figure 2: it is hard to find the relevant information on this figure. The stong contrast of the shaded topography in the background makes it difficult to distinguish the symbols. Also, the most important stations should be highlighted with a bigger symbol, or using another strategy. Currently, all fonts have the same size, so it is hard to identify the important information, for instance to quickly spot the Crescent city and North Spit stations, which are at the core of the study. Extent of the map in the inset should be drawn on the main map using a rectangle (just as is done in the "inset of the inset"... which by the way may be unnecessary). Tick marks for latitudes / longitudes should be tighter (every 0.25° or 0.1° if non-annotated tickmarks are used) and the fonts should be enlarged. In the inset, there is ample space for writing the name of the main stations in full, instead of abbreviations that require the reader to read the caption.

line 122: when mentioning GIA corrections, the authors could provide here a rough estimate of the magnitude of the correction (the reader has to wait until line 373 to learn that the subsidence rate is ~ 1.4 mm/yr), and, more importantly, how it was derived.

line 176: "known earthquakes": which ones (time, location, magnitude, depth), and how much static displacement is removed (and on which stations)?

line 182: the Englehart et al. (2016) referenced in the text does not exist in the references. Instead, there is a Dura, Englehart et al. (2016) reference, and then an Englehart et al. (2015) reference. Which one is correct? Assuming that the authors mean Englehart et al. (2015), instead of Englehart et al. (2016), it remains unclear what value(s) the authors have used to correct for GIA in this study. This should be explicitly mentioned in section 1.1.4.

line 193: it seems that there is a confusion in the values reported in the text for Crescent City. Instead of a RSL rate of -0.97 mm/yr at CC (Table 1), the authors seems to have used a value of -0.83 mm/yr (Table 2) to come up with a VLM rate of 3.11 mm/yr after subtracting an eustatic sea level rate of 2.28 mm/yr.

line 195: again, there is a confusion in the numbers reported in the text and in the tables for North Spit, but this time I cannot figure out where the mistake is. Indeed, 5.58 mm/yr (relative VLM rate of NS minus CC, as in Table 1) minus 3.11 (VLM for CC) should yield 2.47 mm/yr, instead of 2.69 mm/yr reported in the text and in Table 2.

Figure 3: the panels G, H, I and J start in 1975, but they could be made to start in 1930, just like panels A through F, to facilitate comparison and visual correlation.

line 258: missing point (typo).

Figure 4: the colouring scheme used in this figure makes it hard to distinguish the uplift rates, due to (a) exceedingly strong contrast of the shaded relief background and (b) colouring of the faults and roads.

Figure 5: the font sizes used in this figure are too small. It is really hard to distinguish the most important benchmarks, which could be highlighted by a bigger symbol. The upper panel showing vertical land motion versus longitude is not easy to interpret because the direction of projection is not perpendicular to the Cascadia subduction, as explained in the text. The apparent complexity is partly due to this inadequate projection, which makes it hard for the reader to disentangle the different processes at play in the area.

line 296: "handing" should be replaced by "hanging" (typo).

line 302: if the velocity difference across the Little Salmon fault shows up as a step, then this strong velocity gradient implies that the fault is creeping at the surface, or very close to the surface (assuming that the step was not caused by one or several earthquakes, which is implicit since the authors estimate a slip rate). Could the authors comment on this obvious conclusion? Does this fit with independent observations?

Figure 6: four stations located on the North Spit, with subsidence rates clustering around -2.75 mm/yr, have been ignored in the map, as well as in the interpretation in the lower panel (the dots are left with a white fill, implying that they are not relevant). Is there a specific reason for excluding these measurement points? Also, the lower panel displays the vertical velocity as a function of northing, whereas the Little Salmon fault has a northerly (~ N150°E) azimuth. Rotating the projection, or, at least, using the easting instead of the northing, would be more appropriate to highlight the proposed velocity step associated with the fault.

line 324: "plate tectonic factors" is too vague, as it may exclude the existence of shallow crustal faults (which, accordingly, are a product of plate tectonics). Indeed, "plate tectonic factors" may be incorrectly understood as "processes taking place at plate boundaries", i.e. not intraplate deformation. I suggest using the more generic term "tectonic motion", instead of "plate tectonic factors".

line 333: the word "lithospheric" may be confusing and could be removed in this sentence, since crustal faults are evoked later in the sentence. Shallow crustal faults do deform the whole lithosphere, but their footprint is mostly restricted to the upper crust (contrary to the megathrust, which indeed produces deformation at the scale of the lithosphere).

lines 384 and 385: the wording "evidence" is unnecessarily repeated in this sentence. Please reword.

line 417: the East-West trend in VLM rates could be better highlighted if the symbols corresponding to the Humboldt bay transect were distinguished in Figure 5A.

line 424: the authors argue that the different VLM rates at Crescent City versus Humboldt bay area can be mainly explained by a variable distance with respect to the trench. However, the two locations are at the same longitude, whereas the trench is oblique, which makes the Figure 5A confusing. Reprojecting the whole figure perpendicular to the trench would certainly help the reader here.

line 443: "We note some leveling data that are from sites on the North Spit and plot these as white circles because they are also to the west of the highway 101 data (they show a larger negative rate).". More justification is needed here. It is clear that the authors have excluded some stations, and we can see which ones, but the choice seems arbitrary.

line 459: "trends (...) previously thought to be anomalous". This assertion demands further justification. In what sense is the trend considered "anomalous"?

Table S-1 title: do you mean "S-1", instead of "S11"?

Table S-1 caption: "1988-1968" should be "1988-1967" according to the text (section 1.1.2.)

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Authors' answer to 1st round of reviews

Color Legend:

Responses in Blue

Incomplete Tasks in Red

Review direction in Green

We have received two reviews regarding your paper submitted to Tektonika. They are both positive but ask for some revision and clarification (in particular on the figures) before the manuscript becomes acceptable for publication. You will find below these two reviews as well as the recommendation by the associated editor, Jack Williams.

One of the reviewers pointed out to us, editors, that two "unpublished" non peer reviewed reports already presented some of the results. Please take into consideration the associate editor's recommendation about this issue.

I also note that you haven't specifically stated how and where the data used in the paper are accessible. Please add a section "Data availability" at the end of the text. See point 1.7 in τeκτonικa code of conduct (https://tektonika.online/index.php/home/code_of_conduct)

We will place these data on Zenodo. Most of these data are presented in supplemental files already.

Your figures and tables are presently raster images embedded in the manuscript. In addition to the complete manuscript file, I suggest you submit a separate PDF file for each figure and table, ideally with vector figures and text tables, not raster. This will help for the production of the final paper.

I suggest that you not request that papers be submitted with figures embedded in the text. I would prefer to submit (1) a file with only the text, (2) a zip file with all figures, tables, appendices, etc., and (3) a pdf with full page layout. Embedding figures in word destroys the quality of those figures. The reviewers had difficulty reviewing our manuscript because of this problem. Also, they did not understand this.

When making their revisions, the authors should pay particularly attention to making the figures more legible (they would have been legible if we were not asked to embed them in a word document) and incorporating previous published work on this region.

Please also note that one of the reviewers noted that some these results has been previously presented in the following reports:

Patton, J., Williams, T., Anderson, J., & Leroy, T. (2017). Tectonic land level changes and their contribution to sea-level rise, Humboldt Bay region, Northern California. https://digitalcommons.humboldt.edu/cgi/viewcontent.cgi?article=1023&context=hsuslri_local

Patton, J. R., Williams, T. B., Anderson, J., Burgette, R., & Leroy, T. (2014). Tectonic land level changes and their contribution to sea-level rise, Humboldt Bay region, Northern California: 2014 status update. Prepared for US Fish and Wildlife Service Coastal Program. Cascadia GeoSciences, McKinleyville, CA. http://www.hbv.cascadiageo.org/HumBayVert/reports/USFWS/20160322/pdf-final/status_report_CG_HBV_USFWS_fall_2014_small_20141231.pdf

I do not see this as a problem per se for τeκτonικa, and ask only that the authors: (1) confirm these are reports, and not previously peer-reviewed scientific articles, and (2) reference these articles in this manuscript, and demonstrate how this new work builds on, and strengthens the previous studies.

We added these non-peer reviewed reports as referenced and mentioned why this paper is an improvement over these gray literature reports.

Reviewer 1: Romain Jolivet

The objective of the paper does not appear so clearly from the introduction. Maybe I didn't get it right, but it seems that the initial motivation was to understand the cause of the discrepancy between measurements at North Spit compared to further north and that the study concludes that, actually, there is a simple explanation (i.e. the subduction zone). So the objective of the paper is to explain this discrepancy or to provide a unified dataset? or both? or is the unified dataset a consequence of the study. This should be clarified in the introduction. Maybe a simple sentence stating "we show that, with a collection of data over the whole region, these measurements are consistent and explain why" would do the job.

The impetus for the study was developed during a stakeholder workshop held in 2010 where we identified evidence for ongoing subsidence in and around Humboldt Bay. Geologists at the workshop hypothesized that this subsidence was due to Cascadia tectonics, which was unclear to some at the time of the workshop. The goal of the research project was to further document these observations using additional tide gage data and supplementing these observations with independent geodetic data including GPS data and benchmark level survey data. The objective for the paper is to present a unified dataset and to interpret the cause of the findings included within the dataset. We added a few sentences into the introduction to address this.

There is no explanation regarding how the different datasets are put in a same reference frame (or I missed it, which would mean it is unclear). Especially, the motion derived from leveling is relative while GNSS provides something attached to ITRF08. Is this relative motion solved using only the motion of monument #60 as written in the text? How about error propagation in the leveling measurements (errors should increase away from that point)? I think there should be a standalone paragraph on that specific topic.

The leveling errors are calculated using the method from Burgette et al. (2009; Reed did the analysis for our paper as well). They do not increase away from the point (I believe this is because surveys are composed of multiple loops and each loop (a loop is between each survey point) has its own uncertainty; at least that is what we discussed when we were conducting the surveys to the tide gages). The VLM rates are calculated between each monument. We wrote a new section about reference frames.

Map on figure 4 is really cool! Maybe increase the size of markers on the inset focusing on the Humboldt bay.

Figure 4

We increased the marker sizes for the inset map. Great idea!

Figure 5 does not show nicely what figure 4 shows brilliantly. I would suggest modifying two points. First, the markers should be bigger (this may be distracting), with thicker error bars and colored to ease their identification. Second, I am not certain the quantity that actually matters is longitude, but rather distance to the trench. Given the latitudinal spread of the data and the twisted coastline, the pattern of deformation as a function of distance to the trench might highlight a simple elastic deformation pattern, to first order, especially since the subduction is locked offshore there.

Plotting relative to the trench only works for the geodetic sites north of the triple junction. South of the triple junction, this information will exert a strange influence on the plot.

We did not enlarge the markers because this will make the figure difficult to interpret. This figure was destroyed by placing it into ms word document. Many of the comments about the figures are likely because they did not look like they were supposed to, being in the word document.

We did not thicken error bars and color them because one could not see the error bars as the figure was embedded in a word document, which destroyed the figure.

We replotted plotted the latitude plot data, now relative to distance to the trench. We wanted to do this but were pressed for time. So we are glad to have placed this in the trench distance reference frame.

In the discussion section, I would encourage the authors to derive a simple elastic dislocation model to identify, if this signal were the signature of locking of the megathrust, at what depth one would expect to find the transition from a locked to aseismic megathrust. Equations can be found in Segall's book on earthquake and volcano deformation.

We are conducting a study to evaluate the locking due to the megathrust and crustal faults in a separate paper led by Kathryn Materna, a post doc at the USGS. It seems like doing this work with assuming solely elastic processes is a waste of time, especially due to the influence of viscous properties of the mantle. It seems like using coulomb would be a much smarter use of our time. Please stay tuned for a paper from Kathryn Materna (USGS) with Jason R. Patton as a coauthor. They are anticipating publication of this paper so that they can submit their paper. Though we do mention others who have done this elastic modeling as a test case for this phenomenon (e.g., Hyndman and Wang, 1995, Wang et al., 2003; Newton et al., 2021).

One very important point that the authors overlook somehow is the consistency between datasets acquired over different time windows. GNSS data were acquired over the last decades, tide gauge data and leveling data were acquired over the last century and they are consistent with each other. There is very few places in the world where we have records of deformation during the interseismic period in subduction zones over such long periods of time and the fact that, to first order, there is no variation in surface deformation rates is remarkable. This point should be emphasized and discussed. For instance, some visco-elastic models of the earthquake cycle suggest that interseismic strain might change gradually over time, which is not what we see here.

We emphasize this more than we already do. Though we here, in this response, suggest that the time span between 1967-1988 and ~2000 to 2022 may not be a sufficiently different part of the earthquake cycle. We would not be able to evaluate this until there is a CSZ earthquake. We added a few sentences in the conclusion about this.

Minor comments:

Line 55: Is there uncertainties associated with the global estimate?

We added the uncertainty to line 55: 3.4 ± 0.04 mm/year. Thank you!

What motivated the choice of the green background of Figure 1? I have to say that it is almost painful.

We chose green because this figure has been used dozens of times and grayscale is a boring way to present this figure yet again. However, we prepared another exciting map of the subduction zone!

Figure 2 is a nice beautiful map but it is slightly difficult to read. Grey symbols over a grey background is not an ideal choice. Some labels are really small and might not be needed (hence they look heavy on the figure). All faults do not necessarily need to be labeled. We probably do not need roads (all readers of Tektonika might not be used to the geography of the US highway system). If you want to keep the road, the color of highways should be consistent between the legend and the map and similar on both maps. Maybe adding some color would help identifying which marker is which type of data.

Now figure 3. We chose gray letters as labels to preserve graphical hierarchy. Black labels would make these labels too high in hierarchy. We used darker gray for the GNSS site labels. We changed the font for Crescent City to black text.

The road symbols are important because it helps one identify what road the benchmark is associated with. The most important road label is highway 101. We made the road symbol (color) consistent between figures.

We removed the fault names and added an additional figure (figure 2) that has the faults that includes the fault names.

Reviewer 2: Raphael Grandin

The manuscript by Patton et al. presents a synthetic analysis of available tide gage, leveling and GNSS data along the coast of northern California. The area is affected by a number of natural and man-induced processes that make it vulnerable to coastal subsidence. In particular, the interference of sea-level rise and tectonic processes leads to a complex pattern of coastal subsidence in the Humboldt Bay area, the primary focus of this study. Characterizing this subsidence requires using complementary geodetic techniques, which is the approach taken by Patton et al.

Using long datasets gathered at a few tide gage sites, the authors estimate the relative sea level (RSL) rate, and access the local vertical land motion (VLM) rate by subtracting previously published velocities of absolute sea level rise. They also subtract the glacial isostatic adjustment (GIA) to remove the contribution of continental-scale processes (>1000km), so as to be able to focus on land motion of local-to-regional origin, which are caused mainly by tectonic processes. Less-well constrained tide gages are used to assess the relative sea level by differencing their record against those of better-constrained tide gages, which gives access to their own VLM rate. Independent analysis of leveling acquired along coastal roads brings redundancy, which allows for confirming that spatial variations along the coast of VLM rates are robust and consistent within the dataset. Finally, the estimated VLM rates are complemented by GNSS-derived estimations extracted from USGS public databases.

After processing these data, the authors argue that the dominant source of RSL, in addition to the global (nearly uniform) sea level rise, are (a) at broad scale, interseismic loading of the Cascadia megathrust and (b) locally, interseismic loading of crustal (shallow) faults, in particular the Little Salmon fault.

GENERAL COMMENTS

The paper is generally well written, and the methodology is sound and well explained. The consistency between the various datasets is remarkable, and clearly demonstrates the existence of a coastal subsidence pattern in Northern California peaking in the area of the Humboldt Bay, and likely of tectonic origin.

On the downside, the figures could be much improved, as I struggled to find the location of most stations referenced in the text and in the tables. I provide a few suggestions below.

More importantly, the references provided in the text are somehow outdated, and an important paper published in 2018, that also estimates the rate of subsidence in the area using tide gages and GNSS, is not cited in the present manuscript (Montillet, Melbourne and Szeliga, 2018, JGR Ocean, "GPS Vertical Land Motion Corrections to Sea-Level Rise Estimates in the Pacific Northwest"). This paper reports estimates of VLM rates using part of the dataset used by Patton et al., which may (or may not) be in agreement with the estimates of Patton et al. A more recent paper from the same group appears to include updated estimates (He, Montillet et al., 2022, Remote Sensing, MDPI). A comparison with these previous works, and generally speaking, a more thorough review of previous attempts to constrain VLM in the area, as well as interseismic strain accumulation, and a refreshing of the figures, would certainly improve the present manuscript.

We evaluate Montillet et al. (2018, not 2022) and include a comparison. They seem to think that the uplift associated with the saf has to do with the distance to the shoreline (it is more likely to be due to whether the fault is in a restraining or releasing bend) and they don't even know where the northern termination of the San Andreas fault is.

Our estimate for RSL is basically exactly the same as in Montillet 2018. They report -0.8 ± 0.2 mm/yr for the CC tide gage we report -0.84 ± 0.14 mm/yr. Our RSL for the North Spit tide gage is 5.20 ± 0.17 mm/yr and they report, for HUMB, 5.7 ± 0.9 mm/yr. These numbers are very close. However, the tide gage data from the North Spit tide gage are a very short time series, so need to be tied to a longer series of data. We tied the NS data to Crescent City, while the Montillet paper does not.

Finally, although the estimated VLM rates reported in this manuscript (from tide gages, leveling and GNSS) form a generally coherent result, one specific treatment of the results would deserve a more careful analysis. Namely, the interpretation of a present-day accumulation of interseismic deformation at the location of the Little Salmon fault (LSF) may not be fully convincing to the reader (please see Figure 6). Indeed, the apparent "step" of ~ 2 mm/yr at the location of the LSF would require that the fault should slip interseismically up to the surface ("creep"), or at least very close to the surface. However, the authors do not mention this straightforward interpretation, and neither do they discuss its implications nor its possible consistency with independent evidence.

We decided to leave out the Little Salmon fault results and discussion. This part of the paper is complicated and we want to simplify the paper. Preliminary results from our paper led by Kathryn Materna shows that about 50% of the VLM rates in this region are caused by the CSZ and 50% of the VLM rates are caused by crustal faults. We decided to let this discussion live in this other forthcoming paper.

Furthermore, a number of stations along the "North Spit", exhibiting a behavior departing from the simple "step" pattern across the Little Salmon fault, are ignored in the analysis. In section 2.2.2, the authors mention these discarded points: "We note some leveling data that are from sites on the North Spit and plot these as white circles because they are also to the west of the highway 101 data (they show a larger negative rate).". Further justification would be required to convince the reader that these points should indeed be considered as outliers, even if their uncertainty (as far as I can judge from the supplementaty material) is similar to that of other points that have been considered as valid for the interpretation. Considering all the datapoints together (valid or "outliers"), one may reasonably wonder whether the local subsidence pattern reported by Patton et al. may be actually enhanced by a local sediment compaction process, in addition to the tectonic process (whether due to a shallow fault or interseismic loading of the megathrust). Even if the authors do mention these non-tectonic processes, the justification brought by the authors to discard them as negligible warrants further justification.

We clarify this in the text. Some points are not plotted because they are far from the hwy and some points are not plotted because they have a large uncertainty. We also later realized that the points on the north spit are plotted, so we eliminated the text, "We note some leveling data that are from sites on the North Spit and plot these as white circles because they are also to the west of the highway 101 data (they show a larger negative rate)." We point the reviewers to the supplementary (now) S-4 to see our evaluation of nontectonic factors. INSAR results (e.g., Blackwell et al., 2020) show that VLM rates span different geology types (e.g., the VLM subsidence rates that span the uplands north of the lower Eel River Valley and the Eel River floodplain in the lower Eel River Valley have the same rates of subsidence). And preliminary results from other researchers show different patterns of VLM though time. It will take a few years before these INSAR data will be able to be used to disentangle different sources of VLM.

Overall, this manuscript brings a number of important conclusions, that could be better supported by a more careful discussion of the assumptions, improved figures and a more thorough discussion of its implications in the context of pre-existing state of knowledge.

LINE-BY-LINE COMMENTS

line 26: missing point (typo).

We found a missing period. Thanks!

line 41: please replace "This" by "Present-day", to better distinguish deglaciation after the LGM from today's accelerated ice melting.

Excellent, changed!

line 49: "Pacific Northwest": please define for non-US readers.

We rewrote "northeast Pacific Ocean." Hopefully this helps (?).

line 49: sea level rise is estimated to 2.28 ± 0.03 mm/yr from a study of 2009, but other studies estimate different rates. For instance, Montillet et al. (2018) report a sea level rise at 1.99 mm/yr, which is well outside the confidence interval used here. A short discussion about the differences between published estimates could be useful to better constrain the uncertainties.

We are actually using the rate from work published by Montillet et al. (2018) but one of the coauthors did not have the time to complete the tide gage analyses prior to submission. We include Montillet et al. (2018) rates data in the revision.

line 57: it took me some time to spot the location of "Crescent city" in Figure 1. This figure needs to be greatly improved to help the reader to quickly locate the area of interest. Presently, it is a bit cluttered and text fonts are generally too small.

We removed the fault labels and made other improvements. We cannot anticipate that it might take someone a few minutes to locate a position on this map since there are about 60 things on the map that need to be on the map. However, we enlarged the CC in the inset map. This was good advice!

line 60: the references reported here are rather old (1994 and 2006). More importantly, this does not look like a fair depiction of the existing state of knowledge regarding the unusually large RSL rate tide gage recorded at Humboldt Bay tide gage. For instance, Komar, Allan and Ruggiero (2011, Journal of Coastal Research) mention explicitly that this tide gage is affected by tectonic motion.

We added a statement regarding Komar et al. (2011) in the introduction. Thanks!

Figure 1: the area of interest of this study is too small in this figure, or at least should be identified by a rectangle, using the limits in Figure 2. Also, the green layout is questionable. Finally, the plate kinematics boundary conditions in the vicinity of the triple junction, and the interseismic velocity field as deduced from GNSS could be displayed here, to facilitate the understanding of the local tectonic regime. Please see for instance the recent paper by McKenzie & Furlong (2021, Geoscience Letters).

We added an AOI rectangle (and other ones). Great idea!

We updated the map of Cascadia but not because of its lovely green color. (we hope the reviewers have a modest sense of humor)

Yes, we like the McKenzie and Furlong paper and refer to that paper in the text now. Adding that information will make the figure difficult to read (the reviewers already think that our figures are too complicated). Since someone else has a great figure, the readers can look at the figure in their paper.

Figure 2: it is hard to find the relevant information on this figure. The strong contrast of the shaded topography in the background makes it difficult to distinguish the symbols. Also, the most important stations should be highlighted with a bigger symbol, or using another strategy. Currently, all fonts have the same size, so it is hard to identify the important information, for instance to quickly spot the Crescent city and North Spit stations, which are at the core of the study. Extent of the map in the inset should be drawn

on the main map using a rectangle (just as is done in the "inset of the inset"... which by the way may be unnecessary). Tick marks for latitudes / longitudes should be tighter (every 0.25° or 0.1° if non-annotated tickmarks are used) and the fonts should be enlarged. In the inset, there is ample space for writing the name of the main stations in full, instead of abbreviations that require the reader to read the caption.

We are using a min-max color distribution for the hillshade and already are plotting this at 50% transparency. We lowered the transparency in the hillshade to 70%. We tried lower transparency, but it becomes difficult to interpret the topography at lower transparency levels.

We are using font symbology for graphical hierarchy but enlarged some of the fonts. Thanks!

We added a rectangle in the main map showing where the inset is. Great idea!

We do not place lat long tics at greater frequency than 1 degree. These are not really needed for any reason (the only reason for lat long is to place this figure in a global setting and in context to figure 1). This comment contradicts both reviewers' complaints that this figure has too much information. The important figure for comparing geodetic data with the (now) figure 6 (the east-west/trench-distance and north-south geodetic plots) is the (now) figure 5 (the results map). But the GPS and tide gage labels are better for this type of geospatial comparison. We don't anticipate that anyone will use the tics to compare these data.

line 122: when mentioning GIA corrections, the authors could provide here a rough estimate of the magnitude of the correction (the reader has to wait until line 373 to learn that the subsidence rate is ~ 1.4 mm/yr), and, more importantly, how it was derived.

We cannot find anything about GIA anywhere near line 122. However, we added some basic information that the reviewer requests to section 1.1.4. late during revisions (earlier this week) we obtained the data from Englehart (which led to a complete reworking of all tables and figures) so that we are no longer using a linear estimate for GIA. We now use the GIA data directly from Englehart. We added a statement in section 1.1.4 about the magnitude of GIA for the study area.

There is nothing on line 373 (or anywhere near line 373) about GIA, maybe the reviewer is actually writing about the section that begins on line 400? Obviously, we would not want to make a statement about how much the GIA rates are until the results section.

line 176: "known earthquakes": which ones (time, location, magnitude, depth), and how much static displacement is removed (and on which stations)?

This is actually in reference to line 194, not line 176. USGS does this analysis, and they publish this information. We present this information in a new supplemental table, table S-3. Great idea!!! It was time consuming, but quite educational. The static vertical offsets were more than I had anticipated!!!

line 182: the Englehart et al. (2016) referenced in the text does not exist in the references. Instead, there is a Dura, Englehart et al. (2016) reference, and then an Englehart et al. (2015) reference. Which one is correct? Assuming that the authors mean Englehart et al. (2015), instead of Englehart et al. (2016), it remains unclear what value(s) the authors have used to correct for GIA in this study. This should be explicitly mentioned in section 1.1.4.

We fixed the reference. It is 2015. We had used a linear rate from the Englehart figure (and mentioned this, albeit in a poorly written manner apparently), but we are now using their data directly. YAY!

line 193: it seems that there is a confusion in the values reported in the text for Crescent City. Instead of a RSL rate of -0.97 mm/yr at CC (Table 1), the authors seems to have used a value of -0.83 mm/yr (Table 2) to come up with a VLM rate of 3.11 mm/yr after subtracting an eustatic sea level rate of 2.28 mm/yr.

yes, we had two tables, one using the 2.28 mm/yr and one using the Montillet 2022 rate of 1.99 mm/yr. Apparently, we messed up. We fix this in the revision.

line 195: again, there is a confusion in the numbers reported in the text and in the tables for North Spit, but this time I cannot figure out where the mistake is. Indeed, 5.58 mm/yr (relative VLM rate of NS minus CC, as in Table 1) minus 3.11 (VLM for CC) should yield 2.47 mm/yr, instead of 2.69 mm/yr reported in the text and in Table 2.

We have corrected any mistakes. We had initially used one rate, then changed to a different rate, then switched back to the original rate (so there were some mistakes). Now that we have finalized the rates using Montillet (which was our goal the entire time), all should be right in the world.

Figure 3: the panels G, H, I and J start in 1975, but they could be made to start in 1930, just like panels A through F, to facilitate comparison and visual correlation.

We are now plotting these data differently and they (accidentally) happen to satisfy this comment.

line 258: missing point (typo).

We cannot locate this missing period. Though we found a couple missing periods while editing, so one of these could be this missing period (aka point).

Figure 4: the colouring scheme used in this figure makes it hard to distinguish the uplift rates, due to (a) exceedingly strong contrast of the shaded relief background and (b) colouring of the faults and roads.

We improved the figure (now figure 5). Thanks! Also, we got rid of the red-green color scheme and are now using a red-blue color scheme. We cannot believe that we made that mistake (some color blind people would have had difficulty interpreting this figure).

Figure 5: the font sizes used in this figure are too small. It is really hard to distinguish the most important benchmarks, which could be highlighted by a bigger symbol. The upper panel showing vertical land motion versus longitude is not easy to interpret because the direction of projection is not perpendicular to the Cascadia subduction, as explained in the text. The apparent complexity is partly due to this inadequate projection, which makes it hard for the reader to disentangle the different processes at play in the area.

Yes, this figure (now figure 6) was destroyed when we placed it into the word document as requested by the Tektonika website. we don’t know which sites are the most important ones. Every site is equally important, so will have the same size symbol. We recommend that this process for submission be changed for future submissions.

We will plot relative to the trench. Great idea!

line 296: "handing" should be replaced by "hanging" (typo).

Yes, on line 322, we fixed this. Thanks!

line 302: if the velocity difference across the Little Salmon fault shows up as a step, then this strong velocity gradient implies that the fault is creeping at the surface, or very close to the surface (assuming that the step was not caused by one or several earthquakes, which is implicit since the authors estimate a slip rate). Could the authors comment on this obvious conclusion? Does this fit with independent observations?

As mentioned above, with more detail, we eliminated this part of the paper.

Figure 6: four stations located on the North Spit, with subsidence rates clustering around -2.75 mm/yr, have been ignored in the map, as well as in the interpretation in the lower panel (the dots are left with a white fill, implying that they are not relevant). Is there a specific reason for excluding these measurement points? Also, the lower panel displays the vertical velocity as a function of northing, whereas the Little Salmon fault has a northerly (~ N150°E) azimuth. Rotating the projection, or, at least, using the easting instead of the northing, would be more appropriate to highlight the proposed velocity step associated with the fault.

Those 4 sites are all on the map. However, at this scale, this is difficult to tell. It is not possible to enlarge the symbols as the reviewers' request, and still be able to see all the geodetic sites. There are many examples of this on the map. This is a challenge that can only be met by a user plotting these data on their own computer (as is common for many types of datasets). We plan on placing the GIS data on the zenodo website but want to wait until the manuscript is accepted before we do this (e.g., the data currently have many extra fields in the database that help us track the data evolution over time; we will remove these extra fields prior to uploading to zenodo but don't want to do this until this is all finalized).

As mentioned above, with more detail, we eliminated this part of the paper (the static offsets). Though, because we were calculating vertical separation rates for the blocks on either side of the fault, the orientation of the profile is irrelevant. If we were doing 2-D elastic modeling, this would matter. If we were doing 3-D elastic modeling, this would not matter.

line 324: "plate tectonic factors" is too vague, as it may exclude the existence of shallow crustal faults (which, accordingly, are a product of plate tectonics). Indeed, "plate tectonic factors" may be incorrectly understood as "processes taking place at plate boundaries", i.e. not intraplate deformation. I suggest using the more generic term "tectonic motion", instead of "plate tectonic factors".

This is on line 352, not line 324. We changed this to tectonic motion.

line 333: the word "lithospheric" may be confusing and could be removed in this sentence, since crustal faults are evoked later in the sentence. Shallow crustal faults do deform the whole lithosphere, but their footprint is mostly restricted to the upper crust (contrary to the megathrust, which indeed produces deformation at the scale of the lithosphere).

This is on line 362, not line 333. The lithosphere includes the crust and its faults. We would like to make this change but then the sentence would not apply well to the CSZ which is also in the sentence. So, made the sentence longer (lithospheric for CSZ and crustal for the crustal faults). But, we noticed that we need to add the word with before crustal faults. Hopefully this works (?).

lines 384 and 385: the wording "evidence" is unnecessarily repeated in this sentence. Please reword.

We only see the word evidence once in the sentence that begins on line 424 (presuming this is the sentence). We changed the word evidence to support for.

line 417: the East-West trend in VLM rates could be better highlighted if the symbols corresponding to the Humboldt bay transect were distinguished in Figure 5A.

yes, if the figure were not destroyed by placing it into an ms word document, this would be a non-issue. Hopefully the final pdf will be able to include a vector version. We advise that the submission system change in the future. Though we did add labels showing data that are in the Humboldt Bay and Eel River valley areas. These labels are in both panels, east-west and north-south.

line 424: the authors argue that the different VLM rates at Crescent City versus Humboldt bay area can be mainly explained by a variable distance with respect to the trench. However, the two locations are at the same longitude, whereas the trench is oblique, which makes the Figure 5A confusing. Reprojecting the whole figure perpendicular to the trench would certainly help the reader here.

We did this. Great idea!

Line 443: "We note some leveling data that are from sites on the North Spit and plot these as white circles because they are also to the west of the highway 101 data (they show a larger negative rate).". More justification is needed here. It is clear that the authors have excluded some stations, and we can see which ones, but the choice seems arbitrary.

As mentioned above, we explain this better in the revision.

line 459: "trends (...) previously thought to be anomalous". This assertion demands further justification. In what sense is the trend considered "anomalous"?

we mentioned earlier in the paper who thought these data were anomalous (at least in a previous version of the paper). We will make sure that we include this statement and, for the sentence on line 505 (not 459) we will again list these references.

Table S-1 title: do you mean "S-1", instead of "S11"?

Thanks, we fixed this.

Table S-1 caption: "1988-1968" should be "1988-1967" according to the text (section 1.1.2.)

Thanks, we fixed this.

3/ ——————————————————

Second decision letter based on the revised manuscript and the above author's answer.

Dear Jason,

We (associate and executive editors) have made a careful reading of your revised paper and rebuttal. You have clearly taken into account, or answered to, all reviewer's comments. Your paper is much better now. The problems with the figures have been addressed, and their quality is now far better in the provided PDF versions.

Associate Editor's recommendation is: “My recommendation regarding the submission to TEKTONIKA "20th to 21st Century Relative Sea and Land Level Changes in Northern California…" is accept. The authors have indicated they will upload a GIS dataset associated with this study to Zenodo on acceptance, so I ask that this is fulfilled as soon as conviently possible so that the appropriate dataset DOI can appear on the typeset version of this paper.”

We'll however ask for a final round of minor revision to fix some issues, mainly with the aim to help for the production of the paper (keep in mind that the production team is voluntary for TEKTONIKA)

Our decision is thus to accept your manuscript for publication in TEKTONIKA provided you do the minor technical corrections listed in the attached PDF document. Please upload your revised files as soon as possible. Be careful to clearly identify the final version by quoting FINAL in the file name. We hope you will be able to make this final revision within a week (due date: 15 December).

Note that we'll be able to formally accept your paper only after receiving your final revision.

All the best

Jack Williams (Associate Editor)
Robin Lacassin (Executive Editor)

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Manuscript by Patton et al. submitted to TEKTONIKA - Final minor technical revision requested by Associate and Executive Editors, 9 Dec 2022

1/ Please indicate authors' ORCID numbers if available.

2/ In Introduction second paragraph, as requested by reviewer, you changed “Pacific Northwest” for “northeast Pacific Ocean”. It's still confusing as you continue using Pacific Northwest in the following text.

I propose to write the first sentence of this paragraph as:

"For the period of 1925-2006, sea-level rise along the USA northwestern coast, from northern California to Washington states (hereafter called Pacific Northwest), is estimated to..."

Then use “Pacific Northwest” in the following text.

3/ Also in the introduction, you write “After the 2010 workshop...”, but nothing has been said about this workshop in the text itself. I agree that you explain it in the abstract, but I'm not sure it's the right place to put these infos. Also the main text should be understandable alone. Please add some explanation in the introduction, or (perhaps better) move relevant information from the abstract to the introduction.

4/ In the Section ‘Spatial Trends’ the text should be clarified for how distance from the trench was measured, particularly as the trench's map view geometry in Fig 2 looks quite complex. For example, was each site projected to its closet point to the trench, were they projected onto a single cross section running 90 degrees to the trench, or are the authors simply using longitude as a proxy for distance to the trench?

5/ In the .doc manuscript, it's sometimes difficult to separate figure captions and the main text. Please use a different font for the captions, or different line spacing, or another way to identify them easily. This will help for production.

6/ Figures

I agree that Figure 6 has now a much better resolution than in the submitted manuscript. However I fear it will be difficult to reproduce with all the details in the final published PDF. I suggest to also provide it as a high quality supplementary PDF file (and referring to it in the figure caption in the main text)

7/ Data availability / Supplements:

You are using in the figures fault traces from the "USGS active fault and fold database". Please indicate in the data availability section how to accede to this database, and include a relevant reference, ideally with DOI.

I will not detail here, but same remark than above for other databases. Be sure to provide links and references for the different database you are using (NGS leveling, GNSS, tide gauges "from NOAA websites", etc...). Provide relevant statement in the data availability section.

At relevant place in the main text, refer to the supplementary files you are providing. I can see it's done for S-Tables and some other supplements, at least. Verify that all supplementary data are rightly called in the text. Same for the datasets available from Zenodo repository.

Explain in data availability section which datasets are on Zenodo repository. Provide DOI of the dataset(s).

8/ References

DOI are lacking for many references. To help for production, please update and verify all references. This will help a lot for production. Keep in mind that the Tektonika production team is voluntary - help them as much as you can ;-)

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Final author's answer

Greetings,

I have uploaded the final text here and uploaded a new zip file of supplemental files. I did not change any figures or tables. I also uploaded data to zenodo and have that information in the paper. Let me know if there is anything else I can do to help. I am headed to AGU and should be able to make additional changes while on the road.

Cheers, jay

Thanks for these additional improvements that I just made.

1. i added ORCIDs in a section called ORCIDs.

2. I incorporated your suggestion about pacific northwest. great solution!

3. i agree that the workshop mention in the abstract was strange. i moved it into the intro section as you suggested.

4. i added a GIS shapefile to the zenodo page, a brief description of how i calculated trench distance, and a more detailed explanation in the read me file on the zenodo page. https://doi.org/10.5281/zenodo.7420441

5. i made the figure captions red, so they should be easy to identify.

6. i added the full version of fig 6 to supplemental S-6. now S-6 has 2 pages: page 1 is the "high-resolution" version of fig 6 and page 2 is the plot with error bars from all data points (the original S-6).

7. I added links and explanation for (a) the USGS fault database, (b), the sources for geodetic data from the NGS, NOAA, and USGS, (c) the files on zenodo. The files on zenodo include: (i) geodesy GIS dataset, (ii) cascadia trench GIS dataset, (iii) table data in spreadsheet, and (iv) all supplemental files in a zip file.

8. i just found several references that needed to be removed from the list (they were from a previous version of the paper). i added DOI numbers for most every paper (the ones that have them). i also did my best to ensure the references are all formatted the same.

Final editor's decision

Dear Jason and co-authors,
Thanks for uploading your final revision.

We have reached our final decision regarding your submission to τεκτonιkα, "20th to 21st Century Relative Sea and Land Level Changes in Northern California: Tectonic Land Level Changes and their Contribution to Sea-Level Rise, Humboldt Bay Region, Northern California".

Our decision is to: Accept Submission
The paper will now move to our voluntary production team. They will contact you for additional informations, if needed, and for correction of the proofs.

Thanks a lot for submitting to Tektonika. Your manuscript was our first submission, and it's our first accepted paper.

All the best
Robin Lacassin, TEKTONIKA executive editor