Review Report

Trexler et al., Quaternary active faults and the role of inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California. TEKTONIKA, 2024.

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1st Round of Revisions

Decision Letter (3 July 2023)

Dear Authors,

We have now received two detailed reviews on your manuscript "Quaternary-active faults and the role of inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California". Both agree that your study is interesting and could be published in Tektonika. However, they are critical on different aspects, implying that your manuscript needs quite extensive revision.

I have appended the two reviews to this letter, as well as the evaluation by our associate editor, Jack Williams. The second reviewer has also made some comments on a .doc file which is attached to this message too.

I recommend that you take into account, or answer to, all suggestions and recommendations made by the reviewers. After revising your manuscript please provide us with a detailed rebuttal letter explaining how you have addressed all these comments. Also provide a manuscript with all changed marked, together with a clean revised manuscript.

In the abstract you are saying that you are presenting a "synthesis" and a "comprehensive review". If you have this objective in mind, I urge you to verify if your presentation of the state of the art and of the references is fair and exhaustive. The first reviewer has commented on this ("Despite the intent to provide an authoritative summary of previous work, there are numerous errors and misrepresentations of the cited literature in the manuscript, and some key references are not included").

Our associate editor is proposing to give you at least two months to revise your manuscript. Given that summer break is coming, I am proposing a due date in the second half of September. Don't hesitate to contact us if you think you will need more time.

Best regards

Robin Lacassin, Tektonika Executive Editor

Associate Editor, Jack Williams:

Dear authors.

Thank you for submitting your review of active faults in the Sacramento - San Joaquin Delta region to Tektonika. Having read through the manuscript myself and the comments we received from two reviewers, it is clear that this manuscript can provide a useful contribution to understanding this region's tectonics and seismic hazard. However substantial revisions are required in order to achieve this.

In particular, I would ask that you pay particular attention to the comments from one reviewer about reconciling the 3 mm/yr convergence proposed for this region to its subdued topography and constraints from more recent geodetic datasets (e.g., the models used for UCERF or the 2023 US NSHM update). It would also be useful to discuss which structures are accommodating the 5 mm/yr of dextral displacement that is inferred to occur within the Delta (Lines 460-461 and 846-848)? Are these structures blind, or is it because lateral offsets are harder to preserve in the geomorphic record of mapped faults?

A brief comparison is made at Lines 893-894 to the strain partitioning model proposed for this region's faults and international examples. I would ask that these ideas are 'fleshed-out' as in its current state, this manuscript is very regional in its outlook, and making these international comparisons will increase its scope. There are certainly no shortage of literature on the topics discussed in this paper (e.g., Berryman et al 2023, Conand et al 2020; Kim et al 2023; fulls refs at bottom)

Both reviewers also ask for restructuring of the manuscript to make it more concise and focussed. Please also consider their requests for new figures and tables, and correct the references as suggested. My own impression is that the clarity of the manuscript would benefit from more cross references between the text and figures.

Given the nature of these reviews, I am proposing that you have at least 2 months to address them. On resubmission, it is likely that we will sent it out again to review. I hasten to add that this study has interesting and positive aspects, and I look forward to seeing the resubmission.

Best, Jack (Tektonika Associate Editor)

References
Berryman, K., Rattenbury M., Bannister, S., ...Howell, A. Geological structure informs rupture propagation and surface rupture complexity during the 2016 Kaikoura earthquake, New Zealand: insights for future large earthquake hazard. Turkish Journal of Earth Sciences 32, no. 3 (2023): 330-350.
Conand, C., Mouthereau, F., Ganne, J., Lin, A. T. S., Lahfid, A., Daudet, M., ... & Bonzani, M. (2020). Strain partitioning and exhumation in oblique Taiwan collision: Role of rift architecture and plate kinematics. Tectonics, 39(4), e2019TC005798.
Kim, N., Park, S. I., Cho, C. S., Cheon, Y., & Peace, A. L. (2023). Neotectonic transpressional intraplate deformation in eastern Eurasia: Insights from active fault systems in the southeastern Korean Peninsula. Geoscience Frontiers, 14(4), 101559.

Comments by Reviewer 1 (Jeff Unruh)

The stated purpose of this paper is to offer a “comprehensive overview of current understanding of geologic structures in the Delta” and improve seismic source characterization. The manuscript requires significant revision to successfully achieve these goals.

The paper doesn’t make a case for why a “comprehensive overview” is needed. Many of the key references cited in the paper already provide “comprehensive overviews” (e.g., Weber-Band, 1998; Krug et al., 1992; Unruh et al., 2015; Unruh, 2021; etc.). Despite the intent to provide an authoritative summary of previous work, there are numerous errors and misrepresentations of the cited literature in the manuscript, and some key references are not included. Many of the figures are reproduced from previous work with little/no change (including two figures reproduced from Trexler et al. 2022). The paper would be better served by a focused summary of relevant previous work that bears on the kinematic model for the Pittsburg-Kirby Hills fault and Montezuma hills, rather than attempting something more grandiose. The manuscript also presents a relatively lengthy summary of the

Mesozoic and Tertiary development of the western California convergent margin, going into details that are covered much better elsewhere and not directly relevant to the main topics of the paper. This material should be significantly trimmed to reduce the length of the paper and improve the overall focus.

The most significant technical deficiency in this paper is the analysis of geologic and geodetic deformation rates. Rates drive everything in seismic hazard analysis, so it is very important to get them right, especially if the intent of the paper is to improve seismic source characterization in the Delta. The authors present transects of deformation rate across the Pacific-Sierran plate boundary that rely on ~20-year-old geodetic studies to support an interpretation that faults in the Delta accommodate 5 mm/yr of dextral shear and 3 mm/yr of "boundary normal" (presumably NE-directed) shortening. As shown in Figure 3 in the paper, these rates overpredict the total integrated Pacific-Sierran motion at the latitude of the Delta, which is a red flag that something is wrong. The deformation rates utilized and preferred by the authors are not consistent with well-documented observations of modest late Cenozoic structural and topographic relief in the Delta, and the authors do not present a testable model for distribution of their preferred rates on known Quaternary structures. In addition to addressing technical errors in the transects (discussed in detailed comments below), I recommend that the authors review geologic fault slip rates compiled for the 2013 UCERF3 statewide seismic hazard model and use them to recalculate the transects. I also strongly recommend that the authors review the new geodetic deformation models developed for the 2023 update of the National Seismic Hazard Map (Zeng, 2022; Evans, 2022; Pollitz, 2022; Shen and Bird, 2022), which use the most recent compilation of the GPS velocity field and modern modeling methods to estimate fault slip rates at the latitude of the Delta. The authors should acknowledge this relevant recent work and address any discrepancies or disagreements with their preferred model.

Following are comments about specific passages in the text.

1) The abstract should be completely re-written to focus on the key findings of the paper. Keep it short and punchy. The first paragraph of the abstract reads like an introduction to the main text. It is completely unnecessary and should be cut.

2) Line 146-147: Text states "A similar Paleogene-age extensional basin is exposed within Great Valley Group strata on the northern flanks of Mount Diablo…". It's not a "similar basin"; it's the exact same structure, as recognized by MacKevett (1992).

3) Line 170: text refers to the "Coast Ranges block". The (over)use of "block" to describe areas of similar geology should be avoided if it is not a kinematically appropriate description. The northern Coast Ranges currently are accommodating ~36 mm/yr of distributed NW dextral shear across their entire width, which is not very blocklike behavior.

4) Line 195: Text states that the Midway fault is part of the Great Valley (thrust) fault system, but this structure is likely a strike-slip fault, not a thrust or reverse fault.

4) Line 208: "Guzofski" is not spelled properly. Regarding the bibliography in general, many of the citations are erroneous or incomplete. I am surprised that the paper was submitted in this condition, and that the editors sent it out for review without asking that the authors fix the references first.

5) Line 235: Text cites Johnson (1992) to the effect that the Midland fault extends from the city of Davis south to the town of Byron. "Johnson 1992" is one of the incomplete citations in the bibliography, so I couldn't look it up to make my own assessment of this mapping. In any event, mapping of the Midland fault in California

Division of Oil, Gas and Geothermal Resources (DOGGR) gas field data sheets shows that the structure breaks up into a series of right-stepping en echelon splays at the latitude of the Lindsay Slough gas field at the north end of the Montezuma Hills, and the splays are interpreted to continue northward through the Maine Prairie and Bunker gas fields in the northwestern Delta. The Midland fault is not mapped in the Millar West or East Dixon gas fields south of I-80, so it's not clear what Johnson (1992) is mapping as the northern continuation of the Midland fault. In any event, the trend of the fault, if it continues north of I-80, is toward the Winters gas field, not the city of Davis. I strongly recommend that the authors review (and cite) other work on the Midland fault (particularly in the AAPG and SEPM gray literature) rather than relying exclusively on Johnson (1992).

6) Line 240: Text states that the Midland fault initiated in late Cretaceous. This is not correct. Restoration of Paleogene slip in the Rio Vista basin restores all separation of the Cretaceous section across the Midland fault. There is no evidence for structural growth in the upper Cretaceous section.

7) Line 250: "Marsh Creek fault" is misspelled. For the record, there is no positive evidence that the Marsh Creek fault is a Q-active structure. To date, the fault has not been trenched to demonstrate activity or non-activity, and there are no documented field relations that indicate Q activity.

8) Line 251: Text states that the "Midland fault has no Neogene offset south of the West Tracy fault." This is because the exposed trace of the Midland fault here is folded in the forelimb of the anticline in the hanging wall of the West Tracy fault.

9) Line 181+: Text asserts or implies that the "Montezuma Hills fault" and Denverton Creek fault system are part of the Q-active Pittsburg-Kirby hills fault zone. This is just a speculative guess on the part of the authors, not a well-documented relationship (at least not that I'm aware of). These Paleogene structures are several km east of the Pittsburg-Kirby Hills fault, and to date there is no positive evidence (that I am aware of or that the authors cite) the faults have been reactivated in late Cenozoic time.

10) Line 294: Text cites multiple refs to the effect that the Pittsburg-Kirby Hills fault merges with the Kirker fault south of the river. This is an inaccurate representation of the literature. The Kirker fault is interpreted to the southern continuation of the Paleogene Kirby Hills fault zone and the ancestral western margin of Rio Vista basin (MacKevett, 1992). The Pittsburg-Kirby Hills fault is a Q-active structure that locally reactivates pieces of this fault system, primarily north of the river where the structures are all still upright and have not been tilted NE on the backlimb of Mount Diablo anticline. No one I'm aware of has argued or claimed that the Q active Pittsburg-Kirby Hills fault extends all the way south to the surface trace of the Kirker fault.

11) Line 320: text states that there is 2 km dextral offset of an 8 Ma magnetic anomaly across the Sherman Island fault, and a 200 m offset of the same anomaly across the Davis fault, citing "Unruh 2007" and Krug et al. 1992. There are several problems here. First, the text should specify what geologic feature is associated with the "8 Ma magnetic anomaly". Is this the Miocene Neroly Fm? (the Neroly is loaded with magnetite and makes prominent aeromag anomalies). If so, then are comparable offsets of the Neroly contacts mapped across the faults consistent with this interpretation of the aeromag? If the Neroly Fm is not the source of the anomaly, then what is the anomaly associated with and why do you believe it resolves 200 m of dextral displacement on the Davis fault? Second, the text cites "Unruh 2007" as a source for this relationship, but there is no "Unruh 2007" listed in the bibliography, and as far as I know Unruh has never discussed this aeromag anomaly in any

publication. I would be surprised if Krug et al. 1992 discuss it, because the focus of that paper is Paleogene structure north of the river. Finally, the Sherman Island, Antioch, Davis, etc. faults are antithetic structures to the Midland fault that splay to the northwest and create secondary graben and depocenters in the Rio Vista basin. Please review structure maps of Delta gas fields published by DOGGR, and the work of Krug et al. (1992), to see how the faults are distinguished spatially.

12) Line 344+: text states that the Clayton and Marsh Creek faults are considered "the northern extension of the Greenville fault system." Considered by who? There is no citation for this assertion in the paper, and as far as I know there is no consensus around this issue in the local Bay Area seismotectonic community. I reiterate that there are no field observations of faulted Q deposits along these structures. There have been no paleoseismic trench investigations of the Marsh Creek fault to evaluate activity or non-activity. The Clayton fault was extensively trenched in the 1980's and 1990's during construction of residential housing in and around the town of Clayton, and according to Jim Joyce (engineering geologist who did the trenching for the developers) no positive evidence of Q activity was observed.

13) Line 427: text states that the Green Valley fault "runs N-S along the western margin of the Delta". Strictly speaking, it runs along the western margin of Suisun Bay, which is not generally considered to be part of the Delta proper.

14) Line 432+: Citing Lienkaemper (2013) as the main source of data on the 1980 Livermore earthquake is a bit lazy given that there are plenty of primary sources.

15) Line 456: Text states that "up to 5 mm/yr of dextral displacement may be accommodated by structures within the Delta." Please stop and do a gut check on this number. The GPS velocity field shows a smoothly decreasing velocity gradient across the Coast Ranges and Diablo Range. A rate of 5 mm/yr suggests that distributed shear through the Delta is comparable to the slip rate on the northern Calaveras fault, which well to the west of the Delta (on the opposite side of the Diablo Range!). The 5 mm/yr suggested for the Delta is probably twice the slip rate on the Greenville fault, and thus is contrary to the observation of eastward-decreasing rates and a smooth gradient. If the rates are truly this high in the Delta, then why isn't there a big strain rate anomaly at the latitude of the Delta in the GPS data? Where is all this displacement hiding? A major part of the problem here is in the slip-rate bookkeeping. The south transect across the plate boundary in Figure 3 overpredicts the integrated plate boundary motion, and there are SERIOUS problems with the slip rates used in the transect (slip rate on the Hayward fault is closer to 9 mm/yr; both the Concord and Greenville faults are included when they are actually the same structure at different latitudes; the slip rate on the Greenville fault is too high; etc.). The paper cites Prescott (2001) and d'Alessio (2005) as sources of GPS data and slip rates for the transect, but these papers are ~20 years old! Geologic slip rates on major faults crossed by the transects were comprehensively reviewed and vetted for the 2013 UCERF3 statewide EQ hazard model. The authors should carefully review the UCERF3 literature (especially Appendix B by Dawson and Weldon) and compare the geologic slip rates against the values in the transects in Figure 3. Additionally, four new deformation models of GPS data for the 2023 National Seismic Hazard Map were published in late 2022 (Zeng, 2022; Evans, 2022; Pollitz, 2022; Shen and Bird, 2022). What do these more current slip-rate models based on GPS data suggest for structures in the Delta? What does the range of rates estimated by the four models suggest about uncertainty? As currently presented, the "5 mm/yr" rate of dextral strike-slip through the Delta proposed by the authors is simply not credible.

16) Line 481+: The plate motion model of DeMets and Merkouriev (2016) predicts that the relative motion between the Pacific plate and Sierran microplate at the

latitude of Rio Vista is 36 mm/yr of dextral shear directed toward N29°W. Before you can decompose this into a component of “plate boundary-normal compression”, you must first specify what the orientation of “the boundary” is relative to the N29°W plate motion vector. What, and where, is the Pacific plate-Sierran microplate boundary? I would argue it’s a broad zone rather than a discrete plane, and thus an appropriate approach to evaluate the presence or absence of “plate-boundary-normal compression” is to do a vector summation of the slip rates of major faults along an E-W transect at the latitude of the Delta (similar to the approach of Kelson et al., 1992), and then look at the residual vector motion east of the Greenville and Concord faults. This way you avoid the problem of the north transect in Figure 3, where any shortening at a high angle to Pa-SN motion along the western margin of the Sac Valley is not accounted for. Note that there are significant differences in the strike of the San Andreas system dextral faults, so they should be treated as vectors rather than simply summing scalar slip rates as in Figure 3. In any event the authors need to specify how they are decomposing Pa-SN motion--including how they determined “the” orientation of the plate boundary--before we can assess what the magnitude of “boundary-normal compression” is.

17) Line 491+: I strongly, strongly recommend that the authors review the new GPS deformation models for the 2023 National Seismic Hazard Map and consider revising this section, which depends heavily on older references.

18) Section 4.3: The authors cite Guzofski et al. (2007) as interpreting that the 1983 Coalinga earthquake “occurred on a west-vergent backthrust above an east-vergent fault at depth”. This is NOT the conclusion of Guzofski et al., and it is NOT the conclusion of seismologists who evaluated waveforms of the mainshock. See papers by Eaton, Eberhart-Phillips and Reasenberg, Choy, etc. in USGS PP-1487. My strong recommendation is that the authors review and cite the primary seismological data sources for the Coalinga earthquake, and not derivative structural models by Guzofski et al., Namson and Davis, Wentworth and Zoback, et al. Most importantly, please cite other work ACCURATELY.

19) Line 598+: The correct citation for the West Tracy fault seismic reflection study is Lettis Consultants International (LCI) 2022. LCI was solely responsible for scoping the study, managing the data acquisition, interpreting the data, and preparing the report on behalf of DWR.

20) Line 614: The text cites “apparent growth strata within Neogene units in the English Hills and Montezuma hills, including units as young as the Pliocene Tehama formation”, but does not specify the nature of those growth relations. What exactly are they? Westward thinning of the late Neogene-Quaternary section? If so, precisely where has this been observed/documented? Is it above and/or below the 3.4 Ma Putah tuff? Is the timing of structural “growth” consistent with late Neogene stratigraphic and structural relations elsewhere along the western margin of the Central Valley? The text also notes that the structural growth “post-dates the northward propagation of the Mendocino triple junction.” Almost everything of neotectonic interest in this region post-dates passage of the MTJ! Please note that there is a body of literature that points to changes in relative plate motion as a driver for late Neogene regional uplift and deformation of the Diablo Range and northern Coast Ranges, rather than time-transgressive passage of the MTJ. Please see Atwater and Stock for late Neogene changes in relative plate motion, and papers by Ben Page, Dave Montgomery, and Argus and Gordon (2001) for additional background on transpression and late Neogene uplift of the Coast Ranges.

21) Line 695+, Discussion Section: This section starts by citing Prescott et al. (2001) and d’Alessio et al. (2005) to support the statement that “Geodetic investigations suggest as much as ~5 mm/yr of dextral displacement and ~2-3 mm/yr of shortening

may be accommodated in the Delta." I strongly recommend that the authors review the new GPS deformation models for the 2023 update of the National Seismic Hazard Map before committing this to print. Again, I urge a simple gut check of these numbers: 2-3 mm/yr of shortening will drive uplift rates on the order of km/my, which is sufficient to give you topographic relief like Mount Diablo anticline. Is there anything like Mount Diablo sitting out in the middle of the Delta? There's not even anything like the Coalinga anticline or the Kettleman Hills in the Delta! There are only a couple hundred meters of structural relief on the base of the Miocene across the Montezuma hills (Unruh et al., 2015). There is not enough topographic and structural relief in this region to support long-term average shortening rates > 1 mm/yr.

22) Line 763+: In this section, the authors propose that the Montezuma hills are being uplifted by southwardreverse-oblique motion on the Pittsburg-Kirby hills fault. Given that the PKHF exhibits right-lateral focal mechanisms, and the first-order tectonic boundary conditions are distributed Pa-SN dextral shear, the Montezuma Hills is not being translated south in an absolute sense relative to the Sierran microplate. The text should be revised to clarify that the hanging wall of the PKHF is moving south relative to the north-moving footwall. If this is not what the authors mean, then they need to provide a mechanical explanation for how the Montezuma hills are moving south independently of the north-moving crust on the opposite side of the PKHF (i.e., explain how a right-lateral shear traction on the western margin of the hills is making them move south).

23) Line 771+: Here, the authors state that the Montezuma Hills are being translated "up and onto the north-dipping flank of Mount Diablo anticline," possibly along a west-northwest striking, north-dipping thrust fault underlying the Sacramento River. Is this consistent with the previous statement above that the causative structure is the PKHF? Please clarify. It would be useful to draw a north-south cross section across the river to illustrate this interpretation. The cross section can also provide a test of the model by showing whether the base of the Tehama Formation in the southern Montezuma Hills projects southward above its elevation in Antioch south of the river, consistent with being elevated in the hanging wall of a thrust fault.

24) Line 777+: The authors probably don't know this, but they re-stating arguments that were originally proposed in the 2009 DRMS study as a basis for defining the "Montezuma Hills areal source zone". As noted by Weber-Band (1998), the topographic and structural gradients in the Montezuma hills are to the northeast, which makes it problematic to attribute uplift of the hills to simple reverse reactivation of the north-striking, west-dipping Midland fault, or the dextral-oblique PKHF. Workers involved in the DRMS study recognized this and suggested that uplift of the hills may be related to slip on a cryptic structure or structures that have not yet been identified or imaged. The intent of defining the "Montezuma hills areal zone" is to model the possibility these structures exist, including uncertainty about whether they exist, for the probabilistic seismic hazard analysis input into the DRMS risk analysis. Unfortunately, the documentation for the DRMS study is difficult to find (and getting harder with the passage of time). Here's a link to the DRMS seismic hazard analysis; see p. 51 of the pdf for a terse summary of the MH areal zone source characterization:

https://www.waterboards.ca.gov/waterrights/water_issues/programs/bay_delta/california_waterfix/exhibits/docs/dd_jardins/DDJ-157_DRMS_Sec6.pdf

25) Line 790+, "Case Study 2": This section circles around multiple points without landing on any conclusively. The authors recognize that late Cenozoic structural and topographic relief in the Delta are less than in the western San Joaquin Valley to the south (at Coalinga and in the Kettleman Hills) and in the southwestern Sacramento

Valley (English Hills, Rumsey Hills). The authors suggest this difference is due to the presence of the Paleogene Rio Vista basin structures, and a "leftward step" in the Great Valley fault system around Mount Diablo (?); however, this model is not explained in enough detail for me to understand it. These observations all suggest that Quaternary deformation rates in the Delta are lower than along other reaches of the Great Valley fault zone to the north and south. Why is this so? Is it unique to the Delta? Do deformation rates vary elsewhere along the western edge of the Central Valley? What do the new GPS deformation models for the 2023 NSHM updates show? In line 897, the authors throw up their hands: "How the Great Valley Fault System and Pittsburg-Kirby Hills faults interact, and how and whether the partitioned strain is directed onto structures within the Delta, remains enigmatic, even on the scale of individual faults." This is a very unsatisfactory takeaway from what is presented as a "case study".

26) Line 930+, Conclusions: The text states "it is overly simplistic to treat faults within the Delta as subvertical faults accommodating dextral slip", but it's not clear who or what this criticism is addressed to. The previous work cited in the paper does not make this assumption, and existing seismic hazard models (DRMS, MWD, UCERF3) do not make this assumption either. The authors reiterate their claim that faults in the Delta accommodate 5 mm/yr of dextral shear and 3 mm/yr of "boundary normal" (presumably NE-directed) shortening, but this assertion is not convincing because they do not offer a specific model for distributing this motion on known faults, nor do they reference the most current GPS data and modeling to support it.

Comments on Figures

Fig 1. "Thornton arch" is misspelled. What is the dotted line south and west of Mount Diablo supposed to represent?

Fig. 2. Why not combine these two maps into a single figure?

Fig. 3. See previous comments on technical problems with this figure.

Fig. 4. This figure focuses on structure along the southwestern margin of the Sac Valley north of the Delta, AND it is basically a repeat of Fig 1 in Trexler et al. (2022). Does it really add anything to the stated focus on structures in the Delta for this paper?

Fig. 6. This figure is basically repeating Fig. 2 in Trexler et al. (2022). Is it really necessary to include in this paper?

Fig 8. The maps and cross sections are so schematic that it is difficult to tell if the scales are roughly correct. Also, "Late K-early N" covers a lot of time, and it's incorrect and/or misleading to suggest that the structures in (a) all were active simultaneously (?) and continuously (?) during this entire span of time.

Summary

This paper should be extensively revised to correct technical errors, eliminate extraneous and improperly cited "overview" material, and incorporate relevant current data and research. I recommend focusing on the kinematic model for uplift and deformation of the Montezuma hills, which is an interesting new contribution of the paper. The model should be more carefully and fully explained in the text, and better illustrated with original figures, maps and cross sections as appropriate.

Comments by Reviewer 2 (Solène Antoine)

Review of the manuscript:

Quaternary-active faults and the role of inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California

Charles Trexler, Jack Willard, and Belle Philibosian - USGS Earthquake Science Center

Resume of the manuscript:

This study, written by Charles Trexler, Jack Willard, and Belle Philibosian, entitled Quaternary-active faults and the role of inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California, and submitted for publication in Tektonica consists in a review of existing evidences of faulting within the Sacramento-San Joaquin Delta which, along with some new observations made by the authors, allow to discuss the active nature of faulting within the Delta and present a structural model for the region describing the different faults geometries. Using this model, the authors discuss the role of pre-existing structures in controlling the current geometry and activity of the faults across the Delta region.

The Sacramento-San Joaquin Delta constitutes a very strategic area of California where numerous agricultural and economic assets are located, and where small earthquakes and ground deformations can have devastating impacts on the economy, at local and regional scale. Moreover, even though this area does not present a high-seismic hazard, it did experience several small to medium magnitude earthquakes in the past. A major challenge for characterizing active structures across this region lies in the fact that evidence of faulting is poor and concealed due to the low slip-rates and complex faulting geometries, as well as the active erosion and deposition context of the Delta region.

Through their work, the authors aim at improving the understanding of the tectonics and active faulting structures across the Sacramento-San Joaquin Delta, also allowing for improving the seismic hazard assessment across this region. The review work present existing sub-surface, bedrock geology, and geodetic datasets, which are completed with new surface geologic and geomorphic observations made by the authors. This work allows a detailed description of active faults across the Delta region along with a discussion on the potential the slip-rates associated with each of the faults. Authors also discuss how the current state of faulting is the result of re-activation of pre-existing faults from the pre-Neogene period, and how the current fault movements might accommodate part of the right-lateral, transpressional, Pacific-North America plate boundary displacement.

To conclude, this study represents a complete analysis of the active faulting structures across the Sacramento-San Joaquin Delta, which allows for improving (i) the understating of the tectonics and mode of faulting across this region, and (ii) of the seismic hazard assessment within this strategic area of California.

General appreciation of the manuscript:

The submitted manuscript presents a high-level description of the active faults and associated structures across the Sacramento-San Joaquin Delta regions, and is provides a large amount of detailed information, which is necessary to our understanding of the study. However, I deeply regret a lack of organization and clarity in the text, as well as a great lack of figures to illustrate the extensive descriptions. Hence, many descriptive sections of the text remain partly unclear to the reader. Indeed, there is a great need for figures that would provide context information (geologic maps especially), present the observations that are described and discussed in the text, as well as synthesis figures that would present better the main conclusions and implications of the study. Moreover, the authors should consider using tables to synthesize the observations, especially when it comes to the discussion of the active vs inactive nature of the faults, and their associated slip rates.

In addition, the authors should separate more clearly in the text their new observations from the review published observations, to allow the reader to better understand the gain in information brought by these new observations. There is also a lack of justification or explanation in the choice of the authors to describe specific structures within the Delta region. Indeed, this region is complex and presents numerous faults and associated geomorphic features, and sometimes it is not clear why the authors choose to describe some of these structures with higher level of detail.

Finally, I regret a clearer separation between the description of the observations, the derived model of active faulting across the Delta, and the discussion part. Indeed, the authors mention “a model” in the text, but it is never clearly explained what “this model” is, and the latter does not seem to be discussed regarding other existing models. I suggest that the authors consider the classical organization: 1. Descriptions of the observations (1.a. Existing observations, and 1.b. New Observations), 2. Interpretation of the observations and presentation of the derived model, and 3. Discussion of the model. To conclude, my general appreciation of the manuscript is very positive, especially regarding the content of the study, but there is a substantial need for re-organization of the text (observations, model, discussion) as well as for new figures to support the text.

Detailed review:

The detailed review of the manuscript is provided directly in the .docx file provided by the authors. The detailed review of the Figures is presented hereafter.

Figure 1:

For this figure as well as most of the author figure, there is a lack of geology information that would provide the reader a better appreciation of the structures within the region. In fact, the background image from the map does not provide much information apart from the geographic context, which is not crucial to the study. Limits of the tectonic plates and arrows with plate displacement rates could also be added to the inset figure as well. Moreover, all other figures from the study should be located on Figure 1. Finally, why do the authors indicate the Mercalli intensity for the 1892 sequence whereas a moment magnitude is indicated in the text? Is there an approximate location for the epicenter of this seismic sequence?

Figure 2:

Similar comment than for Figure 1. The geology is discussed in the text but not shown the figures. The topography map presented here does not provide crucial information for the understanding of the study. Figure 2 should also be located in Figure 1.

Figure 3:

This figure seems to arrive a little too early compared to other figures. Indeed, it presents slip rates along all the faults of the Plate boundary, which is more related to the discussion part than the observation and data part (I suggest to associated Figure 3 with Figure 8). Moreover, it is not clear faults from the Delta are associated only with a shortening rate whereas the text also discusses the occurrence of dextral slip on some of these faults.

Lower part of Figure 3 should also be explained in more details in the legend as it is not clear to understand at first. Implications of the Figure should also be directly indicated within the figure, eventually using arrows that show the residual slip rate across the plate boundary after cumulating slip along all the faults. Moreover, across the 3 sections presented, it seems that cumulative slip rates across all faults from the plate boundary are greater than the tectonic slip rate, which implies that activity along the Delta faults is not necessary to accommodate the tectonic loading.

Figure 4:

Panel a: This map should provide more geologic information along with the fault geometries. Indeed, the authors discuss the role of pre-existing structures, but these are never shown to the reader. The geology is extensively discussed in the text, and thus it should also be presented in the Figures (at least the main geological units).

Panel b: The two models presented here are not much discussed in the text. The authors should indicate more clearly why these two models are important, and what are the implications for the regional-scale model (Figure 8) of each in terms of fault activity and deformation processes.

Figure 5:

Again, there is a lack of geologic information. It also seems that the two panels, a and b, could be combined together and the main points made clearer to the reader. Legend should also express more clearly what is the aim of this figure, and what are the implications of these observations.

Figure 6:

This figure is very similar to that presented in the study from Trexler et al. (2022). Authors should justify better their choice to present again this Figure. I suggest that the authors cite their previous study and leave space for new figures for this manuscript. Moreover, text size within panel a could be larger.

Figure 7:

Panel a: Legend indicates a water gap but water can be seen all along the Denverton creek. Hence, it is not clear where the water gap is. Color from panel b (wight) is not very well visible on the topographic map.

Panel b: Why is the geological map presented only for this small region and not all region from panel a? Why is the water gap visible in panel b but not in panel a? Moreover, even though the legend says that there is topographic information in both panels a and b, topography map is visible only in panel a (legend should be modified).

Panel c: It is not indicated whether this geologic cross-section is extracted from another study or if it was made by the authors. If it was made by the authors, then authors should provide more explanations on the method (e.g., how do they constrain on the tilt angle of the structures at depth?).

Figure 8:

Figure 8 represents the final synthesis figure of the paper with the structural model of the Delta region. However, the relation between this figure and the main text is not clear. A larger and dedicated paragraph should be provided in the main text to explains more clearly the different modes of deformation presented in the panels, and the implications. Moreover, this Figure could be more complex with greater structural and geologic information. For example, cross sections could be larger and present more details on the fault structures. Geologic information could also be provided, especially in the maps from panels c and d. To highlight the role of inherited structures, a color code could be used to highlight different groups of faults. Finally, Figures 3 and 8 could be presented next to each other since they both discuss the role of the Delta's faults for accommodating tectonic loading across the entire plate boundary.

Authors' Reply to Reviews

Dear Dr. Lacassin, Dr. Williams, and reviewers,

Please find a revised version of our manuscript, "Quaternary-active faults and the role if inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California," attached. We thank the reviewers for the detailed comments on our first submission, and we have undertaken significant revisions to the manuscript and figures following the comprehensive feedback we received. We have added new figures and a new table, and extensively reorganized the manuscript in an attempt to both clarify our main points and more logically guide the reader through our work, which includes both review of literature (including published literature, along with "grey literature" such as consulting reports, meeting proceedings and dissertations) and new geomorphic analyses to constrain Quaternary activity in the Delta. Some of the literature citations we have added were recommended by the reviewers, and we thank them for those important references. Other reviewer recommendations were already extensively cited within the manuscript; in these cases, we have taken care to revisit the text where those citations are found and emphasize their presence. We believe the combined result of these revisions is a much stronger and more sound manuscript.

In addition to the revised manuscript text and figures, we have provided below our responses to individual comments from reviewers, with comments from reviewer 1 in black text, comments from reviewer 2 in blue text, and our responses in green text. The comments are organized here by line number in the original manuscript. We identify the new line number for the commented text (in parentheses and italics) following the original line number (in bold). We note that because the commented text has tracked changes, and because we have undertaken a major reorganization within manuscript, the line numbering diverges significantly from the original manuscript. Reviewer comments, and our responses, are also included as in-line comment threads in the tracked changes version of the manuscript text document.

Thank you very much for considering our manuscript for publication,

Charles Trexler

Reviewer 1

The stated purpose of this paper is to offer a "comprehensive overview of current understanding of geologic structures in the Delta" and improve seismic source characterization. The manuscript requires significant revision to successfully achieve these goals.

The paper doesn't make a case for why a "comprehensive overview" is needed. Many of the key references cited in the paper already provide "comprehensive overviews" (e.g., Weber-Band, 1998; Krug et al., 1992; Unruh et al., 2015; Unruh, 2021; etc.). Despite the intent to provide an authoritative summary of previous work, there are numerous errors and misrepresentations of the cited literature in the manuscript, and some key references are not included. Many of the figures are reproduced from previous work with little/no change (including two figures reproduced from Trexler et al. 2022). The paper would be better served by a focused summary of relevant previous work that bears on the kinematic model for the Pittsburg-Kirby Hills fault and Montezuma hills, rather than attempting something more grandiose. The manuscript also presents a relatively lengthy summary of the Mesozoic and Tertiary development of the western California convergent margin, going into details that are covered much better elsewhere and not directly relevant to the main topics of the paper. This material should be significantly trimmed to reduce the length of the paper and improve the overall focus.

The most significant technical deficiency in this paper is the analysis of geologic and geodetic deformation rates. Rates drive everything in seismic hazard analysis, so it is very important to get them right, especially if the intent of the paper is to improve seismic source characterization in the Delta. The authors present transects of deformation rate across the Pacific-Sierran plate boundary that rely on ~20-year-old geodetic studies to support an interpretation that faults in the Delta accommodate 5 mm/yr of dextral shear and 3 mm/yr of "boundary normal" (presumably NE-directed) shortening. As shown in Figure 3 in the paper, these rates overpredict the total integrated Pacific-Sierran motion at the latitude of the Delta, which is a red flag that something is wrong. The deformation rates utilized and preferred by the authors are not consistent with well-documented observations of modest late Cenozoic structural and topographic relief in the Delta, and the authors do not present a testable model for distribution of their preferred rates on known Quaternary structures. In addition to addressing technical errors in the transects (discussed in detailed comments below), I recommend that the authors review geologic fault slip rates compiled for the 2013 UCERF3 statewide seismic hazard model and use them to recalculate the transects. I also strongly recommend that the authors review the new geodetic deformation models developed for the 2023 update of the National Seismic Hazard Map (Zeng, 2022; Evans, 2022; Pollitz, 2022; Shen and Bird, 2022), which use the most recent compilation of the GPS velocity field and modern modeling methods to estimate fault slip rates at the latitude of the Delta. The authors should acknowledge this relevant recent work and address any discrepancies or disagreements with their preferred model.

Reviewer 2

Review of the manuscript:

Quaternary-active faults and the role of inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California

Charles Trexler, Jack Willard, and Belle Philibosian - USGS Earthquake Science Center

Resume of the manuscript:

This study, written by Charles Trexler, Jack Willard, and Belle Philibosian, entitled Quaternary-active faults and the role of inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California, and submitted for publication in Tektonica consists in a review of existing evidences of faulting within the Sacramento-San Joaquin Delta which, along with some new observations made by the authors, allow to discuss the active nature of faulting within the Delta and present a structural model for the region describing the different faults geometries. Using this model, the authors discuss the role of pre-existing structures in controlling the current geometry and activity of the faults across the Delta region.

The Sacramento-San Joaquin Delta constitutes a very strategic area of California where numerous agricultural and economic assets are located, and where small earthquakes and ground deformations can have devastating impacts on the economy, at local and regional scale. Moreover, even though this area does not present a high-seismic hazard, it did experience several small to medium magnitude earthquakes in the past. A major challenge for characterizing active structures across this region lies in the fact that evidence of faulting is poor and concealed due to the low slip-rates and complex faulting geometries, as well as the active erosion and deposition context of the Delta region.

Through their work, the authors aim at improving the understanding of the tectonics and active faulting structures across the Sacramento-San Joaquin Delta, also allowing for improving the seismic hazard assessment across this region. The review work present existing sub-surface, bedrock geology, and geodetic datasets, which are completed with new surface geologic and geomorphic observations made by the authors. This work allows a detailed description of active faults across the Delta region along with a discussion on the potential the slip-rates associated with each of the faults. Authors also discuss how the current state of faulting is the result of re-activation of pre-existing faults from the pre-Neogene period, and how the current fault movements might accommodate part of the right-lateral, transpressional, Pacific-North America plate boundary displacement.

To conclude, this study represents a complete analysis of the active faulting structures across the Sacramento-San Joaquin Delta, which allows for improving (i) the understating of the tectonics and mode of faulting across this region, and (ii) of the seismic hazard assessment within this strategic area of California.

General appreciation of the manuscript:

The submitted manuscript presents a high-level description of the active faults and associated structures across the Sacramento-San Joaquin Delta regions, and is provides a large amount of detailed information, which is necessary to our understanding of the study. However, I deeply regret a lack of organization and clarity in the text, as well as a great lack of figures to illustrate the extensive descriptions. Hence, many descriptive sections of the text remain partly unclear to the reader. Indeed, there is a great need for figures that would provide context information (geologic maps especially), present the observations that are described and discussed in the text, as well as synthesis figures that would present better the main conclusions and implications of the study. Moreover, the authors should consider using tables to synthesize the observations, especially when it comes to the discussion of the active vs inactive nature of the faults, and their associated slip rates.

In addition, the authors should separate more clearly in the text their new observations from the review published observations, to allow the reader to better understand the gain in information brought by these new observations. There is also a lack of justification or explanation in the choice of the authors to describe specific structures within the Delta region. Indeed, this region is complex and presents numerous faults and associated geomorphic features, and sometimes it is not clear why the authors choose to describe some of these structures with higher level of detail.

Finally, I regret a clearer separation between the description of the observations, the derived model of active faulting across the Delta, and the discussion part. Indeed, the authors mention “a model” in the text, but it is never clearly explained what “this model” is, and the latter does not seem to be discussed regarding other existing models. I suggest that the authors consider the classical organization: 1. Descriptions of the observations (1.a. Existing observations, and 1.b. New Observations), 2. Interpretation of the observations and presentation of the derived model, and 3. Discussion of the model. To conclude, my general appreciation of the manuscript is very positive, especially regarding the content of the study, but there is a substantial need for re-organization of the text (observations, model, discussion) as well as for new figures to support the text.

Summary

This paper should be extensively revised to correct technical errors, eliminate extraneous and improperly cited “overview” material, and incorporate relevant current data and research. I recommend focusing on the kinematic model for uplift and deformation of the Montezuma hills, which is an interesting new contribution of the paper. The model should be more carefully and fully explained in the text, and better illustrated with original figures, maps and cross sections as appropriate.

Detailed comments:

Line 13 (Line 12) The abstract should be completely re-written to focus on the key findings of the paper. Keep it short and punchy. The first paragraph of the abstract reads like an introduction to the main text. It is completely unnecessary and should be cut.

At the reviewer's suggestion, we have cut the majority of the first paragraph and streamlined the remaining text to simplify and shorten the abstract.

Line 13. *(Line 12).* Abstract is too long and lacks clarity. There are too many elements of context (which more likely belong to the introduction) and major conclusions should be emphasized.

At the reviewer's suggestion, we have cut the majority of the first paragraph and streamlined the remaining text to simplify and shorten the abstract. In addition, we have added specific references to the structures in the Delta that we believe are the primary active structures based upon our geomorphology analysis.

Line 42. *(Line 28)* The introduction is well written but authors should make a better connection with the Figures (especially Fig. 1, 2, et 3). Suggestions on Figures are also made in the .pdf file.

We have added citations to Figures 1, 2, and 3 and Table 1 to the introduction.

Line 82. *(Line 107)* This part lacks a Figure presenting the geological context. For example, the authors could consider adding a geological map representing the same area as Figure.1, or adding geological information in the background of Figure 1 instead of the geography map that does not bring much information (see detailed comments in the .pdf file). Author could also consider a Figure describing the evolution of the tectonic history from Pre-Neogene to present (e.g., timeline figure), which would then provide a context for the model from Figure 8.

At the reviewer's request, we have added a regional-scale geologic basemap (Jennings et al., 2010) to Figure 1.

Line 83. *(Line 108)* Authors should indicate the dates along with citations of geological periods in the text.

Added "23 Ma" to this and the following section headings. Added citation to GSA time scale (Walker and Geissman, 2022)

Line 146-147. (*Line 273)* Text states "A similar Paleogene-age extensional basin is exposed within Great Valley Group strata on the northern flanks of Mount Diablo…". It's not a "similar basin"; it's the exact same structure, as recognized by MacKevett (1992).

Changed "A similar…" to "Structures bounding this…" and added reference to MacKevett (1992).

Line 169. *(Line 296)* Location of Figure 2 is not clear and should be shown in Figure 1. Authors should also consider discussing in more details the information from the Figure (patterns of seismicity etc) as well as providing more geological information (background from the Figure does not provide much information).

Also, Figure 2 seems too specific compared to Figures 1 and 3 that present the global context. Hence, this Figure seems little misplaced.

We have added the footprint of Figure 2 to Figure 1.

Line 170: *(Line 164)* text refers to the "Coast Ranges block". The (over)use of "block" to describe areas of similar geology should be avoided if it is not a kinematically appropriate description. The northern Coast Ranges currently are accommodating ~36 mm/yr of distributed NW dextral shear across their entire width, which is not very blocklike behavior.

We have removed the word "block" from this sentence following "Coast Ranges" and "Sierra Nevada."

Line 172. *(Line 299)* This Figure is an important Figure and should (i) comes earlier in the text, and (ii) be more described and discussed.

The revised organization of this manuscript presents the geodetic modeling in one, more comprehensive, section. We do not feel as though it makes sense to put this section any earlier in the manuscript than it currently resides, as it is in the first section following the geologic background.

Line 177. *(Line 304)* Starting from this section, it is not clear which observations were made by the authors and which come from existing studies. As described in my general comments, the authors should separate more clearly the different sections. Moreover, this section could be illustrated by a table providing a synthesis of the active faults and associated slip rates within the Delta (different from Figure 3 that presents all faults from the plate boundary).

It is our intent that this comment has been addressed by the reorganization of the manuscript.

(Line 304) Authors should define the concept of active faults. What are the different definitions (if there exist different ones), and which one they choose to use. What arguments allow to define a fault as active/inactive?

We have added a short paragraph at the beginning of section 2 that clarifies what we mean by 'active faults.'

(Line 304) Authors could maybe provide an introduction sentence to this section quoting the number of active faults and the large groups, before entering in the detailed description of each fault.

This comment played a part in our decision to significantly reorganize the manuscript, with a framework around geographic context rather than method/data type. We hope that reorganization helps address the concerns of the reviewer here.

In addition to the larger reorganization, we have added a short introduction to this section, and brief geographic summaries of faults within each subregion at the top of those sections (see section 3.2)

Line 178. ***(Line 398)*** Authors should introduce better why the GVFS is such an important feature of the Delta, as it is described extensively using text and Figures, and emphasize its location in Figure 1.

The new organizational scheme we have implemented includes new regional introduction paragraphs, which we intend to more clearly set up the context for the major structures we focus on, including the GVFS.

Line 180. ***(Line 405)*** Authors should provide a Figure to illustrate this sentence. For example, geological information could be added to Figure 4.

We have added a geologic map as a baselayer on Figure 1, and added a citation for it here.

Line 184. ***(Line 409)*** This region is not clearly indicated in the maps.

With the addition of a geologic map as a base layer on Figure 1, we hope this boundary and the associated "CRSB" is more apparent. We have elected not to label this feature on the figure directly, to maintain clarity on what is already a dense figure.

Line 190. ***(Line 400)*** Figure 4 should be located on Figure 1, along with other Figures. This would allow a better understanding of the spatial organization and scales of the structures.

We have added the footprint of figure 4 to Figure 1b.

Line 191. ***(Line 415)*** What is the surface expression of the fault then? This is answered in the next paragraph, but it is separated by another paragraph describing the geometry of the GVFS. I suggest that the authors move the following paragraph to another place (before or after).

We believe that in its current context and with additional details added here and elsewhere in this section, this question is more clearly answered.

Line 191. ***(Line 428)*** This section should be places before or after, but here, it separated two sections that should follow one another (see my comment above). Figure 4a should also be cited in the paragraph.

We have added a citation to Figure 4a here.

We believe that the manuscript reorganization has placed this paragraph in a more logical context than its previous location.

Line 195: ***(Line 420)*** Text states that the Midway fault is part of the Great Valley (thrust) fault system, but this structure is likely a strike-slip fault, not a thrust or reverse fault.

Lacking a specific reference for the Midway fault (which, as the reviewer notes, is not particularly well understood), we have removed the reference to it from this sentence.

Line 205. ***(Line 447)*** Authors should cite more clearly the different panels of Figure 4a (different models) along with the corresponding model description in the text.

We have added several references to Figure 4 in this paragraph to more clearly link the text to that figure.

Line 208: ***(Line 449)*** "Guzofski" is not spelled properly. Regarding the bibliography in general, many of the citations are erroneous or incomplete. I am surprised that the paper was submitted in this condition, and that the editors sent it out for review without asking that the authors fix the references first.

We have corrected the misspelling of this reference, and appreciate the reviewer for catching the error.

We have also revisited the references section, and checked for completeness, and revised errors that we identified.

Line 221. ***(Line 478)*** Cite Figure 1.

Citation of Figure 1 added.

Line 222. ***(Line 479)*** Cite Fig. 4b

Citation of figure 4b moved to precede citation at the end of the sentence, so that it is more visible. We also now specifically refer to "model 2" in the figure.

Line 235: ***(Line 898)*** Text cites Johnson (1992) to the effect that the Midland fault extends from the city of Davis south to the town of Byron. "Johnson 1992" is one of the incomplete citations in the bibliography, so I couldn't look it up to make my own assessment of this mapping. In any event, mapping of the Midland fault in California Division of Oil, Gas and Geothermal Resources (DOGGR) gas field data sheets shows that the structure breaks up into a series of right-stepping en echelon splays at the latitude of the Lindsay Slough gas field at the north end of the Montezuma Hills, and the splays are interpreted to continue northward through the Maine Prairie and Bunker gas fields in the northwestern Delta. The Midland fault is not mapped in the Millar West or East Dixon gas fields south of I-80, so it's not clear what Johnson (1992) is mapping as the northern continuation of the Midland fault. In any event, the trend of the fault, if it continues north of I-80, is toward the Winters gas field, not the city of Davis. I strongly recommend that the authors review (and cite) other work on the Midland fault (particularly in the AAPG and SEPM gray literature) rather than relying exclusively on Johnson (1992).

Johnson (1992) is indeed from the AAPG and SEPM grey literature that the reviewer suggests we cite.

The maps that Johnson provides end at the latitude of Dixon, but the Midland Fault appears to continue off the northern edge of their maps, which resulted in our original statement that the fault continues to near the latitude of Davis. However, we admit that this is perhaps a bit too interpretive, and we have modified to refer to Dixon instead, as this is the northern limit of the documentation provided by Johnson (1992).

We have made sure to complete the citation information for Johnson (1992) in the reference list.

Line 240: ***(Line 903)*** Text states that the Midland fault initiated in late Cretaceous. This is not correct. Restoration of Paleogene slip in the Rio Vista basin restores all separation of the Cretaceous section across the Midland fault. There is no evidence for structural growth in the upper Cretaceous section.

With due respect to this reviewer, we have not been able to verify this comment with a citable source. Johnson (1992) specifically cites syndepositional thickening of Upper Cretaceous strata across the Midland Fault; we have clarified that point here in defense of our original text. If the reviewer can point us toward a source to defend their claim and refute the one we cite, we will incorporate that as well.

Line 241. ***(Line 905)*** Authors should provide one sentence explaining the arguments leading to this conclusion.

We have added text to this sentence to clarify the source of this interpretation.

Line 248. ***(Line 913)*** Authors should describe the arguments used to classify active vs inactive faults. This could be introduced at the beginning of section 3.

We have added a definition paragraph to the top of this section

Line 250: ***(Line 914)*** "Marsh Creek fault" is misspelled. For the record, there is no positive evidence that the Marsh Creek fault is a Q-active structure. To date, the fault has not been trenched to demonstrate activity or non-activity, and there are no documented field relations that indicate Q activity.

Misspelling corrected. We agree with the clarification that there is no positive evidence of Q activity of the Marsh Creek, and have updated the text to avoid unintentionally implying otherwise.

Line 251: ***(Line 915)*** Text states that the "Midland fault has no Neogene offset south of the West Tracy fault." This is because the exposed trace of the Midland fault here is folded in the forelimb of the anticline in the hanging wall of the West Tracy fault.

This may very well be the case in terms of fault geometry, but we do not necessarily agree with the added interpretation with regards to activity. We have elected not to add this detail here, for two reasons: first, it is somewhat tangential to the discussion of fault activity, and second, we have not identified a reference that defends this point.

Line 281+: ***(Line 972)*** Text asserts or implies that the "Montezuma Hills fault" and Denverton Creek fault system are part of the Q-active Pittsburg-Kirby hills fault zone. This is just a speculative guess on the part of the authors, not a well-documented relationship (at least not that I'm aware of). These Paleogene structures are several km east of the Pittsburg-Kirby Hills fault, and to date there is no positive evidence (that I am aware of or that the authors cite) the faults have been reactivated in late Cenozoic time.

We have added qualifying statements to this paragraph to clarify that the observations are primarily bedrock/subsurface in nature, and that the apparent consistency with Quaternary kinematics is compatible but not necessarily implied by these studies.

One minor tangential note here: the fault in question here is actually referred to as the Montezuma Fault in these references, not the Montezuma Hills Fault. We have corrected this in the text.

Line 292. ***(Line 997)*** Authors should discuss the uncertainty on the seismicity as it is very diffuse in the region and not well correlated with the fault trace. Is this seismicity occurring along the fault or within the surrounding region on smaller and unmapped faults?

We have added a clarifying point to the text, stating as the reviewer notes that the seismicity is not easily correlated with the surface trace of any particular fault.

Line 294: ***(Line 999)*** Text cites multiple refs to the effect that the Pittsburg-Kirby Hills fault merges with the Kirker fault south of the river. This is an inaccurate representation of the literature. The Kirker fault is interpreted to the southern continuation of the Paleogene Kirby Hills fault zone and the ancestral western margin of Rio Vista basin (MacKevett, 1992). The Pittsburg-Kirby Hills fault is a Q-active structure that locally reactivates pieces of this fault system, primarily north of the river where the structures are all still upright and have not been tilted NE on the backlimb of Mount Diablo anticline. No one I'm aware of has argued or claimed that the Q active Pittsburg-Kirby Hills fault extends all the way south to the surface trace of the Kirker fault.

We appreciate this reframing and clarifying point, and have added it to the text.

Line 303. ***(Line 1072)*** This is an important conclusion. Authors should discuss more this conclusion and provide more data/figures to support it.

This is, as we see it, perhaps the primary focus of this manuscript, and as such we suggest that the whole of the manuscript and figures are in someways data and figures to support this conclusion.

We have undertaken a significant reorganization of the manuscript, which we hope makes this point a clear and fundamental piece of this manuscript.

Line 306. ***(Line 1138)*** Most of the faults described hereafter are considered inactive in the QFFD (USGS & CGS, 2022). Hence, could the authors indicate more clearly why they consider these faults as active? This section also lacks illustration with a Figure.

We have added a reference to Figure 2.

We believe that the larger scale reorganization and refined definitions of "activity" elsewhere in the manuscript should address the first part of this comment.

Line 307. ***(Line 1139)*** Location of the Sacramento River is not clear in the different maps.

We have added a label on the Sacramento River on Figure 2 and Figure 8, both of which also show faults. We have also added a reference to Figure 2 at an appropriate spot later in the paragraph.

Line 320: ***(Line 1151)*** text states that there is 2 km dextral offset of an 8 Ma magnetic anomaly across the Sherman Island fault, and a 200 m offset of the same anomaly across the Davis fault, citing "Unruh 2007" and Krug et al. 1992. There are several problems here. First, the text should specify what geologic feature is associated with the "8 Ma magnetic anomaly". Is this the Miocene Neroly Fm? (the Neroly is loaded with magnetite and makes prominent aeromag anomalies). If so, then are comparable offsets of the Neroly contacts mapped across the faults consistent with this interpretation of the aeromag? If the Neroly Fm is not the source of the anomaly, then what is the anomaly associated with and why do you believe it resolves 200 m of dextral displacement on the Davis fault? Second, the text cites "Unruh 2007" as a source for this relationship, but there is no "Unruh 2007" listed in the bibliography, and as far as I know Unruh has never discussed this aeromag anomaly in any publication. I would be surprised if Krug et al. 1992 discuss it, because the focus of that paper is Paleogene structure north of the river. Finally, the Sherman Island, Antioch, Davis, etc. faults are antithetic structures to the Midland fault that splay to the northwest and create secondary graben and depocenters in the Rio Vista basin. Please review structure maps of Delta gas fields published by DOGGR, and the work of Krug et al. (1992), to see how the faults are distinguished spatially.

We appreciate the suggestion to revisit how we have described structural relationships here, since we had unintentionally spread these descriptions across several sentences. We have reworded this sentence, and brought in information from other sentences, to clarify the structural geometry in one place.

Upon revisiting our references, we too were unable to reproduce the source of this apparent displacement in stratigraphy; as such, we have removed that section of the sentence from the manuscript.

Line 344+: ***(Line 1174)*** text states that the Clayton and Marsh Creek faults are considered "the northern extension of the Greenville fault system." Considered by who? There is no citation for this assertion in the paper, and as far as I know there is no consensus around this issue in the local Bay Area seismotectonic community. I reiterate that there are no field observations of faulted Q deposits along these structures. There have been no paleoseismic trench investigations of the Marsh Creek fault to evaluate activity or non-activity. The Clayton fault was extensively trenched in the 1980's and 1990's during construction of residential housing in and around the town of Clayton, and according to Jim Joyce (engineering geologist who did the trenching for the developers) no positive evidence of Q activity was observed.

This relationship is described by Medwedeff (2021), likely among others, and we have added that citation here. We have also modified the structure of the sentence to clarify that this is not necessarily a consensus view, and added detail to clarify that Q activity is unconstrained.

Line 360. ***(Line 525)*** What is the definition of a "seismic source zone"? How is it defined?

Definition of "seismic source zone" has been added at the top of section 3.2

Line 369. ***(Line 530)*** The location of these hills should be indicated in the Figures, and the latter cited.

Plainfield Ridge is now documented in Figure 6, and cited here.

Line 375. ***(Line 537)*** It is not clear what the authors consider as “the latter model”. They should explain clearly what is this model, and where it comes from, as well as add a figure citation.

In this paragraph's current context, we have elected to remove this phrase from the sentence entirely.

Line 384. ***(Line 1082)*** These observations could be emphasized using figures.

The geomorphology of the Montezuma Hills is thoroughly addressed with text and figures later in the manuscript. Because the dataset we present is largely geomorphic in nature, we prefer to keep the figure and results together in the manuscript, and so keep it in section 4.

Line 387. ***(Line 1085)*** Figure or paper citations?

We have restructured this paragraph so as not to lead with a sentence that does not contain citations.

Line 390. ***(Line 1087)*** What are the evidence of faulting?

These faults are clear in bedrock subsurface datasets. We have added information to this sentence to clarify this.

Line 391. ***(Line 1089)*** This structure is not clearly indicated in the Figure.

A reference to figure 2 is included in the following sentence.

Line 394. ***(Line 1091)*** These structures should be indicated in the Figure.

The Sherman Island Fault System is on Figure 2, and a reference to it has been added earlier in this sentence.

Line 397. ***(Line 1094)*** Figure 5b should be cited in this section.

Figure 5b has been updated and the revised version no longer includes these faults

Line 399. ***(Line 799)*** Similarly to other sections, figure citations should be added at the beginning to provide geographical context within the maps.

Citation to Figure 1 added.

Line 415. ***(Line 540)*** The previous section was also discussing evidence of Quaternary fault activity. Hence, this title is little confusing.

We have removed the direct reference to Quaternary activity from this heading. In addition, the reorganization of the manuscript should help to reduce confusion about what material is covered in each section.

Line 416. ***(Line 309)*** Similarly, previous paragraphs were also bases on a review of existing data. Hence, it is hard to understand the difference between section 3 and section 4.

We believe that the significant manuscript reorganization addresses this comment, and reduces redundancy throughout the manuscript.

Line 417. ***(Line 100)*** Authors should describe the different definitions and provide citations. Moreover, such sentence should be placed earlier in the text, maybe in introduction.

More detailed explanation of definitions of “active” has been added, along with citations. This text now leads the “background” section (section 2)

Line 420. ***(Line 813)*** Authors should provide a table with the slip rate associated with each fault and, maybe, the type of observation (this can include geodetic slip rates as well, which would also allow for an easier comparison of both datasets).

We have added a Table 1 to the manuscript, which reports both geologic rates and geodetic model rates from the 2023 update to the National Seismic Hazard Model

Line 421. ***(Line 305)*** This sentence is similar to some parts of the introduction.

While we see the point that the reviewer makes here, we think that this sentence provides important context for the limited number of slip rate studies and the diversity of datasets we draw upon. In its new context (line ~194, rather than 421) we hope that this sentence makes more sense structurally and feels less repetitious.

Line 427: ***(Line 540)*** text states that the Green Valley fault "runs N-S along the western margin of the Delta". Strictly speaking, it runs along the western margin of Suisun Bay, which is not generally considered to be part of the Delta proper.

The paragraph referring to the Green Valley Fault has been cut, as the reviewer has correctly pointed out that this structure falls outside the Delta proper.

(Line 540) Creeping is evidences from GNSS and not Paleoseisimc studies (Lienkaemper et al., 2013b).

The text to which this comment refers has been cut from the revised manuscript.

Line 432+: ***(Line 813)*** Citing Lienkaemper (2013) as the main source of data on the 1980 Livermore earthquake is a bit lazy given that there are plenty of primary sources.

The text to which this comment refers has been cut from the revised version of the manuscript

Line 434*. (Line 813)* Authors should indicate the mechanisms of the past earthquakes to highlights the mode of active deformation across the region.

The text to which this comment refers has been cut from the revised version of the manuscript

Line 448. ***(Line 328)*** Authors could consider the following title: "geodetic slip rates".

We have renamed the section as suggested. In addition, we have added a table with geologic rates and geodetic rates, incorporating new geodetic models from NSHM23

Moreover, the use of a table, including also data from section 4.2, would allow a better understanding ot eh different slip rates values and their implications.

Line 452. ***(Line 333)*** How does this value compare to the uncertainty on the slip rates at first order?

Uncertainties are included in Table 1, which is now cited here.

Line 455. (***Line 337)*** Authors should define more clearly the components of deformation associated with each cited rate (dextral or compressional).

Components/kinematics are included in Table 1, which is now cited here.

Line 456: Text states that "up to 5 mm/yr of dextral displacement may be accommodated by structures within the Delta." Please stop and do a gut check on this number. The GPS velocity field shows a smoothly decreasing velocity gradient across the Coast Ranges and Diablo Range. A rate of 5 mm/yr suggests that distributed shear through the Delta is comparable to the slip rate on the northern Calaveras fault, which well to the west of the Delta (on the opposite side of the Diablo Range!). The 5 mm/yr suggested for the Delta is probably twice the slip rate on the Greenville fault, and thus is contrary to the observation of eastward-decreasing rates and a smooth gradient. If the rates are truly this high in the Delta, then why isn't there a big strain rate anomaly at the latitude of the Delta in the GPS data? Where is all this displacement hiding? A major part of the problem here is in the slip-rate bookkeeping. The south transect across the plate boundary in Figure 3 overpredicts the integrated plate boundary motion, and there are SERIOUS problems with the slip rates used in the transect (slip rate on the Hayward fault is closer to 9 mm/yr; both the Concord and Greenville faults are included when they are actually the same structure at different latitudes; the slip rate on the Greenville fault is too high; etc.). The paper cites Prescott (2001) and d'Alessio (2005) as sources of GPS data and slip rates for the transect, but these papers are ~20 years old! Geologic slip rates on major faults crossed by the transects were comprehensively reviewed and vetted for the 2013 UCERF3 statewide EQ hazard model. The authors should carefully review the UCERF3 literature (especially Appendix B by Dawson and Weldon) and compare the geologic slip rates against the values in the transects in Figure 3. Additionally, four new deformation models of GPS data for the 2023 National Seismic Hazard Map were published in late 2022 (Zeng, 2022; Evans, 2022; Pollitz, 2022; Shen and Bird, 2022). What do these more current slip-rate models based on GPS data suggest for structures in the Delta? What does the range of rates estimated by the four models suggest about uncertainty? As currently presented, the "5 mm/yr" rate of dextral strike-slip through the Delta proposed by the authors is simply not credible.

Upon revisiting this section, we agree with the reviewer that it is overly simplistic, and have removed it from the manuscript.

Line 456. *(Line 339)* This citation should be fore Figure3? It is not clear why Figure 2 is cited here.

Figure 2 includes geodetic vectors, so we think that citing it here is relevant. However, Figure 3 should also be cited, and we have added it (as well as Table 1) here.

Line 460. *(Line 254)* What are the uncertainties on the different values of slip rates?

These uncertainty values are reported in Table 1.

Line 463. *(Line 253)* What are the implications of this observation?

As noted elsewhere, we recognize that this section was perhaps overly simplistic, and we have cut it (and the associated portion of figure 3) from the revised manuscript.

Line 467. *(Line 253)* In this case, the tectonic rate fits with the modeled fault slip rates across San Andreas + Rodgers Creek + West Napa + Green Valley, and there is no need to accommodate deformation across the Delta.

As noted elsewhere, we recognize that this section was perhaps overly simplistic, and we have cut it (and the associated portion of figure 3) from the revised manuscript.

Line 469. *(Line 254)* Authors should provide quantitative values of the uncertainty estmates.

These uncertainty values are reported in Table 1.

Line 481+: (Line 254) The plate motion model of DeMets and Merkouriev (2016) predicts that the relative motion between the Pacific plate and Sierran microplate at the latitude of Rio Vista is 36 mm/yr of dextral shear directed toward N29°W. Before you can decompose this into a component of "plate boundary-normal compression", you must first specify what the orientation of "the boundary" is relative to the N29°W plate motion vector. What, and where, is the Pacific plate-Sierran microplate boundary? I would argue it's a broad zone rather than a discrete plane, and thus an appropriate approach to evaluate the presence or absence of "plate-boundary-normal compression" is to do a vector summation of the slip rates of major faults along an E-W transect at the latitude of the Delta (similar to the approach of Kelson et al., 1992), and then look at the residual vector motion east of the Greenville and Concord faults. This way you avoid the problem of the north transect in Figure 3, where any shortening at a high angle to Pa-SN motion along the western margin of the Sac Valley is not accounted for. Note that there are significant differences in the strike of the San Andreas system dextral faults, so they should be treated as vectors rather than simply summing scalar slip rates as in Figure 3. In any event the authors need to specify how they are decomposing Pa-SN motion--including how they determined “the” orientation of the plate boundary--before we can assess what the magnitude of “boundary-normal compression” is.

A full-scale reevaluation of the diffuse plate boundary through geodetic datasets is not within the intended scope of this manuscript, and as best we can tell, the reviewer has read far more into our description of shortening as ‘plate-boundary-normal’ than we intended. We simply meant to raise the topic of east-west convergence that is suggested by some geodetic models, and the associated suggestion that this may be accommodated via dip-slip shortening on roughly NW-SE oriented faults.

To avoid accidental implications, we have reworded this paragraph to remove the phrase ‘plate-boundary-normal’ and related phrases, and instead describe deformation as “dip-slip” and/or “approximately E-W directed” as appropriate.

We also agree upon further reflection that the summed velocities (including geodetic velocities) included in a prior version of figure 3 were overly simplistic. We have removed these from the revised version of the figure.

Line 491+: *(Line 272)* I strongly, strongly recommend that the authors review the new GPS deformation models for the 2023 National Seismic Hazard Map and consider revising this section, which depends heavily on older references.

The geodetic models from NSHM23 are now reported in Table 1, and cited here. The values reported in these new models are not dramatically different from those reported in the earlier studies also cited here, and we do not feel the need to make any significant revisions here.

Line 500. *(Line 342)* Is there an uncertainty associated with this value?

Burgmann, 2008 does not report an uncertainty associated with this value. We have added ‘~’ before the 4 to accurately capture the way the number is reported in the reference.

Line 504. *(Line 347)* This title seems too wide. Authors should consider introducing directly in the title the fact that they will talk about historic and modern seismicity.

We have changed the title to ‘Modern and Historic Seismicity,’ as requested.

Line 505. *(Line 498)* What is the mechanism of the earthquake?

This earthquake sequence was in 1892, and thus predates any instrumentation.

No changes made here.

(Line 499) How is the magnitude of this earthquake estimated? Is there an approximate epicenter?

We have added a detail to the following sentence clarifying that magnitude estimates are inverted from shaking reports. We have also clarified the inferred source structure later in the paragraph.

Line 510. *(Line 505)* It is not clear here why there is a need for a modern analogue. Authors should indicate more clearly the problematic associated with the 1892 earthquake event characterization.

We are not quite sure what changes the reviewer is hoping we will implement here. In our eyes, a historic but pre-instrumentation earthquake sequence is one that is ripe for amodern, instrumented analogue. We have clarified that magnitude estimates are from reports of shaking and that the Winters sequence was likely blind, both of which highlight significant questions about this event and its tectonic setting.

Line 514: (*Line 509)* The authors cite Guzofski et al. (2007) as interpreting that the 1983 Coalinga earthquake “occurred on a west-vergent backthrust above an east-vergent fault at depth”. This is NOT the conclusion of Guzofski et al., and it is NOT the conclusion of seismologists who evaluated waveforms of the mainshock. See papers by Eaton, Eberhart-Phillips and Reasenberg, Choy, etc. in USGS PP-1487.

My strong recommendation is that the authors review and cite the primary seismological data sources for the Coalinga earthquake, and not derivative structural models by Guzofski et al., Namson and Davis, Wentworth and Zoback, et al. Most importantly, please cite other work ACCURATELY.

We emphasize that our intent in this sentence is not to describe the seismological data associated with this earthquake, but instead to specifically reference the “derivative structural models” the reviewer mentions. As such, we have elected to keep our reference to Guzovski et al. (2007) here.

We appreciate the reviewer noticing that our summarization may have unintentionally misrepresented the location of seismicity within the structural geometry presented by Guzovski et al (2007), and we have adjusted the sentence to more accurately represent this relationship.

Line 515. *(Line 520)* What was the mechanism of re-activation of this fault? Normal or reverse?

Thrust focal mechanism; we have added that detail here.

Line 521. *(Line 350)* The relation between Figures 1 and 2 is not very clear. Seismicity could be added to Figure 1, or Figure 2 better located in Figure 1.

We have added the footprint of Figure 2 to Figure 1 to clarify the spatial relationship between the two figures.

Line 527*. (Line 355)* This sentence raises a very important and centra question in the manuscript. This should be emphasized by, maybe, introducing this question earlier, or starting a new paragraph.

This is indeed an important question, and we have taken the reviewer’s suggestion to use a paragraph break to emphasize that this sentence is an important one.

The activity of the PKHF is a central focus of this paper, and we believe that this is reinforced by the new organization of the manuscript. That said, the specific question of seismicity and connection at mid-crustal levels is beyond the scope of the work we aim to address in this manuscript specifically.

Line 533. *(Line 362)* From this sentence and the following ones, it seems that seismicity at depth and at the surface might not present similar mechanisms. Can the authors comment in more details about this? Or make a clearer connection between the strike-slip and dip-slip systems. I think that this question is really central to the manuscript, but it is not enough discussed.

Given the relative lack of shallow seismicity, we don’t feel as though we can comment extensively on whether the deep seismicity documented by Parsons et al. (2002) exhibits similar kinematics to what we expect at shallower levels.

Instead, we have taken the opportunity to clarify the reviewer’s point: that there is no requirement that seismicity at these depths be directly representative of strain accommodation at shallower depths.

Line 543. This section presents a review of published data and explains the geometry of the faults across the Delta. Hence, it should come earlier in the manuscript, for example, after the introduction. At this time of the paper, one expects more discussion of the model than description of the data.

This has been address via the reorganization of the manuscript.

Line 560. *(Line 986)* What is the point of this paragraph? It is not clear what the authors are trying to tell us here, and what are the implications of these observations.

This paragraph has been moved to the section discussing the Pittsburg — Kirby Hills Fault, because it discusses potential deformation associated with the PKHF beneath the Sacramento River

Line 581. *(Line 1176)* Again, it is not clear which model the authors refer to.

Comment is no longer relevant at this location, as the text to which it refers has been deleted

Line 582. ***(Line 1685)*** It is not clear what the authors refer to. Is it the fault strike from their model? In this case there should be a figure citation.

We have added additional detail about the orientation of both the fault and the survey to this sentence.

Line 591. ***(Line 491)*** This figure is almost identical to the one presented in Trexler et al. (2022). Hence, I suggest that the authors only cite the study and leave space for new figures.

We have updated figure 6 to include additional analyses not originally reported by Trexler et al. (2022).

(Line 491) What are the implications of these observations for the model?

We are not entire sure what the reviewer is asking for here — the implications (that the fault is east-side up and surface-deforming) are already included in this sentence. We hope that additions in preceding and following paragraphs help clarify the context for this study.

The results reported by Trexler et al. (2022) do not distinguish between model 1 and model 2 of Figure 4b. We have added this detail to the text as well.

Line 595. ***(Line 560)*** Similar to my previous comment: Authors should explain clearly the implications of each observations, and how each observations contributes to building the model.

We have added clarifying details to this sentence to connect observations to interpretations.

Line 598+: ***(Line 556)*** The correct citation for the West Tracy fault seismic reflection study is Lettis Consultants International (LCI) 2022. LCI was solely responsible for scoping the study, managing the data acquisition, interpreting the data, and preparing the report on behalf of DWR.

We appreciate this clarification, and have updated the citation accordingly.

Line 604. ***(Line 1340)*** This is again a part of data review, which should be presented earlier in the manuscript. I suggest that the authors describe all structural data (geology, subsurface geophysics, geomorphology), before they introduce the seismicity and the important questions associated with the seismicity of the Delta region.

In the reorganized manuscript, the analysis of geomorphology is the primary dataset we use to make our interpretations of fault activity. Thus, the seismicity, bedrock geology, and geodesy are discussed in the background/data review, while the geomorphology is presented here in the "results" section.

Line 605. Again, this section lacks Figures to help the reader understand the spatial organization and relations between the different structures.

Line 607. ***(Line 1341)*** What is the reference for this measurement? Or is this measurement made by the authors?

We have added figure citations, as well as a citation to prior work that documents this

Line 612. ***(Line 441)*** This paragraph is confusing at this location. This should belong to the general context, explained earlier in the manuscript.

As part of the large-scale reorganization of this manuscript, the paragraph in question is now located in section 2.

Line 614: ***(Line 436)*** The text cites "apparent growth strata within Neogene units in the English Hills and Montezuma hills, including units as young as the Pliocene Tehama formation", but does not specify the nature of those growth relations. What exactly are they? Westward thinning of the late Neogene-Quaternary section? If so, precisely where has this been observed/documented? Is it above and/or below the 3.4 Ma Putah tuff? Is the timing of structural "growth" consistent with late Neogene stratigraphic and structural relations elsewhere along the western margin of the Central Valley? The text also notes that the structural growth "post-dates the northward propagation of the Mendocino triple junction." Almost everything of neotectonic interest in this region post-dates passage of the MTJ! Please note that there is a body of literature that points to changes in relative plate motion as a driver for late Neogene regional uplift and deformation of the Diablo Range and northern Coast Ranges, rather than time-transgressive passage of the MTJ. Please see Atwater and Stock for late Neogene changes in relative plate motion, and papers by Ben Page, Dave Montgomery, and Argus and Gordon (2001) for additional background on transpression and late Neogene uplift of the Coast Ranges.

This interpretation is our own, and comes from bedrock geologic maps, which is why we have cited Graymer et al (2002). We have clarified what exactly we see in the geologic mapping.

We are indeed aware of a significant body of literature discussing uplift of the Diablo Range and northern Coast Ranges. Our point here is simply that this compression postdates the pre-MTJ forearc. We have left the related text intact.

Line 618. ***(Line 1359)*** Location on the maps is not easy to find. Authors should provide a Figure citation and emphasize these structures in the figures.

Labels added on Figures 2 and 5, and citation added here

Line 621. ***(Line 1362)*** Is there a reference, or a map, associated with this statement?

Reference to Graymer (2002) added.

Line 639. ***(Line 1462)*** This paragraph seems a little misplaced. I suggest that such geomorphic description comes earlier.

The reorganization of the manuscript more clearly introduces the Montezuma Hills and their geographic context earlier in the manuscript (section 3.2.3).

We have kept the geomorphic description here, because the geomorphology analysis of the Montezuma Hills is a major component of the observations we present and interpret in this manuscript.

Line 646. ***(Line 1471)*** This Figure could be more complex and cited earlier in the text. For example, authors could add the geology in the Figure.

We have added an addition panel to the figure that provides more in-depth geomorphology analysis

Line 651. ***(Line 1476)*** Again, it is not clear which model the authors refer to.

This refers specifically to the sentence immediately prior, we have added a semi-colon and made this a single sentence to clarify that point.

Line 670. ***(Line 1354)*** The location and expression of this water gap is not clear for the reader. Authors should indicate it more clearly in the Figure, and the Figure cited (Fig. 7).

Citation to figure showing Denverton water gap added. Water gap is also now labeled in panel 7a.

Line 676. ***(Line 1346)*** This should be the red line of this manuscript; however, it seems like the authors provide extensive descriptions of the structures without always discussing clearly the implications for the active vs inactive nature of the structure.

We have made several changes to this section that we hope drive this point home more clearly:

1) we have reorganized this section of the manuscript to more clearly present our new geomorphic observations and their implications

2) we have added additional details to the text and figures describing the geomorphology

3) we have modified sentences (where appropriate) to more clearly and directly state the activity we are inferring.

These changes are consistent with other changes we have made throughout the manuscript to refine and reinforce the main point of the manuscript, which is highlighting the structures that, based on geomorphic signals, we believe are active.

Line 683. ***(Line 1351)*** Authors should link better the figures to the text (e.g. Fig. 7 here?)

These examples are visible on Figure 2 and Figure 5; and those figures have been cited here. Figure 7 is cited in the following sentence

Line 685. ***(Line 1353)*** Same comment here. Figures or references should be cited.

Citation to figure showing Denverton water gap added

Line 694. ***(Line 1764)*** It is hard to see the relation between the previous parts and the discussion since "a model" has been mentioned at several places along the text but not clearly described. It is not clear what the authors will discuss in this section.

We hope that the major structural reorganization of the manuscript that we have undertaken has addressed this concern. The geographic focus on the western Delta, presentation of existing data and introduction of new observations now are more clearly presented in the framework of the manuscript, which should more clearly set up the discussion section.

Line 695+: ***(Line 1765)*** This section starts by citing Prescott et al. (2001) and d'Alessio et al. (2005) to support the statement that "Geodetic investigations suggest as much as ~5 mm/yr of dextral displacement and ~2-3 mm/yr of shortening may be accommodated in the Delta." I strongly recommend that the authors review the new GPS deformation models for the 2023 update of the National Seismic Hazard Map before committing this to print. Again, I urge a simple gut check of these numbers: 2-3 mm/yr of shortening will drive uplift rates on the order of km/my, which is sufficient to give you topographic relief like Mount Diablo anticline. Is there anything like Mount Diablo sitting out in the middle of the Delta? There's not even anything like the Coalinga anticline or the Kettleman Hills in the Delta! There are only a couple hundred meters of structural relief on the base of the Miocene across the Montezuma hills (Unruh et al., 2015). There is not enough topographic and structural relief in this region to support long-term average shortening rates > 1 mm/yr.

We agree with the reviewer's comment that these numbers seem high; however, we note that they are supported by new geodetic models the reviewer mentions, and which we now cite here in addition to the citations we included in the earlier draft.

We have added additional text to this paragraph to more fully explore the implications of such high apparent rates on these structures.

Line 695. ***(Line 1767)*** This point is central to the study and should be more discussed rather than taken as a valid statement.

We have added text to this paragraph to more fully address this point and its implications. We also have added a citation for Table 1.

Line 698. ***(Line 1772)*** This sentence should come at the beginning of the paragraph since it shows the transition from the data analysis (previously), and the following discussion.

We have added a sentence at the beginning of this paragraph that we intend to serve this transition role.

Line 700. ***(Line 1774)*** These sentences are repetitive with the introduction.

After implementing changes in response to another comment on this section (L694 Reviewer 1, above), we believe this similarities are reduced somewhat. In addition, we have thinned out the explanatory text in the middle of the second paragraph and added text to the end of the section that further develops the structural model we are proposing.

Line 718. ***(Line 1179)*** This part seems more like the "model presentation" rather than a discussion. Here, authors put together all pieces from the puzzle to present their model.

As part of the reorganization of the manuscript, this section of the manuscript now falls in a new, results-focused section that is before the discussion section of the manuscript.

(Line 1179) It is hard to understand why the authors choose these two specific cases of study. This choice should be explained and discussed.

We have added a statement clarifying that these structures were chosen for additional focus based upon geomorphic evidence, which will be discussed in this section.

Line 762. ***(Line 1578)*** This description is the key point of the section. In the previous paragraphs, it is hard to see what point the authors are trying to make.

We have added text both here and in earlier paragraphs in an attempt to clarify the point we are making and how we get there. We hope the new text and revised organization make our observations and resulting interpretations clearer.

Line 763+: ***(Line 1582***) In this section, the authors propose that the Montezuma hills are being uplifted by southward reverse-oblique motion on the Pittsburg-Kirby hills fault. Given that the PKHF exhibits right-lateral focal mechanisms, and the first-order tectonic boundary conditions are distributed Pa-SN dextral shear, the Montezuma Hills is not being translated south in an absolute sense relative to the Sierran microplate. The text should be revised to clarify that the hanging wall of the PKHF is moving south relative to the north-moving footwall. If this is not what the authors mean, then they need to provide a mechanical explanation for how the Montezuma hills are moving south independently of the north-moving crust on the opposite side of the PKHF (i.e., explain how a right-lateral shear traction on the western margin of the hills is making them move south).

We have added additional clarifying text that we are suggesting relative/apparent southward translation relative to material west of the PKHF/Montezuma Hills block.

Line 763. ***(Line 1579)*** Authors should provide a Figure to illustrate their model.

Citation to Figure 8 added at the end of the sentence.

Line 767. ***(Line 1590)*** Similar comment to before, and valid comment for many places along the text, such strong statement should be provided with a reference.

Citations added

Line 770. ***(Line 1590)*** Which model? Authors should cite a figure or be more precise when mentioning the model.

The model being proposed is described in the preceding paragraph, and in this sentence and the ones that follow. We hope that the new organization of the manuscript reduces confusion on this point here and throughout the manuscript.

Line 771+: ***(Line 1590)*** Here, the authors state that the Montezuma Hills are being translated "up and onto the north-dipping flank of Mount Diablo anticline," possibly along a west-northwest striking, north-dipping thrust fault underlying the Sacramento River. Is this consistent with the previous statement above that the causative structure is the PKHF? Please clarify. It would be useful to draw a north-south cross section across the river to illustrate this interpretation. The cross section can also provide a test of the model by showing whether the base of the Tehama Formation in the southern Montezuma Hills projects southward above its elevation in Antioch south of the river, consistent with being elevated in the hanging wall of a thrust fault.

We have added text to the paragraph to clarify the structural geometry we are proposing, and the implications for known and proposed faults. In addition, we have updated Figure 8 to include cross sections across the Montezuma Hills (modified from Medwedeff (2021)) that we hope clarify the geometries and structural relationships we are proposing. We have added citations to that figure here as well.

Line 777. *(Line 1685)* This entire section is not a discussion section, it is a "description and interpretation" section. This comment is valid for most part of the 5. Discussion section.

The text in question is now found in the results and interpretation section of the reorganized manuscript.

Line 777+: *(Line 1594)* The authors probably don't know this, but they re-stating arguments that were originally proposed in the 2009 DRMS study as a basis for defining the "Montezuma Hills areal source zone". As noted by Weber-Band (1998), the topographic and structural gradients in the Montezuma hills are to the northeast, which makes it problematic to attribute uplift of the hills to simple reverse reactivation of the north-striking, west-dipping Midland fault, or the dextral-oblique PKHF. Workers involved in the DRMS study recognized this and suggested that uplift of the hills may be related to slip on a cryptic structure or structures that have not yet been identified or imaged. The intent of defining the "Montezuma hills areal zone" is to model the possibility these structures exist, including uncertainty about whether they exist, for the probabilistic seismic hazard analysis input into the DRMS risk analysis. Unfortunately, the documentation for the DRMS study is difficult to find (and getting harder with the passage of time). Here's a link to the DRMS seismic hazard analysis; see p. 51 of the pdf for a terse summary of the MH areal zone source characterization:

https://www.waterboards.ca.gov/waterrights/water_issues/programs/bay_delta/california_waterfix/exhibits/docs/dd_jardins/DDJ-157_DRMS_Sec6.pdf

We are indeed aware of the Montezuma Hills Source Zone specifically, and the DRMS report more generally. The report is cited extensively in the originally submitted manuscript, including direct references to the Montezuma Hills Source Zone.

We have added a more direct reference to the report and the Montezuma Hills Source Zone here, in addition to further clarifying detail about what we observe and propose.

Line 782. *(Line 1705)* It is not clear which model the authors refer to.

We have added clarification that we are referring to the kinematic model that has been set up and discussed extensively in the preceding paragraphs.

We hope that the reorganization and restructuring of the manuscript makes this and other connections and internal references more clear to future readers.

Line 790+: *(Line 1714)* This section circles around multiple points without landing on any conclusively. The authors recognize that late Cenozoic structural and topographic relief in the Delta are less than in the western San Joaquin Valley to the south (at Coalinga and in the Kettleman Hills) and in the southwestern Sacramento Valley (English Hills, Rumsey Hills). The authors suggest this difference is due to the presence of the Paleogene Rio Vista basin structures, and a "leftward step" in the Great Valley fault system around Mount Diablo (?); however, this model is not explained in enough detail for me to understand it. These observations all suggest that Quaternary deformation rates in the Delta are lower than along other reaches of the Great Valley fault zone to the north and south. Why is this so? Is it unique to the Delta? Do deformation rates vary elsewhere along the western edge of the Central Valley? What do the new GPS deformation models for the 2023 NSHM updates show? In line 897, the authors throw up their hands: “How the Great Valley Fault System and Pittsburg-Kirby Hills faults interact, and how and whether the partitioned strain is directed onto structures within the Delta, remains enigmatic, even on the scale of individual faults.” This is a very unsatisfactory takeaway from what is presented as a “case study”.

This comment refers to a group of paragraphs that have been redistributed between this section and section 5, which follows and discusses the observations reported in section 4.

We believe the reorganization of the manuscript as a whole, including redistribution of these paragraphs, should help address the reviewer's comments by presenting the observations in context with others and similarly by condensing the discussion into one section. In addition, we have added text to the paragraph in section 5 that we hope more clearly represents our proposed model, in which the Delta faults accommodate the same strain, but across more structures (the Rio Vista Basin) and around a restraining bend (the Diablo stepover and anticline).

Line 791. ***(Line 1715)*** What is the question that the authors are trying to address here? It is not indicated clearly?

We have added a short introductory paragraph to the top of section 4 to set the stage for the subsections that follow, including this one.

Line 796. ***(Line 1721)*** There is not geological information in Figure 6 to illustrate this statement.

Though we still believe that a reference to Figure 6 is appropriate here, we have also added a reference to Figure 1, which includes geologic information

Line 799. ***(Line 1723)*** This statement should be more argued, illustrated, and discussed. There is a lack of citations here (Figures, other studies…)

Citation for Trexler et al., 2022 and reference to Figure 6 added.

Line 802. ***(Line 1727)*** Indeed, this section describes observations and does not constitute a proper Discussion.

We believe this comment is addressed by the reorganizaiton of the manuscript, which places this text in the section with observations and results.

Line 808. ***(Line 1746)*** This Figure is the most important Figure of the paper as it presents the model. Hence, it requires a more detailed description and explanation, and should not be cited without mention I the text.

The revised figure 8 is no longer appropriate to cite here, and the citation has been removed.

Line 830. ***(Line 1886)*** Reference? Figure?

Added references to Krug 1992 and MacKevett, 1992 as illustrative examples.

Line 833. ***(Line 1889)*** It is not clear what the authors refer to. Such sentence required Figures and citations.

We have restructured this sentence in an attempt to make it more clearly linked to the preceding sentences, which explain it and provide citations.

Line 846. ***(Line 1882)*** This is a very important statements that needs to be more argued, illustrated, and discussed.

We have added additional clarification about the geomorphic indicators we are referring to, and added a reference to figure 7, which shows a site within the Delta that also does not clearly indicate any active strike-slip displacement (but does appear to record active reverse displacement). While a more in-depth exploration of the GVFS and strain partitioning is a fascinating question, we believe that additional figures and investigation of this hypothesis is beyond the scope of this manuscript.

Line 854. ***(Line 2051)*** Montezuma Hills and GVFS were also described earlier as part of the Delta region. Hence, it is hard to understand why the authors introduce here "Delta faults" as if the previous ones were not part of the Delta system.

We have revised this paragraph to specifically refer to the Pittsburg - Kirby Hills fault and GVFS.

This paragraph is one of the most important of the manuscript since it discuss the activity of the faults across the Delta. However, for the reader to really understand the message, it is necessary to illustrate this part with a Figure.

We have rewritten this paragraph and added references, which combined with the restructured manuscript should make this point more clearly.

Line 930+: ***(Line 2078)*** The text states "it is overly simplistic to treat faults within the Delta as subvertical faults accommodating dextral slip", but it's not clear who or what this criticism is addressed to. The previous work cited in the paper does not make this assumption, and existing seismic hazard models (DRMS, MWD, UCERF3) do not make this assumption either. The authors reiterate their claim that faults in the Delta accommodate 5 mm/yr of dextral shear and 3 mm/yr of "boundary normal" (presumably NE-directed) shortening, but this assertion is not convincing because they do not offer a specific model for distributing this motion on known faults, nor do they reference the most current GPS data and modeling to support it.

We acknowledge that the framing here ('sub vertical…dextral faults') is perhaps a bit overly reductive and we have removed this sentence.

We believe that the second part of this comment is covered in part by our revisions elsewhere in the manuscript. We have also made changes to this section to emphasize our conclusion that the Pittsburg - Kirby Hills Fault plays a larger role than typically recognized, and to clarify the regional-scale model we are proposing for modern strain accumulation.

Comments on Figures

Fig 1.

"Thornton arch" is misspelled. What is the dotted line south and west of Mount Diablo supposed to represent?

We have corrected the spelling on Thornton on the figure. The dotted line south and west of Mount Diablo is a buried/blind fault in the Quaternary Fault and Fold Database, as indicated by the figure legend.

For this figure as well as most of the author figure, there is a lack of geology information that would provide the reader a better appreciation of the structures within the region. In fact, the background image from the map does not provide much information apart from the geographic context, which is not crucial to the study. Limits of the tectonic plates and arrows with plate displacement rates could also be added to the inset figure as well. Moreover, all other figures from the study should be located on Figure 1. Finally, why do the authors indicate the Mercalli intensity for the 1892 sequence whereas a moment magnitude is indicated in the text? Is there an approximate location for the epicenter of this seismic sequence?

We have added a geologic basemap (Jennings et al., 2010) to the figure, and have added extent boxes for figure 2 and 5. We use a polygon of Mercalli intensity for the 1892 sequence because the epicenter is not precisely known.

Fig. 2.

Why not combine these two maps into a single figure?

Similar comment than for Figure 1. The geology is discussed in the text but not shown the figures. The topography map presented here does not provide crucial information for the understanding of the study. Figure 2 should also be located in Figure 1.

Following the comments of both reviewers, we have combined the two maps into a single figure that shows both seismicity and geodetic data. The footprint of this figure has been added to figure 1.

Fig. 3.

See previous comments on technical problems with this figure.

This figure seems to arrive a little too early compared to other figures. Indeed, it presents slip rates along all the faults of the Plate boundary, which is more related to the discussion part than the observation and data part (I suggest to associated Figure 3 with Figure 8). Moreover, it is not clear faults from the Delta are associated only with a shortening rate whereas the text also discusses the occurrence of dextral slip on some of these faults.

Lower part of Figure 3 should also be explained in more details in the legend as it is not clear to understand at first. Implications of the Figure should also be directly indicated within the figure, eventually using arrows that show the residual slip rate across the plate boundary after cumulating slip along all the faults. Moreover, across the 3 sections presented, it seems that cumulative slip rates across all faults from the plate boundary are greater than the tectonic slip rate, which implies that activity along the Delta faults is not necessary to accommodate the tectonic loading.

Following the comments on the lower panel of the original figure 3, and the comments and discussion around this figure in the text, we have elected to remove the lower panel from the figure in the revised manuscript. See discussion in text comments for further detail.

Fig. 4.

This figure focuses on structure along the southwestern margin of the Sac Valley north of the Delta, AND it is basically a repeat of Fig 1 in Trexler et al. (2022). Does it really add anything to the stated focus on structures in the Delta for this paper?

Panel a: This map should provide more geologic information along with the fault geometries. Indeed, the authors discuss the role of pre-existing structures, but these are never shown to the reader. The geology is extensively discussed in the text, and thus it should also be presented in the Figures (at least the main geological units).

Panel b: The two models presented here are not much discussed in the text. The authors should indicate more clearly why these two models are important, and what are the implications for the regional-scale model (Figure 8) of each in terms of fault activity and deformation processes.

In the revised text, these models are referenced more readily and in multiple places; as such, we still believe this figure is important and have elected to keep it in place in the manuscript. We have added a location rectangle to figure 1.

Fig. 5. (this figure was figure 6 in the prior draft of the manuscript)

This figure is basically repeating Fig. 2 in Trexler et al. (2022). Is it really necessary to include in this paper?

This figure is very similar to that presented in the study from Trexler et al. (2022). Authors should justify better their choice to present again this Figure. I suggest that the authors cite their previous study and leave space for new figures for this manuscript. Moreover, text size within panel a could be larger.

We have added a pair of new panels to this figure that show the residual elevation model for a longer reach of Putah Creek, and capture the Plainfield Ridge and Dixon Rise. Particularly with the inclusion of these new panels, we believe this figure provides valuable information for the reader and have elected to keep it.

Fig. 6 (this figure was figure 5 in the prior draft of the manuscript

Again, there is a lack of geologic information. It also seems that the two panels, a and b, could be combined together and the main points made clearer to the reader. Legend should also express more clearly what is the aim of this figure, and what are the implications of these observations.

We have removed the bedrock faults panel, and replaced it with a new panel that shows a swath and longitudinal profile for a trunk stream and tributaries across one of the wind gaps. We believe that this provides additional documentation of the geomorphologic asymmetry across the Montezuma Hills.

Fig. 7.

Panel a: Legend indicates a water gap but water can be seen all along the Denverton creek. Hence, it is not clear where the water gap is. Color from panel b (wight) is not very well visible on the topographic map.

We have added a label to the water gap.

Panel b: Why is the geological map presented only for this small region and not all region from panel a? Why is the water gap visible in panel b but not in panel a? Moreover, even though the legend says that there is topographic information in both panels a and b, topography map is visible only in panel a (legend should be modified).

We have elected to retain the topography in panel a and the geology in panel b, as originally drafted, because we think both datasets are valuable in interpreting the site history. We note that the geology beyond the inset is nearly entirely Qpf, and thus not much information is added by including geology beyond the scale of panel b. We have removed the mention of topography from the caption for panel b.

Panel c: It is not indicated whether this geologic cross-section is extracted from another study or if it was made by the authors. If it was made by the authors, then authors should provide more explanations on the method (e.g., how do they constrain on the tilt angle of the structures at depth?).

This cross section is our own construction, based on bedrock orientation data from geologic mapping by Graymer et al. (2002). We have added this information to the caption.

Fig 8.

The maps and cross sections are so schematic that it is difficult to tell if the scales are roughly correct. Also, “Late K-early N” covers a lot of time, and it’s incorrect and/or misleading to suggest that the structures in (a) all were active simultaneously (?) and continuously (?) during this entire span of time.

Figure 8 represents the final synthesis figure of the paper with the structural model of the Delta region. However, the relation between this figure and the main text is not clear. A larger and dedicated paragraph should be provided in the main text to explains more clearly the different modes of deformation presented in the panels, and the implications. Moreover, this Figure could be more complex with greater structural and geologic information. For example, cross sections could be larger and present more details on the fault structures. Geologic information could also be provided, especially in the maps from panels c and d. To highlight the role of inherited structures, a color code could be used to highlight different groups of faults. Finally, Figures 3 and 8 could be presented next to each other since they both discuss the role of the Delta’s faults for accommodating tectonic loading across the entire plate boundary.

We have replaced the original figure 8 with a new figure that presents geologic cross sections modified from Medwedeff (2021) and highlights structures that we believe to be active in the Quaternary based upon the data reviewed and presented in this manuscript.

2nd Round of Revisions

Decision Letter (30 Nov 2023)

Dear authors,

Thanks for submitting the revised version of your manuscript. Your revision and responses were quite thorough, and it took us some time to reach a decision. After carefully evaluating them, our associate editor, Jack Williams, recommended not to send again your manuscript to the reviewers, but he is requesting some moderate revision before we can accept it for publication.

You'll find Jack's recommendations appended to this letter. I endorse all of his recommendations and ask you to address them in your new revision. I have also listed few remarks and requests from my own at the end.

Please submit your revision together with an answer to our comments, and a marked changes version. As this is a moderate revision, we hope to receive it in less than a month. It would be great if you can submit it before the Christmas break, though I will give you a due date on January 8th to allow for that break and a little longer time if needed.

Best regards

Robin Lacassin, Tektonika Executive Editor

-------- Associate Editor (AE) evaluation:

Dear authors

Thank you for the revised version of this interesting article. On reading the revised manuscript and your reply to reviewers comments I am satisfied that, on the whole, the reviewer's comments have been addressed. Nevertheless, I still have a few relatively minor editorial comments (below) that I would like to see addressed before acceptance of this manuscript. I look forward to seeing the revised version of this article.

Best
Jack Williams

Major Comment

I appreciate the addition of Table 1, which addresses comments from Reviewer #1 that more recent geodetic models for the faults in this study area should be considered. Nevertheless, I still think the revised submission relies too heavily on older geodetic data. For example, the geodetic fault slip rates shown in Figure 3 have not been updated (e.g., the geodetic Green Valley fault slip rate is still shown as ~7 mm/yr (following D'Alessio et al., 2005), whilst its slip rate estimate in more recent geodetic models is 1-5 mm/yr.) Furthermore, Section 5.1 is still framed around the region accommodating "as much as" 5 mm/yr of displacement. Although possible, this is really only one end member for a range of deformation rates within the Delta that are permissible given the various geodetic models documented in Table 1 (for example, the Evans 2022 rates in Table 1 suggest <2 mm/yr on various GVFS strands).

Hence, although I agree that the relatively subdued topography of the Delta could be indicative of relatively high (~5 mm/yr) deformation rates and erosion/buried faults/etc etc as suggested at Lines 838-848, I don't think you can exclude the alternative possibility that its subdued topography is just representative of very low deformation rates.

As a side-point here, I would be particularly wary of relying on the older geodetic models to constrain fault slip rates within the Delta. In other low strain rate regions, earlier geodetic models were subsequently to found overestimate deformation rates compared to newer geodetic data with a longer time series; see for example the downward revisions of rates estimated for the New Madrid Seismic Zone from 5-7 mm/yr in initial studies to <0.5 mm/yr in later studies (Calais et al 2016).

Calais, E., Camelbeeck, T., Stein, S., Liu, M., & Craig, T. J. (2016). A new paradigm for large earthquakes in stable continental plate interiors. Geophysical Research Letters, 43(20), 10-621.

General Comment

Throughout the text, (but particularly Section 3.2), there are long sections describing different fault names/rivers/geomorphic features that are not cross referenced with one of the maps; as a rule of thumb, I would like to see a figure reference given the first time a location is described.

Minor Comments (line references revised clean manuscript version)

Line 258: Suggest add a simple map to show the extent of the different sub regions (or add new panel to an existing figure)

Line 340: Suggest revise to "800 m long seismic survey" to specify what type of geophysical survey is being discussed here.

Line 416: Add a label to the Mokeumne River to one of the figures (Figue 1?)

Line 750: 'Energetically favourable' does not seem the correct term to me when describing fault reactivation; 'mechanically favourable' suits better as that is how we typically conceptualise fault reactivation.

Lines 924-930: A lot of discussion on seismic hazard here, and so an obvious follow-up question is do you have any recommendations for how faults in the Delta should be considered in future seismic hazard models? Perhaps some reference could be made here to how these faults were considered in the 2023 US NSHM, and whether an earthquake from the PKHS to the Green Valley-Bartlett-Springs Fault is already considered in this model (or does this 'jump' exceed the 'plausibility criteria' for possible ruptures in this model?).

Line 1056: Figure captions for Figure 5 (Putah Creek figute) and 6 (i.e. Montezuma Hills figure) are the wrong way round? This also applies to their labels in Figures 1 and 2

------- Executive Editor additional remarks:

- Adding the table 1, as you have done, will surely improve the paper, as well as now considering the recent geodetic data and models. But I agree with AE that we need a more balanced discussion about the rates, unknowns, uncertainties, and issues related to the very subdued relief of the region. Please consider to further improve these aspects.

- Also, as your table 1 only lists the more recent geodetic studies (all from 2022) it's really difficult to compare them with earlier studies you are citing and using in your discussion. Please update the table to be more comprehensive.
- It's really difficult to make the link between the fault / structure names that are listed in this table and the structures shown on the different maps. Please use the same terminology, and numbering. And/or add a map linked to the table to help the reader. See also AE remark about the names of fault names/rivers/geomorphic features.
- Add "Author contributions" and "Data availability" sections. More generally, please format your manuscript using one of the templates (word or latex) available from the authors guidelines on Tektonika web site. This will help the copy-editing team which is all voluntary.
- Even it's not mandatory, and as your manuscript will be a review paper, I suggest to add a "Plain Language Summary" just after the formal scientific abstract.

Authors' answer to editorial comments (2nd round)

Response to editor remarks, "Quaternary-active faults and the role of inherited structures in the Sacramento-San Joaquin Delta, western Central Valley, northern California" by Trexler et al.

Editor remarks are in black text*, and our responses are in blue italics.*

Associate Editor evaluation:

Dear authors

Thank you for the revised version of this interesting article. On reading the revised manuscript and your reply to reviewers comments I am satisfied that, on the whole, the reviewer's comments have been addressed. Nevertheless, I still have a few relatively minor editorial comments (below) that I would like to see addressed before acceptance of this manuscript. I look forward to seeing the revised version of this article.

Best
Jack Williams, Associate Editor

Major Comment

I appreciate the addition of Table 1, which addresses comments from Reviewer #1 that more recent geodetic models for the faults in this study area should be considered. Nevertheless, I still think the revised submission relies too heavily on older geodetic data. For example, the geodetic fault slip rates shown in Figure 3 have not been updated (e.g., the geodetic Green Valley fault slip rate is still shown as ~7 mm/yr (following D'Alessio et al., 2005), whilst its slip rate estimate in more recent geodetic models is 1-5 mm/yr.)

We have updated figure 3 to include NSHM23 rates from Evans (2022). We have elected to include the rates from Evans (2022) because this model reports rates for subsections of individual faults; in the interest of completeness, rates from the remaining NSHM23 models are included in the newly-added Supplementary Table 1.

Furthermore, Section 5.1 is still framed around the region accommodating “as much as” 5 mm/yr of displacement. Although possible, this is really only one end member for a range of deformation rates within the Delta that are permissible given the various geodetic models documented in Table 1 (for example, the Evans 2022 rates in Table 1 suggest <2 mm/yr on various GVFS strands).

Hence, although I agree that the relatively subdued topography of the Delta could be indicative of relatively high (~5 mm/yr) deformation rates and erosion/buried faults/etc etc as suggested at Lines 838-848, I don’t think you can exclude the alternative possibility that its subdued topography is just representative of very low deformation rates.

As a side-point here, I would be particularly wary of relying on the older geodetic models to constrain fault slip rates within the Delta. In other low strain rate regions, earlier geodetic models were subsequently to found overestimate deformation rates compared to newer geodetic data with a longer time series; see for example the downward revisions of rates estimated for the New Madrid Seismic Zone from 5-7 mm/yr in initial studies to <0.5 mm/yr in later studies (Calais et al 2016).

Calais, E., Camelbeeck, T., Stein, S., Liu, M., & Craig, T. J. (2016). A new paradigm for large earthquakes in stable continental plate interiors. Geophysical Research Letters, 43(20), 10-621.

We very much appreciate the comments regarding the meaning/relevance of geodetic rates, and comparisons between geodetic and geologic rates, in low strain rate regions. We have revised the text in section 5.1 to de-emphasize the focus on reasons why high rates of deformation might not be recorded, and to add additional clarification on reasons why the strain rate in the Delta might not actually be at the higher end of the reported geodetic range. We have also made several small changes to section 3.1 (“Regional-scale studies: Geodetic Slip Rates”) to represent the range of rates presented and reduce the emphasis on the upper rate bounds.

General Comment

Throughout the text, (but particularly Section 3.2), there are long sections describing different fault names/rivers/geomorphic features that are not cross referenced with one of the maps; as a rule of thumb, I would like to see a figure reference given the first time a location is described.

We have added river labels to Figure 1, and adjusted the symbology slightly to make it less busy. We have also added additional figure references throughout the text where we use geographic descriptions and/or introduce new features (these are primarily found in section 3).

Minor Comments (line references revised clean manuscript version)

Line 258: Suggest add a simple map to show the extent of the different sub regions (or add new panel to an existing figure)

We have added subregion labels to the simplified fault map in Figure 3, and added a reference in the text.

Line 340: Suggest revise to “800 m long seismic survey” to specify what type of geophysical survey is being discussed here.

Change made.

Line 416: Add a label to the Mokeumne River to one of the figures (Figue 1?)

We have added a label for the Mokelumne River (and other rivers) to figure 1.

Line 750: ‘Energetically favourable’ does not seem the correct term to me when describing fault reactivation; ‘mechanically favourable’ suits better as that is how we typically conceptualise fault reactivation.

This is a fair criticism, and we have changed the word choice as suggested.

Lines 924-930: A lot of discussion on seismic hazard here, and so an obvious follow-up question is do you have any recommendations for how faults in the Delta should be considered in future seismic hazard models? Perhaps some reference could be made here to how these faults were considered in the 2023 US NSHM, and whether an earthquake from the PKHS to the Green Valley-Bartlett-Springs Fault is already considered in this model (or does this 'jump' exceed the ‘plausibility criteria’ for possible ruptures in this model?).

It is beyond the scope of this particular study to examine the plausibility of a throughgoing rupture from one fault to another within the Delta, but we do believe that geologic and geomorphic data may provide constraints on this kind of question for future seismic hazard studies. We have added a short discussion of how Delta faults are treated in NSHM23, and our view of the significance of geologic and geomorphic data in refining seismic hazard assessments in the future.

Line 1056: Figure captions for Figure 5 (Putah Creek figute) and 6 (i.e. Montezuma Hills figure) are the wrong way round? This also applies to their labels in Figures 1 and 2

The figure captions for figures 5 (Putah Creek) and 6 (Montezuma Hills) were indeed flipped. We have switched them into their correct locations, and have confirmed that labels in figures 1 and 2 (and their captions) are appropriately located and described.

Executive Editor additional remarks:

Adding the table 1, as you have done, will surely improve the paper, as well as now considering the recent geodetic data and models. But I agree with AE that we need a more balanced discussion about the rates, unknowns, uncertainties, and issues related to the very subdued relief of the region. Please consider to further improve these aspects.

As discussed above, we have made substantial changes to section 5.1, along with minor changes to section 3.1, to more thoroughly discuss the full range of slip rates in the geodetic models rather than focusing on the high end of permitted rates. Please see responses to the AE’s major comment, above.

Also, as your table 1 only lists the more recent geodetic studies (all from 2022) it's really difficult to compare them with earlier studies you are citing and using in your discussion. Please update the table to be more comprehensive.

As requested, we have added the geodetic rates reported by d’Alessio et al. (2005) and Evans (2012) to Table 1 to assist in the comparison between those models and the four NSHM23 models. Both d’Alessio et al. (2005) and Evans (2012) report model slip rates in dip-slip and strike-slip components; in Table 1, we have calculated the combined slip rate vector and rake to allow for comparison with NSHM23 models.

With the addition of the above data, we have made the decision to separate the dataset into two tables (new Table 1 and Table 2): Table 1 reports geodetic slip rates on Delta faults from NSHM23 models along with d’Alessio et al. (2005) and Evans (2012), while Table 2 reports geologic slip rates on Delta faults from NSHM23 (Hatem et al., 2022) and other sources (cited in the table itself).

It's really difficult to make the link between the fault / structure names that are listed in this table and the structures shown on the different maps. Please use the same terminology, and numbering. And/or add a map linked to the table to help the reader. See also AE remark about the names of fault names/rivers/geomorphic features.

We have standardized how we refer to faults in the tables and figures, in a way that is more consistent with our usage in the text. Because these faults are often referred to by different names in other publications (indeed, this is one of the motivating factors for the creation of this manuscript!), we have attempted to keep some of *those details (i.e. numbered sections of the GVFS, and the fault ID numbers used in NSHM23 models) in the tables to clarify which faults in our naming scheme correspond to which faults in other publication.*

Add "Author contributions" and "Data availability" sections. More generally, please format your manuscript using one of the templates (word or latex) available from the authors guidelines on Tektonika web site. This will help the copy-editing team which is all voluntary.

Even it's not mandatory, and as your manuscript will be a review paper, I suggest to add a "Plain Language Summary" just after the formal scientific abstract.

We have added all three of the requested sections to the revised text.

Final decision (4 Jan 2024)

Dear Charles Trexler, Jack Willard, Belle Philibosian:

The final revision of your manuscript has now been evaluated by our associate editor (Jack Williams) and executive editor (Robin Lacassin). We are now pleased to accept your article for publication in Tektonika. Messages will follow in the coming days / weeks with regards to copy-editing and production.

Best regards, thanks for submitting to Tektonika

Jack Williams, Tektonika Associate Editor

Robin Lacassin, Tektonika Executive Editor