Review Report

Hatch et al., Mechanical Analysis of Fault Slip Rate Sites within the San Gorgonio Pass Region, Southern California USA, TEKTONIKA, 2023.

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1st Round of Revisions

Decision Letter

Dear Michele Cooke, Jennifer Hatch, Hanna Elston:

Thank you for choosing to submit this manuscript to Tektonika. As I have discussed with Michele following the transfer of this manuscript from Seismica to Tektonika, I viewed the manuscript as being publishable as is, but to aid transparency and avoid bias on my part, a couple of expert reviews have been solicited. You will see from below that the reviewers are in agreement that this is an exciting and well-illustrated piece of work, but in need of some clarification, particularly regarding the text and model assumptions. These reviewers are more familiar with the topic presented than I, and I agree with their assessment and think that addressing these comments will help improve the manuscript further. Thus our decision is: Revisions Required. Please provide a detailed response to the reviewer comments as part of your revisions, as this will help expedite the review process.

Hopefully the revisions required will not be too onerous, but do feel free to contact me if you have any queries or want to discuss timeframes.

Kind regards,

Craig Magee (Executive Editor)

NB: As the decision letter alludes to, this manuscript was originally submitted to and reviewed by Seismica. As the journals evolved in their initial stages, the authors decided Tektonika was becoming a better host for their work so transferred the manuscript.

Comments by Reviewer 1 (Sam Wimpenny)

I have provided both a general review, plus tracked changes and comments on the word document due to the lack of line numbers in the submitted manuscript. Hopefully these work for the authors and editor, but let me know if it doesn't. You might need to click 'Show all markup' in Word to see everything.

Review of *"Mechanical Analysis of Fault Slip Rate Sites within the San Gorgonio Pass Region, Southern California USA"* submitted to Tektonika by Hatch et al.,

Review by: Dr. Sam Wimpenny (University of Leeds)

General Comments:

Hatch et al., use a model faulting in an elastic crust subject to far-field relative plate motion to investigate the mechanical compatibility of late Quaternary slip rate estimates from the San Andreas of southern California. Much of the model setup and method used in the manuscript builds upon the earlier work of Beyer et al., (2018). The key contribution of this new manuscript is four more models that explore what effect uncertainties in the slip-rate estimates, and the location of sites of slip rate estimates within the fault network, have on the predicted slip rates and off-fault deformation elsewhere within the fault network. The main conclusions are that by not including a particular set of faults, the models fit the observed slip rates better, but produces larger amounts of off-fault deformation. The authors also use their models to argue that future slip rate studies should focus on fault branches, because branches place the best constraints on the slip rate distribution elsewhere within the fault network in their model setup.

This study touches on an important question in paleoseismological and geomorphological studies of faulting: how does strain get partitioned across complex networks of faults, and is this partitioning accurately reflected in late Quaternary slip rate estimates? However, I found the text and the arguments in the manuscript difficult to follow, particularly with the large number of faults (which, granted, I'm not overly familiar with) and the long model names. I have made comments as tracked-changes to the manuscript file where I think the clarity can be improved, as there were no line numbers. There is a lot of them, and most are recommendations not requirements. The figures are clear and high quality.

In terms of the manuscript layout, I found that the subheadings in each section are not particularly clear about the section's content. I also think the Discussion section should be far more focused on the key contributions of this study – at the moment it mostly acts as a summary of other studies' results. A short Discussion is absolutely fine, and is more impactful, if it really homes in on what the implications are for the results of this modelling. I found Section

5.4 on Seismic Hazard was thin on concrete conceptual/practical arguments for how this work has changed our understanding of seismic hazard, either in the San Gorgonio Pass or more generally. I would recommend either updating Section 5.4 to do this, or remove it. The Conclusions could also be made far more succinct to summarise the results and arguments, as opposed to repeat the findings from the main text.

Technical Comments:

In terms of the manuscript's technical content, the modelling method is well-established and has been a feature of a number of previous papers from this area (e.g. Beyer et al., 2018; Herbert and Cooke, 2012). I list my substantive technical concerns below:

1. **The model the authors use assumes that the faults slip freely in response to far- field loading.** In this case, all of the spatial variability in fault slip rates relates to the geometry of the faults, and their stress interactions. However, there are a number of places where sub-parallel faults with similar geometries are observed to have different slip rates in both strike-slip and rift settings, implying that something other than just fault geometry and stress interactions plays a role in partitioning slip amongst faults, most probably the variability in fault rheology. Examples include normal faults within active rift systems that are sub-parallel but have variable late Quaternary slip rates (e.g. Copley et al., 2018), and sub-parallel normal faults that experience selective reactivation during basin inversion that implies variability in the stresses needed to reactivate and move faults (e.g. Sibson 1995).

   I agree with the author's argument that the shear stress needed to slip a fault during dynamic rupture is low, and so if dynamic friction controlled whether a fault slips or not then it should be reasonable to ignore its effects. However, maybe naively, I assume it is the stress needed to break the fault (i.e. effective static friction) that is important in partitioning the slip between faults, not the shear stress needed to slip the fault during dynamic rupture (i.e. dynamic friction). The reason I say this is, if you imagine that you have two parallel strike-slip faults and one has slightly lower static friction or a weaker sub- fault shear zone, then slip would preferentially partition onto the weaker one. This behaviour wouldn't be captured by the author's current model set-up, but rather it would be attributed to incorrect fault geometries or potentially inaccurate slip rate estimates (as opposed to bad model assumptions).

   Therefore, could the authors please add some text explaining why they believe that the variability in fault-mechanical properties is not important in controlling the partitioning of slip rates throughout the fault network in somewhere like the San Gorgonio Pass.

2. **One small concern that I have relates to the method of estimating the sensitivity of fault slip rate predictions to any particular site along the fault network.** The authors do this by perturbing the slip rate estimate at each site by 1 mm/yr, and measuring the change in the predicted slip rates at other sites. It seems implicit in this approach that the slip rate predictions are linearly dependent on the slip rate increment, such that increasing it by 1 mm/yr will lead to the same relative change in slip rate elsewhere in the network as changing it by 2 mm/yr. I would like to see some evidence from the model that this is the case, because although the models are in a linearly elastic half-space, the model geometry and fault interactions could produce non-linear behaviour. This would mean that the 'impact factors' are dependent on the size of the slip rate increment, and therefore are less meaningful than would first appear. This should be relatively easy to test and the tests could be included within the Supplementary Information.
3. **It is an implicit assumption in this study that the slip rates used to constrain the models reflect a steady state, as the slip rates coming from dating offset landforms of various ages (~4 ka through to ~40 ka).** Although this is definitely the simplest assumption, there's now plenty of evidence that fault slip rates at any particular measurement site vary over 1–100 kyr timescales from high fidelity records of faulting in both strike-slip (e.g. Dolan et al., 2016) and normal-fault settings (e.g. Mouslopoulou et al., 2009; Goodall et al., 2021). I would recommend that the authors add something to the text to state why they think their assumption of steady-state slip rates over the past ~50 kyrs is reasonable. Could the differences in slip rate estimates that cannot be explained by the model reflect non-steady-state behaviour, as opposed to inaccuracies in fault geometries?
4. **By fixing the fault slip rate at the surface but not at depth in the constrained models, the models are mechanically unstable over multiple earthquake cycles because large stresses accumulate in these areas (as evidenced by the off-fault deformation).** Could the authors explain why applying these kinematic constraints does not invalidate the assumption of mechanical consistency, and that faults slip at very low resolved shear stresses over the timeframe of interest (1–50 kyrs), which is still being used in the rest of the model domain?

Review Decision: I found this study interesting, if difficult to follow and read. My main critique is a philosophical one: the authors have observations of slip rates, and if the observations do not match their model, then they force the model to match the observations even if this breaks the initial physical assumptions in the model. There is no consideration of whether the inconsistencies between the modelled and observed slip rates might reflect bad model assumptions in the first place. Nevertheless, I do think there is value in testing a model that can extrapolate slip rate estimates along fault zones in a mechanically consistent way, and to use this to assess where further sampling is best placed to improve our knowledge of the slip- rate distribution in complex networks of faults. Therefore, in light of my comments, my recommendation is that the article needs some **moderate corrections** before being eligible for publication in Tektonika. Hopefully I have made it relatively clear what the authors can do to address my comments, and I look forward to reading their response.

References:

Goodall, H.J., Gregory, L.C., Wedmore, L.N., McCaffrey, K.J.W., Amey, R.M.J., Roberts, G.P., Shanks, R.P., Phillips, R.J. and Hooper, A., 2021. Determining histories of slip on normal faults with bedrock scarps using cosmogenic nuclide exposure data. Tectonics, 40(3), p.e2020TC006457.

Dolan, J.F., McAuliffe, L.J., Rhodes, E.J., McGill, S.F. and Zinke, R., 2016. Extreme multi- millennial slip rate variations on the Garlock fault, California: Strain super-cycles, potentially time-variable fault strength, and implications for system-level earthquake occurrence. Earth and Planetary Science Letters, 446, pp.123-136.

Mouslopoulou, V., Walsh, J.J. and Nicol, A., 2009. Fault displacement rates on a range of timescales. Earth and Planetary Science Letters, 278(3-4), pp.186-197.

Copley, A., Grützner, C., Howell, A., Jackson, J., Penney, C. and Wimpenny, S., 2018. Unexpected earthquake hazard revealed by Holocene rupture on the Kenchreai Fault (central Greece): Implications for weak sub-fault shear zones. Earth and Planetary Science Letters, 486, pp.141-154.

Sibson, R.H., 1995. Selective fault reactivation during basin inversion: potential for fluid redistribution through fault-valve action. Geological Society, London, Special Publications, 88(1), pp.3-19.

Comments by Reviewer 2 (Rebecca Bell)

This paper tackles an interesting question- do uncertainties in slip rates from different segments of a fault have differing impacts on constraining kinematic models? The paper tests a range of models for the San Andreas fault system in terms of whether there is or is not an active slip pathway in the San Gorgonia pass area and explores which scenario best reproduces slip rate values that have been determined from geological observations. The paper deals with slip rates determined from geological data averaged over the last 16 kyr, recognizing that slip rates may vary over time (I presume this is why there is a conspicuous lack of modern slip rates from satellite data included in the study?). This is a well written and well-illustrated manuscript that should certainly be published. I do have a few minor comments and suggestions that I think would help to increase the papers accessibility before publication that the authors may wish to consider:

I find the very first sentence of the abstract very difficult to follow and an unhelpful way to open the study- "Crustal deformation models show incompatibility between inferred fault geometry and geologic slip rates where model and geologic slip rates disagree". It was not until I had read the full introduction that I understood what you were trying to say in this opening sentence. To improve the accessibility of this article (which will be of interest to a wide range of geoscientists) I would highly recommend starting the abstract more like your non-technical summary with a statement about why this topic is important before launching into the details.

On a similar note, the non-technical summary is still too technical in places. For example 'inform active fault shape within the crust' will be meaningless to non-experts. Both the technical and non-technical abstract would benefit from some revision to make them as accessible as possible.

In the introduction below Fig 1 you review some slip rates that have been proposed for the San Andreas fault system. It would be really useful to know over what time periods these slip rates have been averaged. Are we comparing slip rates averaged over similar time periods here? A reference to Table 1 may be all you need to fix this.

At the bottom of page 4 you say "Rather than letting faults slip in response to tectonic loading as done with many previous models…" – however, in section 3.2 you do let the faults slip in response to tectonic loading (and then proceed to prescribing fault slip)('"Within a first set of unconstrained models, we allow the faults in the model to slip freely in response to tectonic loading and fault interaction"). The approach you follow just needs to be clarified so there are no contradictions.

Table 1 contains slip rates from geological data only. I am wondering why you don't consider slip rates derived from continuous GPS or survey GPS in this study? Presumably you would like to compare slip rate data consistently averaged over the last 16 ka? If so, you should state that clearly. It would be nice though to see some information on the modern GPS rates and how they compare to the Holocene averaged rates presented and modelled here.

On page 16 in the results, you say “The results highlight several regions where recent uplift data might distinguish between the two plausible models (roman numerals on Figure 5 e&f).”- I would be really interested to compare Figs 5e and f with modern vertical deformation fields from satellite data. Do you think this is worthwhile or are the modern deformation patterns not likely to be consistent with deformation patterns averaged over the Holocene? If not, I think you should state that somewhere as the lack of modern geodetic data in the paper is a bit conspicuous.

On this topic- the sentence in the discussion “Studies also show that slip rates can vary over tens of thousands of years, which makes older rates less reliable estimates of current activity. “ is needed much earlier in the introduction and expanded upon with references to said ‘studies’

This is a very thorough, exciting study and I look forward to seeing it published

Rebecca Bell, Imperial College

Authors' Reply to Reviewer 1

Hatch et al., use a model faulting in an elastic crust subject to far-field relative plate motion to investigate the mechanical compatibility of late Quaternary slip rate estimates from the San Andreas of southern California. Much of the model setup and method used in the manuscript builds upon the earlier work of Beyer et al., (2018). The key contribution of this new manuscript is four more models that explore what effect uncertainties in the slip-rate estimates, and the location of sites of slip rate estimates within the fault network, have on the predicted slip rates and off-fault deformation elsewhere within the fault network. The main conclusions are that by not including a particular set of faults, the models fit the observed slip rates better, but produces larger amounts of off-fault deformation. The authors also use their models to argue that future slip rate studies should focus on fault branches, because branches place the best constraints on the slip rate distribution elsewhere within the fault network in their model setup.

The reviewer has misunderstood the main conclusions of the paper. This study does not argue for one or the other viable fault interpretations. Instead, we consider both alternatives in our investigations of structural position of slip rate sites. The primary conclusion is that fault branches are regions where both fault slip rates and fault geometry should be carefully investigated. We believe that many of this reviewer's concerns and questions may stem from this critical misunderstanding. For example, the reviewer suggested to remove 'sites' from the title of our paper; however, the paper is not about slip rates but, in fact, about slip rate sites.

This study touches on an important question in paleoseismological and geomorphological studies of faulting: how does strain get partitioned across complex networks of faults, and is this partitioning accurately reflected in late Quaternary slip rate estimates? However, I found the text and the arguments in the manuscript difficult to follow, particularly with the large number of faults (which, granted, I'm not overly familiar with) and the long model names. I have made comments as tracked-changes to the manuscript file where I think the clarity can be improved, as there were no line numbers. There is a lot of them, and most are recommendations not requirements. The figures are clear and high quality.

We have adjusted the text throughout in response to many of the reviewer's helpful comments and suggestions.

In terms of the manuscript layout, I found that the subheadings in each section are not particularly clear about the section's content. I also think the Discussion section should be far more focused on the key contributions of this study – at the moment it mostly acts as a summary of other studies' results. A short Discussion is absolutely fine, and is more impactful, if it really homes in on what the implications are for the results of this modelling. I found Section 5.4 on Seismic Hazard was thin on concrete conceptual/practical arguments for how this work has changed our understanding of seismic hazard, either in the San Gorgonio Pass or more generally. I would recommend either updating Section 5.4 to do this, or remove it. The Conclusions could also be made far more succinct to summarise the results and arguments, as opposed to repeat the findings from the main text.

We feel that the current discussion provides opportunity to explore the various implications of the model results. The role of structural position, the incompatibility of interpreted fault geometry and measured slip rates, potential data that could inform activity of the northern slip pathway and the implications of our findings for seismic hazards are all valuable discussion topics. The start of the discussion appropriately cites other work in order to show how the findings of this study interface with and enhance existing knowledge.

We disagree with reviewer #2 that section 5.4 doesn't provide concrete example of the link to seismic hazard. We make a strong case for the need to gather additional data from fault branches in order to better assess the partitioning of active deformation. We have revised section 5.4 to make this more clear.

We prefer conclusions that summarize the findings rather than tight conclusions that read as bulleted lists. The reviewer may not appreciate that some readers scan the conclusions before reading the whole text. In our opinion, three paragraphs is not a lengthy conclusion.

Technical Comments:

In terms of the manuscript's technical content, the modelling method is well-established and has been a feature of a number of previous papers from this area (e.g. Beyer et al., 2018; Herbert and Cooke, 2012). I list my substantive technical concerns below:

1. The model the authors use assumes that the faults slip freely in response to far-field loading. In this case, all of the spatial variability in fault slip rates relates to the geometry of the faults, and their stress interactions. However, there are a number of places where sub-parallel faults with similar geometries are observed to have different slip rates in both strike-slip and rift settings, implying that something other than just fault geometry and stress interactions plays a role in partitioning slip amongst faults, most probably the variability in fault rheology. Examples include normal faults within active rift systems that are sub-parallel but have variable late Quaternary slip rates (e.g. Copley et al., 2018), and sub-parallel normal faults that experience selective reactivation during basin inversion that implies variability in the stresses needed to reactivate and move faults (e.g. Sibson 1995).

I agree with the author's argument that the shear stress needed to slip a fault during dynamic rupture is low, and so if dynamic friction controlled whether a fault slips or not then it should be reasonable to ignore its effects. However, maybe naively, I assume it is the stress needed to break the fault (i.e. effective static friction) that is important in partitioning the slip between faults, not the shear stress needed to slip the fault during dynamic rupture (i.e. dynamic friction). The reason I say this is, if you imagine that you have two parallel strike-slip faults and one has slightly lower static friction or a weaker sub-fault shear zone, then slip would preferentially partition onto the weaker one. This behaviour wouldn’t be captured by the author’s current model set-up, but rather it would be attributed to incorrect fault geometries or potentially inaccurate slip rate estimates (as opposed to bad model assumptions).

Therefore, could the authors please add some text explaining why they believe that the variability in fault-mechanical properties is not important in controlling the partitioning of slip rates throughout the fault network in somewhere like the San Gorgonio Pass.

The reviewer seems to conflate the static stress required to start the earthquake and the dynamic stress that produces slip on the fault. The slip that faults produce primarily happens during earthquakes when shear strength is low. The models of this study are not simulating the initiation of specific earthquake events but instead simulate the long-term slip rates over many earthquake cycles. In other words, we are not investigating how much shear stress is required to slip the fault but how the fault geometry resists the accumulation of slip over many earthquake cycles. Because of the low shear strength of faults within the long-term model, the model cannot evaluate whether a particular fault is active or not – all faults in the model will slip. For this reason, we include two plausible fault geometries for the San Andreas fault, one with and one without the Mill Creek. We have revised the text to clarify our explanation of this approach.

We cannot speak to the specifics of the cases that the reviewer describes, but two faults with similar strike and dip do not necessarily have the same loading rate. In any plate boundary, even when faults are parallel, they have different structural position within that plate boundary. Maybe one of the normal faults sits within a region of greater local strain rate than the other. Maybe one is longer or smoother than the other. The paper by Copley et al. (2018) that the reviewer cites suggests that heterogeneous lower crustal rheology may provide variations in loading. This interpretation for why some faults may have faster slip rate is not inconsistent with the upper crustal faults being weak during slip events. In this manuscript, we do not speculate on the mechanism driving slip rates on faults outside the study area, as this is well beyond both our expertise and the scope of this paper.

2. One small concern that I have relates to the method of estimating the sensitivity of fault slip rate predictions to any particular site along the fault network. The authors do this by perturbing the slip rate estimate at each site by 1 mm/yr, and measuring the change in the predicted slip rates at other sites. It seems implicit in this approach that the slip rate predictions are linearly dependent on the slip rate increment, such that increasing it by 1 mm/yr will lead to the same relative change in slip rate elsewhere in the network as changing it by 2 mm/yr. I would like to see some evidence from the model that this is the case, because although the models are in a linearly elastic half-space, the model geometry and fault interactions could produce non-linear behaviour. This would mean that the ‘impact factors’ are dependent on the size of the slip rate increment, and therefore are less meaningful than would first appear. This should be relatively easy to test and the tests could be included within the Supplementary

Information.

Doubling the applied slip, doubles the slip along the faults because the model is elastic. Crider and Pollard, (1998) provide benchmarking of Poly3D to linear elastic fracture mechanics analytical formula. It doesn't matter the value of the applied slip rate, as long as the slip rate is consistent among the sites and that the slip rate is applied over the same area at each site. We added a sentence to the text to explaining the elastic response of the model and that applying 1 mm/yr facilitates normalizing the impact to slip rate uncertainties.

3. It is an implicit assumption in this study that the slip rates used to constrain the models reflect a steady state, as the slip rates coming from dating offset landforms of various ages (~4 ka through to ~40 ka). Although this is definitely the simplest assumption, there's now plenty of evidence that fault slip rates at any particular measurement site vary over 1–100 kyr timescales from high fidelity records of faulting in both strike-slip (e.g. Dolan et al., 2016) and normal-fault settings (e.g. Mouslopoulou et al., 2009; Goodall et al., 2021). I would recommend that the authors add something to the text to state why they think their assumption of steady-state slip rates over the past ~50 kyrs is reasonable. Could the differences in slip rate estimates that cannot be explained by the model reflect non-steady state behaviour, as opposed to inaccuracies in fault geometries?

The slip rates that we use in the analysis range between 2.6 and 16 ka (see table 1), which is a more narrow time frame than the reviewer states. We are well-aware of temporal changes in fault slip rate and cite in the manuscript our recent paper on this topic (Elston et al., 2022). This is exactly why we do not use slip rates older than 40 ka. One of the key processes that Elston et al. (2022) demonstrate can drive slip rates changes is shifts in fault activity. Because evidence suggests that activity ceased on portions of the Mill Creek fault ~100 ka, we avoid considering slip rates older than 40 ka. This is explained within section 3.1. Reviewer #1 thought this issue of temporal slip rate variations interesting and asked for additional citations in the discussion, which we have added.

4. By fixing the fault slip rate at the surface but not at depth in the constrained models, the models are mechanically unstable over multiple earthquake cycles because large stresses accumulate in these areas (as evidenced by the off-fault deformation). Could the authors explain why applying these kinematic constraints does not invalidate the assumption of mechanical consistency, and that faults slip at very low resolved shear stresses over the timeframe of interest (1–50 kyrs), which is still being used in the rest of the model domain?

The reviewer misunderstands that because we fix the slip rate at the surface, this does not mean that slip is zero at depth. We still apply displacements along the edges of the model to simulate tectonic loading (figure 2) and the slip is non-zero along all active faults. This is explained at the end of the first paragraph of Section 3.1. The mechanical instability evidenced from off-fault deformation arises from the kinematic incompatibility between applied slip rate and fault geometry. We apply slip rate in order to investigate this incompatibility.

Review Decision: I found this study interesting, if difficult to follow and read. My main critique is a philosophical one: the authors have observations of slip rates, and if the observations do not match their model, then they force the model to match the observations even if this breaks the initial physical assumptions in the model. There is no consideration of whether the inconsistencies between the modelled and observed slip rates might reflect bad model assumptions in the first place.

The reviewer misunderstands that the reason that we apply slip rates in the constrained models is to assess the impact of kinematically incompatible slip rates and fault geometry at particular sites. We do not intend the constrained model to be a more accurate simulation of the crustal deformation within southern California. In other words, our goal in this study is not to match geologic observations. We are using the model as a tool to explore the impact of slip rate site locations with the goal of understanding how inaccuracies in slip rate and interpreted fault geometry might impact the fault system.

Nevertheless, I do think there is value in testing a model that can extrapolate slip rate estimates along fault zones in a mechanically consistent way, and to use this to assess where further sampling is best placed to improve our knowledge of the slip-rate distribution in complex networks of faults.

We agree with the reviewer that the paper’s finding that branches require additional data collection has value to the community.

Authors' Reply to Reviewer 2

Reviewer#1: Rebecca Bell

1. I find the very first sentence of the abstract very difficult to follow and an unhelpful way to open the study- "Crustal deformation models show incompatibility between inferred fault geometry and geologic slip rates where model and geologic slip rates disagree". It was not until I had read the full introduction that I understood what you were trying to say in this opening sentence. To improve the accessibility of this article (which will be of interest to a wide range of geoscientists) I would highly recommend starting the abstract more like your non-technical summary with a statement about why this topic is important before launching into the details.

Good suggestion. We revised the start of the abstract, which we think helps its accessibility.

2. On a similar note, the non-technical summary is still too technical in places. For example 'inform active fault shape within the crust' will be meaningless to non-experts. Both the technical and non-technical abstract would benefit from some revision to make them as accessible as possible.

We revised both abstracts and trimmed some less important information about the relevant performance of two plausible models out of the technical abstract.

3. In the introduction below Fig 1 you review some slip rates that have been proposed for the San Andreas fault system. It would be really useful to know over what time periods these slip rates have been averaged. Are we comparing slip rates averaged over similar time periods here? A reference to Table 1 may be all you need to fix this.

We added a sentence to emphasize the different time periods of the slip rates that also references Table 1.

4. At the bottom of page 4 you say "Rather than letting faults slip in response to tectonic loading as done with many previous models…" – however, in section 3.2 you do let the faults slip in response to tectonic loading (and then proceed to prescribing fault slip)('"Within a first set of unconstrained models, we allow the faults in the model to slip freely in response to tectonic loading and fault interaction"). The approach you follow just needs to be clarified so there are no contradictions.

Good suggestion. The previous wording also confused reviewer #2. We have revised for clarity.

5. Table 1 contains slip rates from geological data only. I am wondering why you don't consider slip rates derived from continuous GPS or survey GPS in this study? Presumably you would like to compare slip rate data consistently averaged over the last 16 ka? If so, you should state that clearly. It would be nice though to see some

information on the modern GPS rates and how they compare to the Holocene averaged rates presented and modelled here.

GPS inversions for interseismic slip rates do not provide the spatial resolution of slip rates that we need in order to explore the impact of structural position. Such inversions typically provide a general slip rate for the fault segment. We have compared forward model slip rates to GPS inversions for slip rate in previous papers (e.g., Marshall et al., 2009; Herbert et al., 2014a; Dorsett et al, 2019). While the model slip rates generally compare well to GPS inversions, Herbert et al., 2014b shows that because GPS inversions neglect off-fault deformation, 3D forward models that include off-fault deformation are better aligned with geologic slip rates than the GPS inversions. In regions of isolated planar faults with minimal off-fault deformation, geodetic inversions are a great way to estimate slip rates. However, in the study region with very closely spaced active faults, the GPS inversions do not provide reliable slip rate distributions and are not useful for the purpose of this study.

6. On page 16 in the results, you say "The results highlight several regions where recent uplift data might distinguish between the two plausible models (roman numerals on Figure 5 e&f)."- I would be really interested to compare Figs 5e and f with modern vertical deformation fields from satellite data. Do you think this is worthwhile or are the modern deformation patterns not likely to be consistent with deformation patterns averaged over the Holocene? If not, I think you should state that somewhere as the lack of modern geodetic data in the paper is a bit conspicuous.

Because the uplift from the model is steady-state uplift over earthquake cycles, it is not the same signal as the modern interseismic uplift measured by vertical GPS or InSAR. The modern data only show broad uplift signals from deep interseismic deformation and are missing the coseismic uplift signal that produces smaller wavelength variations. The data that we really need are geomorphic exhumation data. We have revised the sentence about geomorphic data to explain why this data is most appropriate.

7. On this topic- the sentence in the discussion "Studies also show that slip rates can vary over tens of thousands of years, which makes older rates less reliable estimates of current activity. " is needed much earlier in the introduction and expanded upon with references to said 'studies'

Good suggestion. We have added citations to this sentence in the discussion. The fourth sentence of the introduction explains that slip rates vary in both space and time (with citations). We added a sentence about older records being more vulnerable to temporal variations in the second paragraph. Within the methods section we explain that we do not consider slip rates older than 40 ka in our analysis because older slip rates are unreliable in this region of shifting fault activity.

Acceptance letter

Michele Cooke, Jennifer Hatch, Hanna Elston:

We have reached a decision regarding your submission to τεκτonικα, "Mechanical Analysis of Fault Slip Rate Sites within the San Gorgonio Pass Region, Southern California USA".

Our decision is to: Accept Submission