Initial Editor Decision
Subject: [Seismica] Editor Decision

Dear Dannielle Fougere, James Dolan, Edward Rhodes, Sally McGill:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Refined Holocene Slip Rate for the Western and Central Segments of the Garlock Fault: Record of Alternating Millennial-Scale Periods of Fast and Slow Fault Slip". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some minor revisions.

I have now received two detailed reviews of your manuscript. As you will see, both reviewers are enthusiastic about the prospects of this work to make a good contribution at Seismica.

In general, I concur with the comments provided by the reviewers. Reviewer #1 (anonymous) raises valid questions about the validity of your research to appreciably impact seismic hazard analysis, particularly probabilistic seismic hazard analysis (PSHA). Reviewer #2 (Jonathan Griffin) offers some thoughts on this subject as well. In addition to the comments made by both reviewers on this topic, I would ask that you also consider two related ideas. The first is that PSHA is focused on forecasting the potential for damaging ground motions in forward time. In this way, it is perhaps less important that we understand how slip rates have varied over the geological time scales leading up to the present day. A more salient question for PSHA efforts might be ‘how do we expect slip rates to vary in the future’, particularly over the ~50-100 year time scales that are often the operative domain of the hazard estimates those analyses produce? It is not clear to me that the data you present here can realistically speak to that issue, but it may make for useful discussion material. The second idea I would ask you to consider is that perhaps the contribution this manuscript has to make in the general realm of seismic hazard analysis is less about the nature of the seismic hazard analysis itself, but more about the way in which we parameterize the seismic source models (SMMs) that underly the hazard analysis. More specifically, it is often assumed that slip rates calculated on the basis of geologic observations of Holocene structures and/or deposits provide reasonable constraints on a SSM that may be used to forecast the future for rupture. The data you present here suggest that (depending on the setting) this assumption may not be valid, and indeed that it may not be valid over time scales as short as a few thousand years. This is perhaps a subtle departure from what you originally wrote (so much so that perhaps this is what you intended, but it is not clear in the current text), but I do think that a bit of additional nuance on the subject would make for a much stronger discussion.

Reviewer #2 (Jonathan Griffin) provides some important commentary and questions on how the individual IRSL ages determined for different horizons within individual fans were combined to produce the probability distribution of fan age. More specifically, they argue that the approach you are taking may underestimate the reasonable uncertainty in the proposed ages. They also provide references to previous work documenting an alternative approach to uncertainty estimation for your consideration. Reviewer #2 also requests some additional discussion be added regarding the proposed relationship between activities rates on the San Andreas and their potential to impact rates on the Garlock. As the reviewer points out, this is an interesting hypothesis, but I might argue that the current text on the subject "kicks the can down the road" a bit with respect to why large structures such as the Garlock or San Andreas may experience such substantial variations in activity rate over millennial time scales. Some additional commentary on the subject would be welcome.

As a final editorial comment, please note that Seismica does not permit "in revision" citations (line 696). You are welcome to leave the citation in place, but your manuscript would not be able to be published in Seismica until the work you are citing is available in published form.

When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Randy Williams

Comments of Reviewer #1

This paper presents new Holocene slip rates for the Garlock Fault, obtained from mapping offset features and determining luminescence ages of these features. The authors show that the fault has factor of 2-5 variations in fault slip rate, ranging from about 3 mm/yr to 13-14 mm/yr over different periods during the Holocene. Results such as these are important for understanding how fault systems behave, and for characterising fault behaviour to inform seismic hazard assessment.

Overall I found the study to be well done and clearly presented. I have no major issues, but a few minor comments for the authors' consideration.

Thanks for the opportunity to review this paper.

Jonathan Griffin

Main comments

While not a major issue, I do have some questions about why the ages were combined the way they were. The statistical approach for combining ages for the fans shown in Figures 4c, 6c and 9c treats each IRSL sample as a measurement of the exact age of the fan with associated measurement errors, as opposed to measurements of sediments that may be of different ages. This approach results in the narrow uncertainty bounds on the combined distribution, and is reasonable if you believe that the entire stratigraphy exposed in the pit was deposited at the same time (or sufficiently approximately so). However, while it may be possible that all the sediments sampled were deposited in one event (I haven't seen the stratigraphy), from the IRSL results it also seems possible that the fan sediments accumulated over some non-trivial finite period of time (e.g., 500 years in 4c, a few thousand years in 6c), as suggested by the general increase in age with depth of your samples. In this case your IRSL ages aren't necessarily characterising the same point in time, and you have both measurement uncertainties for individual ages and the finite period of fan deposition to contend with. We encountered a somewhat analogous problem in a previous study where we had exposure ages from multiple samples on a fan surface that we expected to be representative of fan age, but not necessarily all deposited at exactly the same time (Griffin et al., 2022). In that study we took Monte Carlo samples of the ages distribution from multiple samples from the same fan, then combined these to estimate the mean and standard deviation from all the samples, resulting in a broader distribution. See Figure 8 in the aforementioned paper. Possibly in this case you may want to favour the shallower samples as more representative of the age of the surface of the fan, which has then been offset. While you state reasons for combining the pdfs in Line 320ff, I am not entirely convinced that the uncertainty bounds obtained on the calculated slip rates fully capture the all the uncertainties associated with the age of the fan. So I suggest some more consideration be given to the assumptions inherent in your approach to determining the age of the fan. Broader characterisation off fan ages and subsequent larger slip rate uncertainties may have some implications for the significance of variations in slip rate presented in this study.

I also note that there is quite a bit of repetition of methodology in how the samples are treated, for example the correction for fading and description of how the ages were combined is repeated for each site. It would be better to have a brief methods description that described the treatments once, and then note any variations for individual sites without the repetition. Similarly, I don't think you need to cite references for OxCal every time it's mentioned, just do it when first described in the methods section.

I like the hypothesis that increased slip rates on the San Andreas will lead to increased slip rates on the Garlock Fault. Any comment on why we might expect changes in slip rate on a major plate boundary like the San Andreas? Presumably plate motions remain relatively constant over the 10kyr timescales considered here.

Section 5.4 is interesting, however I think it somewhat incorrectly characterises how PSHA can deal with variability in earthquake rates. While PSHA typically assume that there is a mean rate of earthquakes on a particular fault, where time-dependent distributions are used (e.g., Weibull, BPT, lognormal etc) then the aperiodicty of the distribution can be used to capture variability in earthquake inter-event times. So PSHAs assume a mean long-term rate, but don't have to assume that this is expressed as constant release rate. While large variations in earthquake rates clearly occur on the Garlock fault, it may be possible to capture these within typically used statistical models through appropriate characterisation of the aperiodicity. E.g., the 2023 New Zealand seismic hazard model (Gerstenberger et al., 2024; Coffey et al, 2024) allowed for higher aperiodicty values than was used in UCERF3 (Field et al., 2015). A more deterministic approach, where the mean was modified, may be possible if we could independently know that the fault is presently in a faster or slower slipping state, but I'm not sure we'd have the confidence to be so deterministic. But if statistical inference could be used to consider the probability of the fault being more or less active, then that could yield improvements.

Minor comments

L66: Here you've used slip-rate; elsewhere slip rate. Please be consistent and check throughout manuscript.

L127: I found the statement 'a 66 ± 6 m offset of Clark Wash incised into an older alluvial fan' ambiguous. Was the offset incised after faulting, or did an incision occur first and was later offset by faulting? I see my question is answered at line 137, but please be clearer here.

L211: Qfc1 needs introducing.

L222. Missing full stop.

L261-264: This needs more explanation. Aren't you trying to understand how slip rates change through time, so why not try to use this more recent offset? Were you unable to date it?

L293; 315: It is a bit confusing to report 2-sigma uncertainties here, 1-sigma uncertainties for the ages (L284; L295) and maximum limits for your offset measurements (L244).

L401: Short not shorty?

L504: Does the stratigraphy show a layered fan structure, or are you relying solely on the ages? Line 518ff seems to suggested there are stratigraphic differences. Better to lead with this first, and then bring in the ages, otherwise you risk looking like you've made stratigraphic interpretations based on the dating, which is not best practice (e.g., what if one of the ages was incorrect).

L560: are your confidence intervals from RISeR actually the limits containing 95% of the distribution, rather than 2-sigma uncertainty (which would normally be symmetrical, if a Gaussian distribution was used). Similarly elsewhere, e.g. L579.

L712: model not models.

L766ff: This sentence could do with splitting up/rewriting, it’s a bit clunky.

Figures

Figure 1. Labels of the fault segments and geographic features (e.g. Death Valley) should have font-size increased.

Figure 10: Suggest plotting the long-term average slip rate as well, so emphasise departures from constant slip rate.

References

Coffey, G.L., Rollins, C., Van Dissen, R.J., Rhoades, D.A., Gerstenberger, M.C., Litchfield, N.J. and Thingbaijam, K.K., 2024. Paleoseismic earthquake recurrence interval derivation for the 2022 revision of the New Zealand National Seismic Hazard Model. *Seismological Research Letters*, *95*(1), pp.78-94.

Field, E.H., Biasi, G.P., Bird, P., Dawson, T.E., Felzer, K.R., Jackson, D.D., Johnson, K.M., Jordan, T.H., Madden, C., Michael, A.J. and Milner, K.R., 2015. Long-term time-dependent probabilities for the third Uniform California Earthquake Rupture Forecast (UCERF3). *Bulletin of the Seismological Society of America*, *105*(2A), pp.511-543.

Gerstenberger, M.C., Van Dissen, R., Rollins, C., DiCaprio, C., Thingbaijim, K.K., Bora, S., Chamberlain, C., Christophersen, A., Coffey, G.L., Ellis, S.M. and Iturrieta, P., 2024. The seismicity rate model for the 2022 Aotearoa New Zealand national seismic hazard model. *Bulletin of the Seismological Society of America*, *114*(1), pp.182-216.

Griffin, J.D., Stirling, M.W., Wilcken, K.M. and Barrell, D.J., 2022. Late Quaternary slip rates for the Hyde and Dunstan faults, southern New Zealand: Implications for strain migration in a slowly deforming continental plate margin. *Tectonics*, *41*(9), p.e2022TC007250.

Comments of Reviewer #2

This is a straightforward manuscript that reports new/updated slip rate estimates for the Garlock fault in southern California. The authors map surficial geology at different slip rate sites and date offset deposits to determine these new slip rates. This manuscript is of interest to the community because it provides additional evidence for variable slip rates through time in complex fault settings.

This manuscript is suitable for Seismica.

It is my opinion that this manuscript be accepted after minor revisions.

General comments:

Abstract is very well done, I appreciate a simple and concise abstract.

IRSL: While I think it is ok to rely on referencing previous papers using IRSL techniques in this area for prep/methods because the use of IRSL is so established in this region, I do think it is useful to include (in the supplement is fine) some basics of what was done to ensure the reliability/consistency of the method. Did you run a dose recovery test, were there any partial bleaching issues, were there any fading problems/was a fading test performed, etc. It would not be out of the question to include a full methods section in the supplement (e.g., Saha et al., 2021).

In an effort for more open analysis, I suggest including the OxCal code(s) used throughout this manuscript in the Supplementary Material.

Line by line comments:

Line 70-73: I recognize the absence of something is harder to demonstrate, but are there any sources that can be provided to show that PSHAs often do not take temporal variability into account?

Line 71/75: I do not have a strong opinion on whether the A in PSHA stands for "Assessment" or "Analysis," but I suggest choosing one and being consistent. It appears that "assessment" is used through the rest of the document. Further, "PSHA" can be introduced in Line 71 and not Line 75.

Line 73-75: It is not clear to me how better constraining slip rates through time helps improve PSHA models if those models cannot use variable slip rates currently. Once those models can better take time into account, improved inputs into those models (i.e., the new slip rates being provided here) will be welcome. I might be misreading this sentence, but to me it reads as having improved slip rates will help better develop the models, instead of people working on the models helping improve the models.

Line 91: The "likely at ca. 11 ka" feels like it was part of the sentence in an older draft that since has been changed. The sentence without that fragment at the end reads well.

Line 203: This is an annotated hillshade that is derived from lidar (likely with a DEM being created from the raw lidar as an intermediate product), a "lidar image" would be an image of the raw point cloud itself. I prefer the phrase "lidar-derived hillshade" for these sorts of hillshades, as they state what the product is (a hillshade) and where it came from (the lidar).

Line 295-297: Was evidence of bioturbation seen in the pit? Are there any other reasons that a younger age population can be found?

Line 330-335: This could be demonstrated with a figure in the Supplementary Material. It would not have to be highly polished, but since numbers are being reported, it needs to be shown that here is virtually no vertical offset.

Line 385-387, & Line 507-509: I suggest including explanations or speculations as to why the ages are out of expected stratigraphic order, as both sets of samples in SRW and CW have ages that are out of order.

Line 456: Figure 8a, not 6a

Line 470: Figure 8a, not 6a

Line 475: Hillshade is one word, not hyphenated.

Line 500-502: Sentence ends abruptly, suggest taking off "using" at the end or otherwise modifying the sentence.

Line 513: Figure 9c, not 9b

Line 537, 544, & Table 2: Sample names taken from McGill et al., 2009 are inconsistently used. McGill et al. states they drop the H14 prefix for discussing samples, but that is not stated in this manuscript. Line 544 still has that prefix, but line 537 and Table 2 drop the prefix (notably, McGill et al. maintain the prefix usage in their radiocarbon table). I do not have strong opinions on whether or not the H14 prefix is maintained or dropped, but I do suggest being more explicit with how sample names are used, especially when they are being taken from other sources.

Line 542: Figure 9d, not Figure 9

Line 626-629: Why is an analytical sampling approach better than MCMC sampling ideal for data sets in which dated markers are independent? A short explanation would be helpful for explaining your methodological reasoning. This is fine to include in the Supplementary Material instead of the main text, but it should be somewhere.

Line 698: Include a citation for the MRE timing.

Line 766-769: The "as suggest by the Garlock fault geodetic-to-geologic rate discrepancy" part of this sentence makes it awkward to read, like there is a part of the sentence missing. I suggest rewriting this sentence in a clearer fashion.

General figure comments:

I would find it helpful if figures showing the minimum and maximum restored back-slip (variations on Figures 3, 5, and 8) are included. As an example, a set of images similar to Figure 5B/C showing both 63 m of back-slip and 77 m of back-slip. It would be fine if they are in the Supplementary Material, but they need to be shown somewhere as they are being used to determine the range of allowable slip, which is then used in slip rate calculations.

I suggest changing "lidar hillshade" for various figure captions (e.g., Figure 3) to "lidar-derived hillshade." This change is in my line by line comments specifically for line 203, but applies to other figure captions as well. I feel that it is important to be very specific about what product is being used as the base topography and where it ultimately comes from.

Individual figure comments:

Figure 1: Please include your source for the fault traces shown.

Figure 1/Figure 10: The PKV site of Rittase et al., 2014 is shown in Figure 1, but Figure 10 shows the Rittase et al. site as SR (Slate Range). I believe Rittase et al. called their site PKV, and so SR in Figure 10 should be changed to reflect that.

Figure 2: Include lat/long graticule so readers know the location of this site. I am not sure what the topographic map (part B) is adding to this figure, as opposed to using the same hillshade from part A to show the fault trace. I suggest adding text that makes what is trying to be shown with that choice clear. See my line 203 comment for a comment on language within the caption.

Figure 5: There is a red line for the fault trace in part C that does not exist for the equivalent part C sub figure sections in Figures 3 or 7. I suggest removing this red fault trace to maintain consistency between the figures.

Figure 7: Include lat/long graticule so readers know the location of this site.e

Editor's Decision on Revised Version

Subject: [Seismica] Editor Decision

Dear Dannielle Fougere, James Dolan, Edward Rhodes, Sally McGill:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Refined Holocene Slip Rate for the Western and Central Segments of the Garlock Fault: Record of Alternating Millennial-Scale Periods of Fast and Slow Fault Slip". Thank you once again for submitting your work to Seismica.

Thank you for taking the time to diligently address the reviewer comments. This is a thorough and considerate revision. I am requesting just a few very minor revisions prior to acceptance. I do not intend to send this manuscript back out for additional peer review.

First, I would like to clarify my previous comments related to PSHA. My intention was not to say that your data do not have implications in that realm. Rather, I was hitting on the point that you state succinctly in your reviewer response - that attempts to incorporate temporal variations in slip/loading rate into forward models without specific and quantitative constraints on those variations will likely result in large uncertainties that effectively eliminate any predictive value of the models. That being said, I am largely satisfied with how you revised the relevant discussion text on that issue. The only suggestion I have is to consider including a more generalized concluding statement along the lines of your concluding comments to me in the reviewer response letter. Specifically, the last paragraph of your reviewer response on this issue is a strong, concise, and clear statement about what we should be doing as a community to improve our approach to seismic hazards assessment.

I also appreciate your attempting to reprocess your data using the approach described in Griffin et al. (2022) per reviewer #2's request. Given that Griffin's approach provided a statistically identical answer to your original estimate, it is of course reasonable to retain the original methodology and results. I will request, however, that you add a sentence in the results section to indicate that you also reprocessed the data using Griffin's approach, but that it yielded indistinguishable results. To be clear, I do not think that you need to include the specific results of the Griffin approach in the

Supplemental material or elsewhere, but simply state that the attempt was made and no difference was found.

This is an exciting manuscript that I look forward to seeing published in Seismica!

When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards
Randy Williams

Response to Reviewers comments for Manuscript "Refined Holocene Slip Rate for the Western and Central Segments of the Garlock Fault: Record of Alternating Millennial-Scale Periods of Fast and Slow Fault Slip"

Dear Editor Williams,

Thank you for considering our manuscript for publication in *Seismica* and for forwarding your comments and those of the reviewers. Please find our responses to the reviewers' comments below. We would like to thank you and both reviewers for your conscientious reviews. We have made the appropriate changes in the manuscript.

The suggestions from you and both reviews helped us to improve our manuscript. All three of you raised questions regarding the impact of these results on probabilistic seismic hazard assessment (PSHA), which we discuss in detail below. Furthermore, we have responded to the reviewers' comments regarding additional discussion of the proposed relationship between activity rates on the San Andreas and their potential to impact rates on the Garlock fault, and comments on calculating IRSL age estimates in our line-by-line responses. We also include an expanded supplementary material file that details the IRSL protocol used in this study. We have also updated the "in revision" citation to reflect its recently published status.

We have included detailed line-by-line responses (blue text) to comments made by the reviewers (black text) explaining specific changes made to the revised manuscript.

Sincerely,

Dannielle Fougere, James Dolan, Edward Rhodes, Sally McGill

Comments from Editor Williams:

The only suggestion I have is to consider including a more generalized concluding statement along the lines of your concluding comments to me in the reviewer response letter. Specifically, the last paragraph of your reviewer response on this issue is a strong, concise, and clear statement about what we should be doing as a community to improve our approach to seismic hazards assessment.

We have added verbiage to *Section 5.4 - Implications for probabilistic seismic hazard assessment* to strengthen the concluding remarks of this section.

I also appreciate your attempting to reprocess your data using the approach described in Griffin et al. (2022) per reviewer #2's request. Given that Griffin's approach provided a statistically identical answer to your original estimate, it is of course reasonable to retain the original methodology and results. I will request, however, that you add a sentence in the results section to indicate that you also reprocessed the data using Griffin's approach, but that it yielded indistinguishable results. To be clear, I do not think that you need to include the specific results of the Griffin approach in the Supplemental material or elsewhere, but simply state that the attempt was made and no difference was found.

We have added a sentence to section *4.1.2 – Age constraints* to reflect this comment

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We have changed the PSHA discussion to include verbiage used in our original review response below.

We respectfully disagree with the statement "*it is perhaps less important that we understand how slip rates have varied over the geological time scales leading up to the present day*". Many PSHA studies that the second author of this paper has worked on over the past couple of decades do not focus on 30-100 year time scales, but rather on 500 and 2500 year (or longer) return periods. Thus, the timescales we discuss in this paper are very much germane to PSHA. As pointed out in your summary comments, our aim is to emphasize that the lack of slip rate and earthquake recurrence data throughout the Holocene does not provide reasonable constraints when using these as basic inputs into seismic source models. This is what we intended but we obviously did not explain ourselves clearly enough. We have revised this part of the text to clarify that our basic point is that what is needed to provide more accurate PSHA is a more accurate characterization of the seismic source model that encompasses realistic variability in the rate of earthquake recurrence. Currently, this basic input into all PSHA is very poorly constrained, and this entire aspect of the PSHA enterprise is in our view one of the biggest potential sources of error, rivaling, or even exceeding, that from errors in strong ground motion prediction models. What is needed are many more data of the sort that we document in this paper.

As detailed by Van Dissen et al. (2020), there are multiple ways of dealing with this unconstrained/barely-constrained aspect of PSHA, including assuming very large bounds on the potential variability in EQ recurrence, as manifest in fault displacement rates. But, treating the variability of future EQ recurrence in this manner yields little predictive value given the large error ranges that result from this approach. If, alternately, we use a single slip rate calculated over some arbitrary displacement range, we risk either under-or-overestimating the hazard based on whether that particular displacement range yields a slip rate that is slower or faster than the current rate of fault slip. (i.e., is that fault correctly experiencing an earthquake cluster or an earthquake lull). As noted above, what is needed at this point are many more data sets like we document here, which will be necessary to develop statistically meaningful ranges on the input parameters used in all PSHA for future EQ recurrence rates and especially the variability of those rates.

Until we generate a statistically meaningful catalog of earthquake-by-earthquake displacements through time on numerous faults in numerous different tectonic settings we cannot accurately (in our view) constrain the most basic input parameter used in PSHA, so we very much think this study and studies like it are indeed a critically important source of information for next generation PSHA.

Reviewer #1 general responses:

IRSL: While I think it is ok to rely on referencing previous papers using IRSL techniques in this area for prep/methods because the use of IRSL is so established in this region, I do think it is useful to include (in the supplement is fine) some basics of what was done to ensure the reliability/consistency of the method. Did you run a dose recovery test, were there any partial bleaching issues, were there any fading problems/was a fading test performed, etc. It would not be out of the question to include a full methods section in the supplement (e.g., Saha et al., 2021).

We have added a new Section 3 to the Supplementary Material that details the methodology used in determining IRSL ages.

In an effort for more open analysis, I suggest including the OxCal code(s) used throughout this manuscript in the Supplementary Material.

OxCal calibration software is available for free online: https://c14.arch.ox.ac.uk/oxcal/OxCal.html

Reviewer #1 line-by-line comments:

Line 70-73: I recognize the absence of something is harder to demonstrate, but are there any sources that can be provided to show that PSHAs often do not take temporal variability into account?

Two examples we cite are Field et al., 2015 and 2017 (Stirling et al., 2012 also assumed this), which assume that the rate of earthquake recurrence is assumed to be constant over timescales of several large earthquakes. In addition, as an example of how considering variable slip rates can affect PSHA, we have added a citation for Van Dissen et al. (2020) to support this idea. Van Dissen et al. (2020) tested the potential impacts of known slip rate variations on probabilistic ground shaking hazard estimation finding variation in Wellington fault slip rate potentially has a significant impact on hazard estimation with a difference of 30-50% in calculated peak ground acceleration (PGA) depending on if the fault was in a “slow” slip rate phase or “fast” slip rate phase.

Line 71/75: I do not have a strong opinion on whether the A in PSHA stands for “Assessment” or “Analysis,” but I suggest choosing one and being consistent. It appears that “assessment” is used through the rest of the document. Further, “PSHA” can be introduced in Line 71 and not Line 75.

Fixed

Line 73-75: It is not clear to me how better constraining slip rates through time helps improve PSHA models if those models cannot use variable slip rates currently. Once those models can better take time into account, improved inputs into those models (i.e., the new slip rates being provided here) will be welcome. I might be misreading this sentence, but to me it reads as having improved slip rates will help better develop the models, instead of people working on the models helping improve the models.

There are as yet far too few data to apply to these models. Indeed, generating the type of data needed is the entire purpose of this study. See the PSHA discussion above. The bottom line is that the modelers cannot improve their models without real-world constraints on what kind of variability real faults exhibit.

Line 91: The "likely at ca. 11 ka" feels like it was part of the sentence in an older draft that since has been changed. The sentence without that fragment at the end reads well.

Typo. That should have been Ma.

Line 203: This is an annotated hillshade that is derived from lidar (likely with a DEM being created from the raw lidar as an intermediate product), a "lidar image" would be an image of the raw point cloud itself. I prefer the phrase "lidar-derived hillshade" for these sorts of hillshades, as they state what the product is (a hillshade) and where it came from (the lidar).

Fixed.

Line 295-297: Was evidence of bioturbation seen in the pit? Are there any other reasons that a younger age population can be found?

The single-grain plots for the two lower samples at this site (SRE14-03 and SRE14-04) look very secure, but note each of these has one or two younger intrusive grains, even down to 95 cm depth, indicating that bioturbation probably occurred at this location. Additionally, there was evidence of contemporary bioturbation after observing numerous active ant nests at the Summit Range East site. Other reasons that could lead to two age populations being found are partial bleaching (would show an older age population), and other post-depositional changes (e.g., re-exposure to light after erosion, human activity).

Line 330-335: This could be demonstrated with a figure in the Supplementary Material. It would not have to be highly polished, but since numbers are being reported, it needs to be shown that here is virtually no vertical offset.

Added Figure S2 to Supplementary Material to show there is virtually no vertical separation at the Summit Range East site.

Line 385-387, & Line 507-509: I suggest including explanations or speculations as to why the ages are out of expected stratigraphic order, as both sets of samples in SRW and CW have ages that are out of order.

Stratigraphic inversions exist for IRSL sample ages at all three sites. This is not an issue as these out-of-stratigraphic order age estimates overlap within 2-sigma uncertainty with adjacent sample age estimates.

Line 456: Figure 8a, not 6a

Fixed

Line 470: Figure 8a, not 6

Fixed.

Line 475: Hillshade is one word, not hyphenated.

Fixed.

Line 500-502: Sentence ends abruptly, suggest taking off "using" at the end or otherwise modifying the sentence.

Fixed.

Line 513: Figure 9c, not 9b

Fixed.

Line 537, 544, & Table 2: Sample names taken from McGill et al., 2009 are inconsistently used. McGill et al. states they drop the H14 prefix for discussing samples, but that is not stated in this manuscript. Line 544 still has that prefix, but line 537 and Table 2 drop the prefix (notably, McGill et al. maintain the prefix usage in their radiocarbon table). I do not have strong opinions on whether or not the H14 prefix is maintained or dropped, but I do suggest being more explicit with how sample names are used, especially when they are being taken from other sources.

Added H14 prefix to sample IDs to be consistent with McGill et al. (2009).

Line 542: Figure 9d, not Figure 9

Fixed.

Line 626-629: Why is an analytical sampling approach better than MCMC sampling ideal for data sets in which dated markers are independent? A short explanation would be helpful for explaining your methodological reasoning. This is fine to include in the Supplementary Material instead of the main text, but it should be somewhere.

The decision to use an analytical sampling approach rather than MCMC sampling is explained in the Supplementary Material Section 1.1. We also calculated incremental slip rates for a dated fault slip history using RISeR's Markov Chain Monte Carlo (MCMC) formulation (Supplementary Material Section 1.2) that yielded relatively similar incremental slip rates and uncertainties.

Line 698: Include a citation for the MRE timing.

Fixed.

Line 766-769: The "as suggest by the Garlock fault geodetic-to-geologic rate discrepancy" part of this sentence makes it awkward to read, like there is a part of the sentence missing. I suggest rewriting this sentence in a clearer fashion.

Changed to - if rates of elastic strain accumulation also vary through time, as suggested by the geodetic-to-geologic rate discrepancy for the Garlock fault, then PSHA based on the assumption of constant strain accumulation and release may not provide useful forecasts of near-future fault behavior. Gauriau and Dolan (2024) have shown that such geologic-to-geodetic discrepancies may be typical for faults like the Garlock fault that are embedded within in complex plate boundary fault systems.

General figure comments – I would find it helpful if figures showing the minimum and maximum restored back-slip (variations on Figures 3, 5, and 8) are included. As an example, a set of images similar to Figure 5B/C showing both 63 m of back-slip and 77 m of back-slip. It would be fine if they are in the Supplementary Material, but they need to be shown somewhere as they are being used to determine the range of allowable slip, which is then used in slip rate calculations.

Added Figures S6, S7, and S8 to Supplementary Material to show minimum and maximum offset restorations

Figure 1: Please include your source for the fault traces shown.

Added "Quaternary fault traces sourced from U.S. Geological Survey and California Geological Survey, Quaternary fault and fold database for the United States, accessed February 1, 2023, at: https://www.usgs.gov/natural-hazards/earthquake-hazards/faults."

Figure 1/Figure 10: The PKV site of Rittase et al., 2014 is shown in Figure 1, but Figure 10 shows the Rittase et al. site as SR (Slate Range). I believe Rittase et al. called their site PKV, and so SR in Figure 10 should be changed to reflect that.

Fixed.

Figure 2: Include lat/long graticule so readers know the location of this site. I am not sure what the topographic map (part B) is adding to this figure, as opposed to using the same hillshade from part A to show the fault trace. I suggest adding text that makes what is trying to be shown with that choice clear. See my line 203 comment for a comment on language within the caption.

Lat/long added in the caption for each site.

Figure 5: There is a red line for the fault trace in part C that does not exist for the equivalent part C sub figure sections in Figures 3 or 7. I suggest removing this red fault trace to maintain consistency between the figures.

Fixed.

Figure 7: Include lat/long graticule so readers know the location of this site.

Fixed.

Reviewer #2 main comments:

IRSL age combination

While not a major issue, I do have some questions about why the ages were combined the way they were. The statistical approach for combining ages for the fans shown in Figures 4c, 6c and 9c treats each IRSL sample as a measurement of the exact age of the fan with associated measurement errors, as opposed to measurements of sediments that may be of different ages. This approach results in the narrow uncertainty bounds on the combined distribution, and is reasonable if you believe that the entire stratigraphy exposed in the pit was deposited at the same time (or sufficiently approximately so). However, while it may be possible that all the sediments sampled were deposited in one event (I haven't seen the stratigraphy), from the IRSL results it also seems possible that the fan sediments accumulated over some non-trivial finite period of time (e.g., 500 years in 4c, a few thousand years in 6c), as suggested by the general increase in age with depth of your samples.

We combined the three youngest samples from the SRE site using the same MC method yielding an age of 5,570 ± 190 years, slightly younger and better constrained than when all four samples were included of 5675 ± 356 years. The 5,570 ± 190-year estimate yielded a slip rate of 6.9 ± 0.3 mm/yr, slightly (0.2 mm/yr) faster than when all four samples were included and within error of our preferred rate based on these samples. We agree that using the three youngest samples is more representative of the geomorphic feature we use to measure displacement. This has been updated in the text.

In this case your IRSL ages aren't necessarily characterizing the same point in time, and you have both measurement uncertainties for individual ages and the finite period of fan deposition to contend with. We encountered a somewhat analogous problem in a previous study where we had exposure ages from multiple samples on a fan surface that we expected to be representative of fan age, but not necessarily all deposited at exactly the same time (Griffin et al., 2022). In that study we took Monte Carlo samples of the age distribution from multiple samples from the same fan, then combined these to estimate the mean and standard deviation from all the samples, resulting in a broader distribution. See Figure 8 in the aforementioned paper. Possibly in this case you may want to favor the shallower samples as more representative of the age of the surface of the fan, which has then been offset. While you state reasons for combining the pdfs in Line 320ff, I am not entirely convinced that the uncertainty bounds obtained on the calculated slip rates fully capture all the uncertainties associated with the age of the fan. So I suggest some more consideration be given to the assumptions inherent in your approach to determining the age of the fan. Broader characterisation of fan ages and subsequent larger slip rate uncertainties may have some implications for the significance of variations in slip rate presented in this study.

We combined ages for the SRE site using the same method as Figure 8 in Griffin et al. (2022) by taking 10,000 Monte Carlo samples from each IRSL age distribution, and then taking the mean and standard deviation of the combined samples from all four ages, assuming they come from a normal distribution. Using the above MC method yielded 5675 ± 356 yb2014 (all reported 2-σ uncertainties) which is almost identical to the reported combined value using a chi-squared test performed on OxCal 4.4 of 5,680 ± 340 yb2014. The Clark Wash site ages yielded similar results using the MC method of 8003 ± 584 years versus OxCal of 8095 ± 575 years. We therefore retain our original methodology and results. We have added a citation for Griffin et al. (2022).

I also note that there is quite a bit of repetition of methodology in how the samples are treated, for example the correction for fading and description of how the ages were combined is repeated for each site. It would be better to have a brief method description that described the treatments once, and then note any variations for individual sites without the repetition. Similarly, I don’t think you need to cite references for OxCal every time it’s mentioned, just do it when first described in the methods section.

We disagree, we think it's important to be specific about reporting so we retain our original citations.

I like the hypothesis that increased slip rates on the San Andreas will lead to increased slip rates on the Garlock Fault. Any comment on why we might expect changes in slip rate on a major plate boundary like the San Andreas? Presumably plate motions remain relatively constant over the 10 kyr timescales considered here.

The main purpose of this paper is to document our incremental slip rate studies along the Garlock fault. The type of detailed discussion about potential causative mechanisms is detailed in two recent papers (Dolan et al., 2024 [EPSL] and Gauriau and Dolan, 2024 [Seismica]). In addition to those recent papers, we do plan to discuss these issues in more detail in a future paper focused on a comprehensive analysis of all earthquake recurrence and fault slip rate data from the Garlock, the Mojave section of the San Andreas, and the faults of the ECSZ. But, these studies are not yet complete, so such a discussion would be premature in this paper.

Reviewer #2 line-by-line comments:

L66: Here you've used slip-rate; elsewhere slip rate. Please be consistent and check throughout the manuscript.

Fixed.

L127: I found the statement 'a 66 ± 6 m offset of Clark Wash incised into an older alluvial fan' ambiguous. Was the offset incised after faulting, or did an incision occur first and was later offset by faulting? I see my question is answered at line 137, but please be clearer here.

Changed sentence to "a 66 ± 6 m offset of an older alluvial fan into which Clark Wash has subsequently incised".

L211: Qfc1 needs introducing.

Moved sentences in paragraph around.

L222. Missing full stop.

Fixed.

L261-264: This needs more explanation. Aren't you trying to understand how slip rates change through time, so why not try to use this more recent offset? Were you unable to date it?

That is correct, we were unable to date the smaller (and therefore younger) offset.

L293; 315: It is a bit confusing to report 2-sigma uncertainties here, 1-sigma uncertainties for the ages (L284; L295) and maximum limits for your offset measurements (L244).

IRSL age estimates are reported with 1-sigma uncertainties. 2-sigma uncertainties are reported when they are combined to get a representative age for an alluvial deposit.

L401: Short not shorty?
Fixed.

L504: Does the stratigraphy show a layered fan structure, or are you relying solely on the ages? Line 518ff seems to suggested there are stratigraphic differences. Better to lead with this first, and then bring in the ages, otherwise you risk looking like you've made stratigraphic interpretations based on the dating, which is not best practice (e.g., what if one of the ages was incorrect).

As shown in Figure 4a, the fan locally does exhibit crude depositional stratigraphy, but much of the section is massive.

Figure to left - SRE14-01 to 04. The gamma spectrometer is in the position of SRE14-02, and sample SRE14-04 is still in position, prior to extraction and capping. There are no clear stratigraphic boundaries, though the top sample was collected in a coarser layer of angular pebbles

L560: are your confidence intervals from RISeR actually the limits containing 95% of the distribution, rather than 2-sigma uncertainty (which would normally be symmetrical, if a Gaussian distribution was used). Similarly elsewhere, e.g. L579.

The confidence intervals from RISeR are the limits containing 95% of the distribution, updated in text.

L712: model not models.

Fixed.

L766ff: This sentence could do with splitting up/rewriting, it's a bit clunky.

Changed to "If rates of elastic strain accumulation also vary through time, as suggested by the geodetic-to-geologic rate discrepancy for the Garlock fault, then PSHA models based on the assumption of constant strain accumulation and release may not provide a useful forecasts prediction of near-future fault behavior."

Figure 1. Labels of the fault segments and geographic features (e.g. Death Valley) should have font-size increased.

Fixed.

Figure 10: Suggest plotting the long-term average slip rate as well, so emphasise departures from constant slip rate.

See Supplementary Material Figure S7 for this figure.