Reviewer Comments

For author and editor

This is an excellently written paper that is easy to read and to follow the scientific results and subsequent discussion. There are two broad areas where I feel that the manuscript needs some changes to better justify assumptions and to ensure a broader discussion of prior literature on the topic. I have outlined these two areas below, followed by a few more comments throughout the manuscript.

Section 3

What time interval are geodetic rates calculated over? As you give the time intervals used to calculate the small- and large-displacement rates, I think you should also give the intervals for the geodetic rates for completeness.

Is there a minimum length of time that the geodetic community hypothesises is long enough to accurately characterise the strain accumulation? Can the findings of your paper be used to support or challenge any assumptions on this?

There is an implicit assumption that the strain accumulation is uniform throughout the interseismic period (after neglecting post-seismic effects). There is a study (Hussain et al., 2018) from the North Anatolian fault that should be cited to support this assumption – these authors found that the strain accumulation is constant over a range of time intervals since the most recent event.

Section 6

I broadly agree with the authors that variations in strain accumulation rates are likely to be a result of variable shear rate on ductile shear zones, partly because I've recently published on this (Mildon et al., 2022)! Others have also published on this hypothesis, for example (Oskin et al., 2008), and variable creep rates have also been numerically modelled (e.g. (Ellis & Stöckhert, 2004)), so these contributions to the topic should also be cited in the discussion.

However there are some alternatives that could be discussed. For example, temporal clustering of earthquakes is observed in many tectonic settings. Perhaps higher geodetic strain rates are caused by a fault experiencing a cluster of events? This topic is alluded to in the manuscript by considering the time since the Most Recent Event, but this is not related to the measured/hypothesised recurrence intervals. Furthermore, could fault interaction via Coulomb Stress Transfer explain any of the observations in the manuscript?

Other comments:

Line 97 – could you give further explanation how the CoCo values are standardized by plate motion. Furthermore on line 154, it says that a standardized CoCo value is used, but no explanation is provided to justify this exact value – please give further explanation.

Figure 3 is referred to in the text before Figure 2.

Figure 2 – I don't see the value is separating the data into Lower and Higher CoCo faults (panels b and c). I'd suggest keeping panel a and making it larger.

Lines 161-171 – I found this paragraph difficult to understand. I think what is being said is that the blue dots representing lower CoCo values broadly plot along the 1:1 line, whereas there is more scatter in the orange-red dots representing higher CoCo values. If I have understood this correctly, I suggest rewording this paragraph to make is easier for the reader to follow the reasoning.

Figure 3 – the caption says “CST refers to “current shortest-term” rates”, but I cannot see this on Figure 3.

Line 216 – do the coefficient of determination values come from the data plotted in Figure S1? If this is the case, add the values to Figure S1 and add a figure reference in the text.

Line 243 – “assumption used in numerous studies” can you give some examples/references of where this assumption is used?

References

Ellis, S., & Stöckhert, B. (2004). Imposed strain localization in the lower crust on seismic timescales. *Earth, Planets Sp.*, *56*(12), 1103–1109. https://doi.org/10.1186/BF03353329

Hussain, E., Wright, T. J., Walters, R. J., Bekaert, D. P. S., Lloyd, R., & Hooper, A. (2018). Constant strain accumulation rate between major earthquakes on the North Anatolian Fault. *Nat. Commun.*, *9*(1), 1–9. https://doi.org/10.1038/s41467-018-03739-2

Mildon, Z. K., Roberts, G. P., Faure Walker, J. P., Beck, J., Papanikolaou, I., Michetti, A. M., et al. (2022). Surface faulting earthquake clustering controlled by fault and shear-zone interactions. *Nature Communications*, *13*(1), 7126. https://doi.org/10.1038/s41467-022-34821-5

Oskin, M., Perg, L., Shelef, E., Strane, M., Gurney, E., Singer, B., & Zhang, X. (2008). Elevated shear zone loading rate during an earthquake cluster in eastern California. *Geology*, *36*(6), 507. https://doi.org/10.1130/G24814A.1

Rousset, B., Jolivet, R., Simons, M., Lasserre, C., Riel, B., Milillo, P., et al. (2016). An aseismic slip transient on the North Anatolian Fault. *Geophysical Research Letters*, *43*(7), 3254–3262. https://doi.org/10.1002/2016GL068250

Review of "*Variability of elastic strain accumulation and release rates on strike-slip faults is controlled by the relative structural complexity of their surrounding plate-boundary fault systems*" by Gauriau et al., submitted to Seismica

Reviewer: Dr Sam Wimpenny, University of Bristol

Paper Overview:

Gauriau et al., present a metadata analysis of geodetic strain accumulation rates and geological strain release rates from time-averaged fault slip on major strike-slip faults. Similar analyses have been performed previously, but the novel component of the author's analysis is that they consider these measurements in the context of the structural complexity of the fault systems. Their key finding is that faults that sit within more complex fault networks experience more temporally variable strain accumulation and release rates, whilst isolated faults that accommodate most of the relative plate motion have strain accumulation and release rates that are more stable through time. The authors present a range of explanations that could account for greater variability in the rates of strain accumulation and release occurring preferentially within more complex fault networks, most of which centre around the mechanical interactions between faults.

The manuscript does not present any new data. The figures are clear, and the text is well written and relatively easy to follow. There are places where I believe the text can be shortened to focus more on the key results, as opposed to summarising previous work, which could help with making the author's arguments clearer. Similarly, the authors often combine the presentation of observations with mechanical interpretations, and I think the manuscript would benefit from separating these out to make the case of how their data lead to their conclusions clearer. I have tried to highlight where I believe these changes could be made in the line-by-line review.

I have no major methodological concerns with the analyses presented – they mainly build upon a previous article [Gauriau and Dolan, 2021]. I do have some questions about the sensitivity of the Coefficient of Complexity summation to the uncertainties in slip rate estimates, which could be easily addressed with a simple calculation.

Overall, the manuscript contributes an interesting explanation for the mismatch between geological estimates of fault slip rates (which reflect time-averaged elastic strain release) and geodetic estimates of elastic strain accumulation rates. There have typically been two mechanisms proposed to explain these discrepancies: (1) time-variation in strain accumulation rates, and (2) time-variation in fault strength. The key implication, at least in my view, is that fault strengthening/weakening may play a secondary role in controlling the timing of earthquakes, because the many of the strengthening/weakening processes should operate on faults irrespective of whether they sit within a complex or simple fault network. I agree with the author's conclusions they draw from the data, but some slight edits to the manuscript are needed to tidy up how this conclusion is reached from the geodetic/geologic data. Considering these comments, **I recommend that this manuscript could be published after addressing these minor corrections.**

General Comments:

1. **Implicit assumption that geodetic/geologic rate differences reflect temporal variability in elastic strain accumulation:**

Throughout the paper, the authors state that the difference between the geological/geodetic rate estimates “must” reflect differences in the rate of elastic strain accumulation through time. There are some assumptions in this logic, which I believe are reasonable, but that need clarifying. The geodetic slip deficit rate I agree is a measure of the rate at which strain is accumulating around the fault. The fault slip rate is a measure of the rate of strain release on the fault. Strain release does not have to be the same as strain accumulation, because temporal variability in the amount of shear strain needed to exert a shear stress on the fault to cause it to rupture could create periods of accelerated or decelerated strain release. The authors do touch on this briefly, but I think the assumption needs to be more explicit.

The implicit assumption is that a fault can only store a finite amount of elastic strain – roughly enough to generate a few tens of metres of slip. Therefore, slip rate estimates that derive from offset landforms with small displacements could be affected by periods of accelerated or decelerated strain release, as the “crustal strain capacitor” (in the author’s jargon) is either being discharged or charged. Small-displacement slip rate estimates can therefore be affected by temporal variability in strain accumulation rate, as well as changes in fault strength that cause the stored elastic strain to be released.

Slip rates estimated from offset landforms with greater than a few tens of metres of slip should have averaged out these periods of clustered strain release, as a fault cannot store enough elastic strain to create hundreds of metres of fault slip solely through weakening the fault zone. This seems reasonable given the strain limit of elastic materials like rocks is on the order of $\sim 10^{-3}$ (i.e. the rock surrounding the fault would just break when the slip deficit to length ratio u/L exceeds $\sim 10^{-3}$). Therefore, the only way to explain why the long-term rate of strain release differs from the decadal strain accumulation would be to have temporal variability in the strain accumulation rates.

As I say – I agree with the author’s interpretation of the data, I just think that it is worth clarifying the assumptions (if they agree with me) stated above early in the manuscript. This will help the reader establish how strain accumulation rates might relate to slip rates, as I found myself asking “what about changes in fault strength?” multiple times whilst reading manuscript.

2. **Recognising that many of the proposed rheological mechanisms for temporal variability in earthquake timing should operate on faults irrespective of structural complexity of the network within which it is contained:**

   This is a point I think the authors could add more explicitly somewhere, which is that if fault weakening mechanisms (e.g. fluid incursion into the fault zone) were to effect all fault zones, then we should see discrepancies between the geodetic and small-displacement geologic rates irrespective of the complexity of the fault network. Their compilation does not show this, and therefore temporal variability in strain accumulation rates seem the most likely explanation for the patterns in their data.

3. **The authors variably use slip-deficit rates, slip rates, strain accumulation rates and strain release rates throughout the text.**

   I would recommend just sticking to using the terms strain accumulation and strain release, providing the caveat that the fault slip rate estimates provide the time-average strain release rate somewhere early in the manuscript. Alternatively, the authors could use “slip rate” and “slip deficit rate” – but maybe try to stick to one or the other.

4. **Maybe this is pedantic, but I do not think the Coefficient of Complexity is a coefficient, as far as I understand that word. A coefficient is a multiplicative factor that comes before a variable (e.g., 4*pi*r, where 4*pi is the coefficient and r is the radius for example).**

   In this case, there is no variable that comes after the coefficient, so it is a metric. I know this does not change anything and is just semantic, but worth considering.

5. **Uncertainty on the Coefficient of Complexity:**

   The one methodological query I have regarding the manuscript is whether the algorithm the authors use for computing the coefficient of complexity is sensitive to the uncertainties in fault slip rates within the radius of interest. I would recommend the following test: (1) establish the range of possible slip rates for each fault, (2) define some form of probability distribution for each fault slip range [i.e. Gaussian if the slip rate is a±b where b is the standard deviation, or just uniform distribution if you have a range a–b], (3) perform multiple iterations of the CoCo calculation that uses the slip rates sampled from these distributions, (4) construct the resulting distribution of CoCo values. From this, the authors will get an estimate of either the range (if using uniform distributions) or standard deviation (if using Gaussian distributions) of the CoCo, and could put the error bars onto Figure 3a. It may well be that it has very little effect, but this seems important for determining whether the trends in CoCo are robust or not.

Line-by-Line Review:

Line 27-29: What the authors mean by “mechanically complementary” is a little unclear here – does it mean they slip in response to the same stress field, or do they mean “kinematically complementary” in that they accommodate different components of the displacement field? I would recommend sticking with just “*complex fault systems characterised by multiple faults accommodating the deformation field, which we refer to as ….”* – this more accurately reflects the data analysis.

Line 31-33: “rates of ductile shear-zone roots also vary through time” – re-phrase this to “… that the rate of ductile shear beneath the seismogenic portion of faults also varies through time.”

Line 37: Change “… relatively constant fault slip rates …” to “ … relatively stable fault slip rates …”

Line 56: I am not sure what Seismica’s policy is over citing unpublished or in review articles, but I would recommend avoiding this practice. Just because you have plenty of examples that are already published to make the point here. This applies throughout the manuscript too – there are lots of places where the text relies on in review or in preparation work. In these places, just give a brief summary of the arguments they present, such that the reviewer (and potentially reader) can follow the logic without needing these papers.

Line 73: There is a colon where I think there should be a period. “… different geological timescales. Faults that lie…”

Line 75-76: I would recommend some slight re-wording here to highlight that this study is motivated by the results of Gauriau and Dolan, [2021]. I was not familiar with this work, but on reading it then it becomes clear where this manuscript has emerged from, and I think other readers would benefit from this context too. Consider re-wording to something like:

"Unlike estimates of geological slip rates, which reflect the rate at which elastic strain is released on a fault averaged over time scales of thousands of years, geodetic measurements of deformation around fault zones can be used to infer the rates of elastic strain accumulation over a time-period of years. The rate of elastic strain accumulation is often expressed as a 'slip deficit rate' on the fault where it is locked in the shallow crust. Meade et al., (2013) compared geological fault slip rates with geodetic slip deficit rates from 15 major continental strike-slip faults and fault that, as an ensemble, these faults exhibit a near 1:1 relationship (with a …) between the geological and geodetic rates. Slight differences between the datasets could be attributable to short-lived periods of higher-than-average strain accumulation during the post-seismic period.

The geological rates used as inputs into the analysis of Meade et al., (2013) are derived from landform offsets that span a large range of displacements (13 m to 600 m) and ages (2 kyr to 160 kyr). We recently presented results that demonstrate that geological estimates of slip rates vary depending on the displacement scale over which the slip rate is estimated, particularly on faults that form part of a network of closely-spaced faults accommodating a given deformation field [Gauriau and Dolan, 2021]. Isolated faults that accommodate the majority of the deformation field exhibit more steady slip rates [Gauriau and Dolan, 2021]. Therefore, it is possible that differences between geodetic and geological estimates of fault slip rates are also sensitive to the complexity of the fault network.

In this paper, …"

This is just a suggestion, but it is worth highlighting the importance of the author's earlier work here, even with some other edits.

Table 1: The slip-rate and age estimates are presented in a range of different formats, including: a±b, a-b, and with a range of different decimal places. All of this may well be deliberate to reflect varying amounts of precision and various bounds on the ages of landforms that are offset, but this is not clear from the table description. I'd recommend either explaining why the formats are different or present them in a consistent format.

Line 122: The title of this subsection does not seem grammatically quote correct, and it does not give much information about what's contained within the following paragraphs. Maybe consider a new title for this subsection? Possibly "Selection of Geodetic Estimates of Slip-Deficit Rates"?

Line 184-185: This is an example of where the interpretation (first sentence) comes before the description of the observations. The issue with doing it this way around is that it was not immediately clear why the loading rate (and not, say, the fault strength) must vary through time to explain why there is a different between the geologic-geodetic rate scaling. I'd recommend removing this sentence, or move it to somewhere in Section 5, to more clearly separate observations from interpretations.

Line 212-214: Here is another place where the mechanical interpretation is mixed in with the description of the observations. I'd recommend removing this sentence or move it to somewhere in Section 5.

Line 221: spelling errors, should read: "… than …"

Figure 3: I like this figure – it really captures the key point the authors are trying to make regarding the dependence of geodetic/geologic slip rates on the CoCo values. However, one thing I don't necessarily understand, is why there is such a sharp change in the geodetic/geologic rate from ~1:1 at CoCo > 0.002 – if mechanical interaction were important then wouldn't we expect to see more of a continuous variation with increasing CoCo? It is possible that there are just too few examples to properly delineate the trends, and that the result is somewhat sensitive to the radius used to compute the CoCo, but this is just a thought for the authors to consider.

Section 5: I would recommend just adding a little more in the way of explanation as to why the relationship between CoCo and geodetic/geologic slip rate indicates time-dependent variability in the rate of elastic strain accumulation in this section. I'd recommend this to be the place to address my "General Comment" about clarifying the assumptions in the study.

Line 262: Replace colon with a period.

Line 299: I would recommend not referring to the 'in review' paper, and rather just explain explicitly what is meant by the "crustal strain capacitor". If it is the idea that faults only release part of the elastic strain stored in their surrounding wall rocks, and therefore multiple episodes of strain release can occur that do not necessarily balance the strain accumulation, this is not necessarily a new idea and other citations could be found [e.g. Mencin et al., 2016].

Line 300: Is it not the shear strain that is stored within the crust, which exerts a shear stress onto the fault plane?

Lines 280-303: Here time-dependent changes in fault strength are suggested to play an important role in modulating the slip rate when viewed over a small number of earthquake cycles. What has not been addressed at this point is why the authors think that such time-dependent changes in fault strength, which I agree have the capacity to lead to pulses of a few earthquakes and therefore strain release, are not to blame for the differences between the geodetic and long-term geological rate discrepancies. Elsewhere the authors state that the differences "must" be caused by time variability in the rate of elastic strain accumulation – I have not yet seen the argument (in this manuscript at least) for why this is the case. See my point in the "General Comments" section.

Line 315: Change to "… as the dashed arrows in Figure 3a illustrate"

Line 330: Change to "… the lack of significant coseismic slip in 2016 on the Hope Fault…"

Line 407: Replace "SM" with "Supplementary Material"

Line 415-419: This sentence provides the key thesis of the paper – though I am still not entirely sure that it follows from the preceding paragraphs, unless we make the assumption that the large-displacement slip rate estimates represent the time-average strain accumulation rate and not the time-average strain release rate that is affected by changes in fault strength.

Section 6.1: Much of the discussion here relies on references to, as yet, unpublished works. I'd recommend either pre-printing these papers so they can be properly cited (as there's no way as a reviewer for me to check what these papers say in them), or just explain what is said in those pre-published papers.

Line 466: Please could the authors explain what is meant by "the primary fault is forces to slip at small-displacement scales"? My reading of this would be that tectonically isolated faults slip with small amounts, but I think the authors mean something else.

Line 493: Not sure the "(and currently missing?)" is needed here – I would suggest just removing it.

Section 7: The argument here is qualitative, referring to "higher" and "lower" future seismic hazard. I think the authors make a clear conceptual point about how geologic slip rate estimates might under/over predict hazard, but it is not clear how the methodology presented could be implemented in a quantitative way into the PSHA framework.

Conclusions: The conclusions are mostly a repeat of the findings from the main manuscript and could be shortened somewhat to extract the key points. As a reader, I always think short and sharp conclusions are the most effective, but I will leave it up to the authors to decide on the format.

Review of Supplementary Materials:

Section 1: Typo where colons are followed by a capital letter – should read "… brought. Instead of using … "

Section 1: I am not entirely sure why the slip rates are assigned a median within the centre of a given range in the CoCo calculation, as opposed to just using the range in fault slip rate (or uncertainty) and propagating these through the summation to get a CoCo estimate plus its range/uncertainty. Maybe I am missing something here. Having read Gauriau and Dolan 2021 it seems to me that this approach could neglect the broad uncertainties associated with slip rate estimates, which can be large, and might therefore lead to CoCo estimates that could have large uncertainties too. See my comment in the "General Comments" section.

Section 3: Change section header to "Comparison of Geodetic Rates with Geologic Rates"

Section 6: The text explanation for how the authors calculated the dispersion of the data was difficult to follow. I think it would benefit from explaining the calculation using formulae, or the authors could also simply rely on the visually clear change in the dispersion of the data from the Figures to make their point, particularly given that the dispersion metric has no obvious physical meaning or statistical relevance.

Section 7: I cannot find Figure 1c, presumably this is a typo and refers to Figure 3c given that the section explains why the authors think their analysis is not biased by absolute slip rate? This piece of text is also so short it could well be put in the main text.

References:

Mencin, D., Bendick, R., Upreti, B. *et al.* Himalayan strain reservoir inferred from limited afterslip following the Gorkha earthquake. *Nature Geosci* **9**, 533–537 (2016). https://doi.org/10.1038/ngeo2734

<u>Response to Reviewer's comments for Manuscript</u>

"Comparison of geodetic slip-deficit and geologic fault slip rates reveals that variability of elastic strain accumulation and release rates on strike-slip faults is controlled by the relative structural complexity of their surrounding plate-boundary fault systems"

Los Angeles, December 6, 2023

Dear Editor Fagereng,

Thank you for considering our manuscript and sharing with us your thoughts. Thank you also for allowing us a few last-minute changes regarding new data we wanted to include in our analysis. Please find below our response to both your summary of the comments and the reviewers' specific comments. We would like to thank the two reviewers for their very thoughtful and constructive reviews. We have adopted most of the clarifications and changes they suggested. Their comments improved the effectiveness of the original manuscript, and we think that they helped make it a better paper.

For your convenience, we have included a paragraph that summarizes the general changes performed, followed by your comments and our responses, and then the reviewers' comments with our point-by-point replies to each reviewer comment (black text) with our responses (blue text) detailing specific changes that have been made to the revised manuscript, as well as the related line numbers.

Sincerely,

Judith Gauriau* and James Dolan

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General changes to the manuscript

The main changes to the text relate to one of the main comments from Reviewer Wimpenny (his comment #1), regarding a more thorough description of the reasoning behind our interpretation of our results.

We have split our original section 3 into two sections (3 and 4), to better reflect what was contained in the original two paragraphs that were forming section 3.

We have slightly modified our original title ("*Variability of elastic strain accumulation and release rates on strike-slip faults is controlled by the relative structural complexity of their surrounding plate-boundary fault systems"*) to reflect Review Wimpenny's concerns about our use of different terms in the original submission concerning geodetic slip-deficit rates and geologic fault slip rates.

We have added a new datum to our analysis, coming from the Pazarcık segment of the East Anatolian fault (EAF), for which a recent paper (Yönlü and Karabacak, 2023) highlights a long-term slip rate of 5.6 mm/yr. We thought that adding this datum would provide additional support for our case, since this section of the EAF ruptured in February 2023, whereas geodetic slip-deficit data (of 10.3 mm/yr) were acquired before the earthquake, suggesting that the fault was in what we refer to as a "fast mode". That might indicate a higher near-future likelihood of earthquake occurrence.

Figures:

- Figures 2, 3, 5: We replaced "geodetic rate" by "geodetic slip-deficit rate"
- Figures 2, 3: the new datum has been added (numbered 24)
- Figure S1: We added the coefficients of determination on each graph as suggested by reviewer Mildon.
- Figure S2: New figure added in the Supplementary materials to better explain our calculation of the dispersion of data shown in Figure 3 of the main text (in response to one of Reviewer Wimpenny's comments).

Please find below our responses (in blue) to both the suggestions you provided in your email (received on November 6, 2023) and the reviewers' comments. Whenever we refer to lines where changes have been made, they are the lines from the current file with tracked changes.

Answers to editor's comments

As said by both reviewers, the paper is very well written, makes a compelling case, but can be improved by clarifying a few points. I think the reviewers' reports are constructive and very clear, so there is little for me to add. I will however, specify two points, which were also noted by the reviewers, that I was left unclear about after reading the paper and that could do with a bit more explanation:

1) Like Reviewer A's comment on Section 6, I also wondered about alternatives to requiring variable shear rate on underlying viscous shear zones – I agree it is a valid interpretation, but I was also thinking about stress interaction between different fault strands on complex faults as an option for variable strain

accumulation. I feel the interpretation regarding the role of shear zones below the seismogenic zone needs a bit more discussion.

The stress interactions between fault strands in complex systems have been discussed in and forms the whole point of Dolan et al. (2007), as well as our original paper Gauriau & Dolan 2021.

On that note, there are additional references that may be useful (but I am not demanding all this is cited), in particular a rich literature exists on variable strength/rheology/shearing rate in ductile shear zones (Section 6) so the current reliance on self-citations is not necessary. In addition to suggestions by Reviewer A, there are for example Mark Handy and his group's work in the 1990s and 2000s, Jordi Carrera and colleagues on shear zone networks, and Mancktelow and Pennacchioni (with others) on roles of existing heterogeneities.

Thank you for these suggestions. Our paper is not aimed at detailing the many potential mechanisms occurring within a ductile shear zone, and this complex topic will be the focus of a companion paper to our submitted manuscript by Tarryn Cawood (Cawood and Dolan, in prep). However, we have added references to a few key papers, as suggested in your comment. See lines 493-496 for the related changes. We also note that the Cawood and Dolan will be submitted to Seismica in the next few weeks by Dr. Cawood, with you as the suggested handling editor.

It has also been remarked before that viscous roots may control average fault slip rates:

Cowie, P., Scholz, C., Roberts, G. et al. Viscous roots of active seismogenic faults revealed by geologic slip rate variations. Nature Geosci 6, 1036–1040 (2013). https://doi.org/10.1038/ngeo1991

and some of the same authors have also published on structural complexity and slip rate variability: Patience A. Cowie, Gerald P. Roberts, Jonathan M. Bull, Francesco Visini, Relationships between fault geometry, slip rate variability and earthquake recurrence in extensional settings, Geophysical Journal International, Volume 189, Issue 1, April 2012, Pages 143–160, https://doi.org/10.1111/j.1365-246X.2012.05378.x

These are very good suggestions, to bring up the work that has been done on simulated slip rate behaviors. We have added Cowie et al. (2017) (line 442) and Mildon et al. (2022) (line 479) as two examples of earthquake numerical modelling works that highlight changes of fault slip rates in extensional settings.

Stress interaction affecting slip rates at least at some time scale (without necessarily invoking variation in lower crustal ductile shearing rates?) has also been suggested. For examples:

Luo, G., & Liu, M. (2010). Stress evolution and fault interactions before and after the 2008 Great Wenchuan earthquake. Tectonophysics, 491(1-4), 127-140.

This is a good suggestion for another type of kinematics, which reinforces our argument about variability of slip rates (and therefore loading rates, according to our line of reasoning) within complex fault networks such as the Longmen Shan Fault zone, where the Wenchuan earthquake occurred (line 594).

Pollitz, F., Vergnolle, M., & Calais, E. (2003). Fault interaction and stress triggering of twentieth century earthquakes in Mongolia. Journal of Geophysical Research: Solid Earth, 108(B10).

We discussed this in our 2021 Gauriau & Dolan paper (in the 2nd paragraph of its discussion section). We wish we had included Pollitz et al. (2003) results in our 2021 paper. However, given that the aim of this manuscript submitted to *Seismica* is more towards explaining variations of fault loading rates, we do not think we should repeat what we conveyed in the 2021 paper. We are aware, though, of newly released resources that are worth a citation (such as Mildon et al. (2022), published after our 2021 paper).

2) Similarly, it wasn't obvious to me why the observation of variable ratio of geodetic/geologic rates requires a variable geodetic rate (as opposed to a variable geologic rate – as for example stated in Lines 211-212, and also highlighted by Reviewer B). Reviewer B provides ideas for clarifying this assumption, which I also think needs to be done in some way.

Thanks to both you and Reviewer Wimpenny for letting us know that we were not clear enough about explaining our reasoning for this key result. As noted in more detail in our response to Reviewer Wimpenny below, he did indeed understand our key inference that whereas a non-1:1 ratio between geodetic slip deficit rate and geologic slip rate on a fault does not have to reflect changes in "loading rate" (i.e., elastic strain accumulation rate) for fault slip rates that are averaged over small displacement ranges (<50 m and relatively few earthquakes), this is not true for comparisons of geodetic slip-deficit rates and geologic slip rates that are averaged over much larger displacement spanning numerous earthquakes (i.e., our large-displacement slip rates, which are averaged over displacements spanning 50 to >900 m, depending on the fault). These large-displacement rates will average over any shorter-term/smaller-displacement changes in rate and will reflect the long-term average rate of stain release as fault displacement, which over such long/large scales must equal in the energy being stored on the fault, as manifest in geodetic slip-deficit rates. Thus, the mismatches we observe between geodetic and geologic rates averaged over large displacements ("energy in" vs. "energy out", respectively) requires that the rate of elastic strain energy storage manifest as geodetic slip-deficit rates must change with time. Reviewer Wimpenny understood this, despite our apparently less-than-clear discussion. In response, we've used some his suggested text to clarify this. We're really pleased that reviewer Wimpenny understood exactly what we were trying to say.

Answers to reviewers

Reviewer #1 – Zoë Mildon:

This is an excellently written paper that is easy to read and to follow the scientific results and subsequent discussion. There are two broad areas where I feel that the manuscript needs some changes to better justify assumptions and to ensure a broader discussion of prior literature on the topic. I have outlined these two areas below, followed by a few more comments throughout the manuscript.

We thank reviewer Mildon for her great feedback on our manuscript. Please see our responses below to her comments and suggestions.

Section 3

What time interval are geodetic rates calculated over? As you give the time intervals used to calculate the small- and large-displacement rates, I think you should also give the intervals for the geodetic rates for completeness.

As with all primarily GPS-based or InSAR-based geodetic slip-deficit rate estimates, the ones we use in this paper are all averaged over multi-annual to decadal time scales (see lines 123-125).

Is there a minimum length of time that the geodetic community hypothesises is long enough to accurately characterise the strain accumulation? Can the findings of your paper be used to support or challenge any assumptions on this?

We don't think there is any absolute consensus as to what time interval constitutes a period that is "long enough", but it is certainly beyond single-year time scales, and in general our sense is that the community uses the longest possible time series to use as inputs into models of elastic slip-deficit rates, which in the case of GPS extends back to a maximum of about 20 to 30 years in most locations.

There is an implicit assumption that the strain accumulation is uniform throughout the interseismic period (after neglecting post-seismic effects). There is a study (Hussain et al., 2018) from the North Anatolian fault that should be cited to support this assumption – these authors found that the strain accumulation is constant over a range of time intervals since the most recent event.

Yes, this is the assumption we use. We explicitly addressed this point in Section 3 of the original manuscript. Rather than citing individual papers for individual faults such as the NAF (or Phoebe DeVries's work on the North Anatolian fault [DeVries et al., 2016]), we think it is more effective to cite the global compilation-based study by Meade et al. (2013), as we did in the original manuscript.

However, we found this paper interesting to cite in our discussion section, regarding former suggestions to use geodetic data as potential inputs for seismic hazard assessments (see line 548).

Section 6

I broadly agree with the authors that variations in strain accumulation rates are likely to be a result of variable shear rate on ductile shear zones, partly because I've recently published on this (Mildon et al., 2022)! Others have also published on this hypothesis, for example (Oskin et al., 2008), and variable creep rates have also been numerically modelled (e.g. (Ellis & Stöckhert, 2004)), so these contributions to the topic should also be cited in the discussion.

First off, our sincere apologies to reviewer Mildon! We of course should have cited her work in this paper. This oversight has been corrected in the revised manuscript. Specifically, we now cite Mildon et al. (2022) in line 479, and in our Conclusions.

As for Oskin et al. (2008), which refers to the mismatch of geodetic measurements with geologic slip-rate estimates throughout the East California Shear Zone, we now refer to it within a relevant paragraph of Section 6.2 (formerly section 5.2) (see line 419).

Regarding Ellis & Stöckhert (2004): We now cite it in line 478, and refer to it when we tackle acceleration of underlying ductile shear rates through viscous coupling.

However there are some alternatives that could be discussed. For example, temporal clustering of earthquakes is observed in many tectonic settings. Perhaps higher geodetic strain rates are caused by a fault experiencing a cluster of events? This topic is alluded to in the manuscript by considering the time since the Most Recent Event, but this is not related to the measured/hypothesised recurrence intervals.

This is a tricky issue, and it has a certain "chicken-or-egg" aspect to it. We certainly agree that it is likely that there is a correspondence between acceleration of ductile shear zone shearing rate/increased elastic strain accumulation rate and clustering of earthquakes. But we want to be cautious with what we can actually say using our observations, and it is not clear that clusters trigger ductile shear zone accelerations. We say this because our geodetic rate:geologic rate comparisons on high-CoCo faults likely randomly sample any specific fault's "position" within a fast or slow period. In other words, not all of the faults experiencing a period of accelerated elastic strain accumulation rate will have already experienced a cluster of events – we could just as easily be at the beginning of any such sequence of earthquakes, in which case any earthquake, including only the first one in a possible cluster, would result in an accelerated ductile shear zone rate. Of the five examples of "faster geodetic rate than geologic rate" that we discuss in the paper, the Clarence fault has definitely not experienced an earthquake cluster going back at least 5 ky, the nNAF has been suggested to have very regular earthquake recurrence going back at least 1ky (Rockwell et al., 2001), and the Owens Valley fault has experienced a recent earthquake (1872 CE) but no cluster. The Calico fault has not experienced a Holocene cluster, although Ganev et al. (2010) noted that the youngest earthquake there could be part of a regional ECSZ cluster. Finally, the East Anatolian fault, for which we use geodetic data that were acquired before the February 2023 rupture, did not experience a cluster, and the penultimate earthquake that occurred on this section of the fault was in 1795. Thus, the data do not seem to support the necessary occurrence of cluster of earthquakes to explain accelerated elastic strain accumulation rate.

Furthermore, could fault interaction via Coulomb Stress Transfer explain any of the observations in the manuscript?

This is an interesting topic and we now do cite the Mildon et al. (2022) (see line 479) study in this regard, but looking at this issue in detail would need to be part of another study, and a future paper, which is beyond the scope of the submitted manuscript. We also note that both Dolan et al. 2007 and 2023 discussed this possibility in an attempt to explain fault interactions in both southern California and the Marlborough fault system in New Zealand.

Other comments:

Line 97 – could you give further explanation how the CoCo values are standardized by plate motion. Furthermore on line 154, it says that a standardized CoCo value is used, but no explanation is provided to justify this exact value – please give further explanation.

Thank you for asking this question. In our 2021 paper, we used the term “normalization” of CoCo values (to refer to the metric obtained from the computation of the complexity within a given radius, divided by the plate boundary rate that would total within that radius of observation). We agree that this is not a proper normalization, since we do not end up with a unitless number, but it is rather a way to “uniformize” our results from a plate boundary to another, since they all have different plate rate ranges. We therefore prefer using the term “standardization” in this 2023 paper, to avoid confusion. In addition, we have added some explanation to justify and explain this term use in lines 98-100.

Figure 3 is referred to in the text before Figure 2.

We have solved that issue. Thanks for catching this!

Figure 2 – I don’t see the value is separating the data into Lower and Higher CoCo faults (panels b and c). I’d suggest keeping panel a and making it larger.

Figure 2 illustrates one of the fundamental observations that we make in our analysis, and we have retained our original version of this key figure. Specifically, separating the low-CoCo and high-CoCo faults on different figures with same axes drives home the point that these types of faults exhibit fundamentally different geodetic:geologic rate relationships. We suspect that there was a misunderstanding of the figure, and we provide below a clarification.

The value that separates what we refer to as low-CoCo faults from the high-CoCo faults is displayed as $1.6x10^{-2}$ yr^{-1}, and is illustrated in white in the middle of the gradient panel of CoCo values (ranging from low, in blue, to high, in red) at the left top corner of Figure 2a. Similarly, this value is shown on the other top left corners of figures 2b and 2c to serve as a reference.

Lines 161-171 – I found this paragraph difficult to understand. I think what is being said is that the blue dots representing lower CoCo values broadly plot along the 1:1 line, whereas there is more scatter in the orange-red dots representing higher CoCo values. If I have understood this correctly, I suggest rewording this paragraph to make is easier for the reader to follow the reasoning.

The point raised by the reviewer is the second observation made in section 5 (former section 4), which refers to the paragraph (lines 195-206) that comes after the first observation that the reviewer is referring to here. In lines 168-182, we reemphasize what we have shown in our 2021 paper by detailing the comparisons between geodetic rate and both small-displacement geologic slip rate (i.e., a slip rate averaged over a small displacement, <~50 m) and large-displacement geologic slip rate (i.e., a slip rate averaged over a large displacement, >50-900 m). The definition of large-displacement and small-displacement geologic slip rate was detailed in section 2.

We have taken the reviewer's concern into account regarding the wording of that paragraph, and have provided a few more details to make it clearer to readers.

Figure 3 – the caption says "CST refers to "current shortest-term" rates", but I cannot see this on Figure 3.

This was an error, and a term we ended up not using at all in the manuscript. We removed this terminology from the caption, and further explained the display of the dashed arrows of Figure 3.

Line 216 – do the coefficient of determination values come from the data plotted in Figure S1? If this is the case, add the values to Figure S1 and add a figure reference in the text.

Yes, they do, and we have added that information on Figure S1 in the Supplementary Materials. Thank you for suggesting this.

Line 243 – "assumption used in numerous studies" can you give some examples/references of where this assumption is used?

This is the assumption used (both explicitly and implicitly) in an untold number of studies (most geodynamical, fault mechanics, and fault system behavior models and most seismic hazard assessments all assume a steady "loading rate"). But the point is moot, as we decided to remove this sentence during our revisions (line 273).

References

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Mildon, Z. K., Roberts, G. P., Faure Walker, J. P., Beck, J., Papanikolaou, I., Michetti, A. M., et al. (2022). Surface faulting earthquake clustering controlled by fault and shear-zone interactions. *Nature Communications*, *13*(1), 7126. https://doi.org/10.1038/s41467-022-34821-5

Oskin, M., Perg, L., Shelef, E., Strane, M., Gurney, E., Singer, B., & Zhang, X. (2008). Elevated shear zone loading rate during an earthquake cluster in eastern California. *Geology*, *36*(6), 507. https://doi.org/10.1130/G24814A.1

Rousset, B., Jolivet, R., Simons, M., Lasserre, C., Riel, B., Milillo, P., et al. (2016). An aseismic slip transient on the North Anatolian Fault. *Geophysical Research Letters*, *43*(7), 3254–3262. https://doi.org/10.1002/2016GL068250

Reviewer #2 – Sam Wimpenny:

Paper Overview:

Gauriau et al., present a metadata analysis of geodetic strain accumulation rates and geological strain release rates from time-averaged fault slip on major strike-slip faults. Similar analyses have been performed previously, but the novel component of the author's analysis is that they consider these measurements in the context of the structural complexity of the fault systems. Their key finding is that faults that sit within more complex fault networks experience more temporally variable strain accumulation and release rates, whilst isolated faults that accommodate most of the relative plate motion have strain accumulation and release rates that are more stable through time. The authors present a range of explanations that could account for greater variability in the rates of strain accumulation and release occurring preferentially within more complex fault networks, most of which centre around the mechanical interactions between faults.

The manuscript does not present any new data. The figures are clear, and the text is well written and relatively easy to follow. There are places where I believe the text can be shortened to focus more on the key results, as opposed to summarising previous work, which could help with making the author's arguments clearer. Similarly, the authors often combine the presentation of observations with mechanical interpretations, and I think the manuscript would benefit from separating these out to make the case of how their data lead

to their conclusions clearer. I have tried to highlight where I believe these changes could be made in the line-by-line review.
I have no major methodological concerns with the analyses presented – they mainly build upon a previous article [Gauriau and Dolan, 2021]. I do have some questions about the sensitivity of the Coefficient of Complexity summation to the uncertainties in slip rate estimates, which could be easily addressed with a simple calculation.
Overall, the manuscript contributes an interesting explanation for the mismatch between geological estimates of fault slip rates (which reflect time-averaged elastic strain release) and geodetic estimates of elastic strain accumulation rates. There have typically been two mechanisms proposed to explain these discrepancies: (1) time-variation in strain accumulation rates, and (2) time-variation in fault strength. The key implication, at least in my view, is that fault strengthening/weakening may play a secondary role in controlling the timing of earthquakes, because the many of the strengthening/weakening processes should operate on faults irrespective of whether they sit within a complex or simple fault network.

We note here that we disagree that potential time-variable fault strength changes play a secondary role in the behavior of **high**-CoCo faults. Indeed, we think that such changes are likely to be one of, and perhaps the main control on the behavior of such faults. We understand, however, what Reviewer Wimpenny is saying here about the fact that such strength changes will be superseded on low-CoCo fault by the imperative that the plates must keep moving at a steady rate, and these low-CoCo faults are the only faults in such settings that are capable of accommodating most of/almost all plate-boundary motion. As detailed below, we explain these nuances in more detail in response the the reviewer's insightful comments in the revised ms.

I agree with the author's conclusions they draw from the data, but some slight edits to the manuscript are needed to tidy up how this conclusion is reached from the geodetic/geologic data. Considering these comments, **I recommend that this manuscript could be published after addressing these minor corrections.**

We thank Reviewer Wimpenny for his insightful and constructive review.

General Comments:

1. **Implicit assumption that geodetic/geologic rate differences reflect temporal variability in elastic strain accumulation:**

Throughout the paper, the authors state that the difference between the geological/geodetic rate estimates "must" reflect differences in the rate of elastic strain accumulation through time. There are some assumptions in this logic, which I believe are reasonable, but that need clarifying. The geodetic slip deficit rate I agree is a measure of the rate at which strain is accumulating around the fault. The fault slip rate is a measure of the rate of strain release on the fault. Strain release does not have to be the same as strain accumulation, because temporal variability in the amount of shear strain needed to exert a shear stress on the fault to cause it to rupture could create periods of accelerated or decelerated strain release. The authors do touch on this briefly, but I think the assumption needs to be more explicit.
The implicit assumption is that a fault can only store a finite amount of elastic strain – roughly enough to generate a few tens of metres of slip. Therefore, slip rate estimates that derive from offset landforms with small displacements could be affected by periods of accelerated or decelerated strain release, as the "crustal strain capacitor" (in the author's

jargon) is either being discharged or charged. Small-displacement slip rate estimates can therefore be affected by temporal variability in strain accumulation rate, as well as changes in fault strength that cause the stored elastic strain to be released.
Slip rates estimated from offset landforms with greater than a few tens of metres of slip should have averaged out these periods of clustered strain release, as a fault cannot store enough elastic strain to create hundreds of metres of fault slip solely through weakening the fault zone. This seems reasonable given the strain limit of elastic materials like rocks is on the order of ~10^{-3} (i.e. the rock surrounding the fault would just break when the slip deficit to length ratio u/L exceeds ~10^{-3}). Therefore, the only way to explain why the long-term rate of strain release differs from the decadal strain accumulation would be to have temporal variability in the strain accumulation rates.
As I say – I agree with the author's interpretation of the data, I just think that it is worth clarifying the assumptions (if they agree with me) stated above early in the manuscript. This will help the reader establish how strain accumulation rates might relate to slip rates, as I found myself asking "what about changes in fault strength?" multiple times whilst reading manuscript.

We are pleased that the reviewer understands one of our most basic points, and yes, we do agree with everything he wrote here. We would, however, comment that whereas the reviewer refers to this as implicit assumption, we contend that this is in fact an observation. Geodetic slip-deficit rate data are the result of models of elastic strain accumulation in the elastic upper crust due to viscoelastic (in more sophisticated such models) flow within and beneath the brittle-ductile transition. The fact that these estimates of elastic strain accumulation in the elastic crust surrounding the fault we discuss do not match the long-term/**large-displacement** slip rates we cite as a point of comparison requires that there is temporal variability in the elastic strain accumulation rate. This is because, as the reviewer notes, the long-term/large-displacement geologic slip rate (measured over minimum of displacement of 50 m – tens of earthquakes – with some of those rates measured over displacements up to 900 m) will average over any short-term variability such as that observed on some fault system at the scale of 20-25 m of displacement (e.g., Dolan et al., 2023). As detailed below, we have a clearer exposition of these points in the revised manuscript.

2. **Recognising that many of the proposed rheological mechanisms for temporal variability in earthquake timing should operate on faults irrespective of structural complexity of the network within which it is contained:**

This is a point I think the authors could add more explicitly somewhere, which is that if fault weakening mechanisms (e.g. fluid incursion into the fault zone) were to effect all fault zones, then we should see discrepancies between the geodetic and small-displacement geologic rates irrespective of the complexity of the fault network. Their compilation does not show this, and therefore temporal variability in strain accumulation rates seem the most likely explanation for the patterns in their data.

Thanks to the reviewer for pointing out that we weren't clear enough in explaining our reasoning behind this key point, which he understood correctly. In response, we have added a much more detailed discussion of this in the revised manuscript (section 7.2). Specifically noting that, indeed, all the same potential strengthening/weakening mechanisms we discuss (the focus of the upcoming paper by Tarryn Cawood) must be operating on low-CoCo faults, as well, but these are precluded by the imperative of maintaining a steady overall plate-boundary system-level rate in systems that are dominated by a single fast-slipping fault (i.e., low-CoCo faults, such as central SAF, central NAF, southern DSF). This was a very insightful and constructive comment from the reviewer.

3. **The authors variably use slip-deficit rates, slip rates, strain accumulation rates and strain release rates throughout the text.** I would recommend just sticking to using the terms strain accumulation and strain release, providing the caveat that the fault slip rate estimates provide the time- average strain release rate somewhere early in the manuscript. Alternatively, the authors could use “slip rate” and “slip deficit rate” – but maybe try to stick to one or the other.

These terms have specific meanings that we retain in the revised manuscript. For example, a “geodetic slip-deficit rate” is a very specific term that describes the model-derived value for elastic strain accumulation rate on a fault. Many in the community use the nonsensical term “geodetic slip rate”, even though the value they are describing is no such thing. This term needs to be abandoned. Similarly, a geologic fault slip rate is a very specific result of measurement of an offset feature (geomorphic, usually) that has been dated. We did note however a few places in the manuscript where we did not include the adjectives “geodetic” and “geologic” in front of these terms. This has been corrected in the revised manuscript. We also retain our usage of the terms elastic strain accumulation rate (i.e., the value derived from a geodetic slip-deficit rate model) and strain release (the value defined by a geologic slip rate), where appropriate. In the revised manuscript, we define these specific terms explicitly in the second paragraph of our introduction and in the last paragraph of section 2.

1. **Maybe this is pedantic, but I do not think the Coefficient of Complexity is a coefficient, as far as I understand that word. A coefficient is a multiplicative factor that comes before a variable (e.g., 4*pi*r, where 4*pi is the coefficient and r is the radius for example).**

In this case, there is no variable that comes after the coefficient, so it is a metric. I know this does not change anything and is just semantic, but worth considering.

Yes, the reviewer is correct, CoCo is not a coefficient, if we consider theoretical physics. It also does have a pretty arcane unit, whereas a coefficient supposedly is unitless. We took, however, the liberty to call this metric as such, as do several engineers who introduce new “coefficients” in the field of geotechnics or structural engineering, and which do have arcane units as well. We recognize that the use of this term may bewilder a few, but we hope its originality rather drives readers to remember the importance of the original Gauriau & Dolan study and its companion study, here. We retain its usage here.

1. Uncertainty on the Coefficient of Complexity:

The one methodological query I have regarding the manuscript is whether the algorithm the authors use for computing the coefficient of complexity is sensitive to the uncertainties in fault slip rates within the radius of interest. I would recommend the following test: (1) establish the range of possible slip rates for each fault, (2) define some form of probability distribution for each fault slip range [i.e. Gaussian if the slip rate is a±b where b is the standard deviation, or just uniform distribution if you have a range a–b], (3) perform multiple iterations of the CoCo calculation that uses the slip rates sampled from these distributions, (4) construct the resulting distribution of CoCo values. From this, the authors will get an estimate of either the range (if using uniform distributions) or standard deviation (if using Gaussian distributions) of the CoCo, and could put the error bars onto Figure 3a. It may well be that it has very little effect, but this seems important for determining whether the trends in CoCo are robust or not.

The reviewer is correct that the slip rates used in determining the CoCo value for any fault study site in the surrounding fault network are extremely important. We explicitly addressed this in the original Gauriau and Dolan (2021) paper, where we carefully documented how we derive the CoCo metric. While we think it is inappropriate to repeat everything we said in the 2021 paper in this analysis (which builds on that paper), let us just say that in calculating the CoCo metric for any site, we binned the slip rates for all faults within the area of observation within nine slip rate categories. The ranges of these slip rate bins almost certainly cover the variability that the reviewer is concerned about. The value used in our computation is a median value of each of these slip-rate bins. Doing this in more detail would be a truly enormous undertaking, even if it were possible, which it isn't, given that detailed slip-rate values with formal 2σ error limits are lacking for the vast majority of faults in most plate-boundaries. Binning such faults into the categories as we did in our 2021 paper will be as close as we can get with currently available data to assigning slip-rate ranges. Beyond that, even if such data were available for all faults within surrounding plate-boundary fault network, the type of analysis the reviewer is suggesting would entail years of work (based on the experience of a former PhD student in our group, who was charged with doing something similar for only the faults in the intermountain seismic belt of the western US).

Line-by-Line Review:

Line 27-29: What the authors mean by "mechanically complementary" is a little unclear here – does it mean they slip in response to the same stress field, or do they mean "kinematically complementary" in that they accommodate different components of the displacement field? I would recommend sticking with just "*complex fault systems characterised by multiple faults accommodating the deformation field, which we refer to as ….*" – this more accurately reflects the data analysis.

We have rephrased the revised text to make this sentence clearer (see abstract, line 29).

Line 31-33: "rates of ductile shear-zone roots also vary through time" – re-phrase this to "… that the rate of ductile shear beneath the seismogenic portion of faults also varies through time." ✓

Line 37: Change "… relatively constant fault slip rates …" to " … relatively stable fault slip rates …"

We disagree with this suggestion and retain the original phrasing. "Constant" is a very specific and correct term for what we mean, whereas "stable" has a variety of meanings and introduces potential confusion.

Line 56: I am not sure what Seismica's policy is over citing unpublished or in review articles, but I would recommend avoiding this practice. Just because you have plenty of examples that are already published to make the point here. This applies throughout the manuscript too – there are lots of places where the text relies on in review or in preparation work. In these places, just give a brief summary of the arguments they present, such that the reviewer (and potentially reader) can follow the logic without needing these papers.

We agree with the reviewer with respect to citing unpublished work. We think it is bad policy and prefer to avoid it if at all possible. It just so happens that three papers related to this one are all either near completion or have recently been submitted. In the time since our manuscript was submitted, Dolan et al. (2023) has now been published in EPSL. In anticipation of Fougere et al.'s paper on the incremental slip rate for the Garlock fault (now submitted), we now cite her 2023 AGU abstract on this topic. As alluded to earlier in our response, with respect to Tarryn Cawood's paper exploring ductile shear zone strengthening and weakening mechanisms with respect to their possible role in acceleration and deceleration of ductile shear zones, in addition to citing Tarryn's "in prep" paper, we have added several references that are also cited in the Cawood and Dolan paper, as suggested by reviewer B and editor Fagereng. Tarryn's paper will be submitted to *Seismica* (with editor Fagereng suggested as handling editor) within the next few weeks.

Line 73: There is a colon where I think there should be a period. "… different geological timescales. Faults that lie…"

The American punctuation (which we have adopted, along with the spelling) uses capitalized letters after a colon. In any event, this sentence has been reworded according the reviewer's suggestions for reformulating this paragraph.

Line 75-76: I would recommend some slight re-wording here to highlight that this study is motivated by the results of Gauriau and Dolan, [2021]. I was not familiar with this work, but on reading it then it becomes clear where this manuscript has emerged from, and I think other readers would benefit from this context too. Consider re-wording to something like:
"Unlike estimates of geological slip rates, which reflect the rate at which elastic strain is released on a fault averaged over time scales of thousands of years, geodetic measurements of deformation around fault zones can be used to infer the rates of elastic strain accumulation over a time-period of years. The rate of elastic strain accumulation is often expressed as a 'slip deficit rate' on the fault where it is locked in the shallow crust. Meade et al., (2013) compared geological fault slip rates with geodetic slip deficit rates from 15 major continental strike-slip faults and fault that, as an ensemble, these faults exhibit a near 1:1 relationship (with a …) between the geological and geodetic rates. Slight differences between the datasets could be attributable to short-lived periods of higher-than-average strain accumulation during the post-seismic period.
The geological rates used as inputs into the analysis of Meade et al., (2013) are derived from landform offsets that span a large range of displacements (13 m to 600 m) and ages (2 kyr to 160 kyr). We recently presented results that demonstrate that geological estimates of slip rates vary depending on the displacement scale over which the slip rate is estimated, particularly on faults that form part of a network of closely-spaced faults accommodating a given deformation field [Gauriau and Dolan, 2021]. Isolated faults that accommodate the majority of the deformation field exhibit more steady slip rates [Gauriau and Dolan, 2021].

Therefore, it is possible that differences between geodetic and geological estimates of fault slip rates are also sensitive to the complexity of the fault network.
In this paper, …"
This is just a suggestion, but it is worth highlighting the importance of the author's earlier work here, even with some other edits.

We thank the reviewer for this suggestion, which we have adopted to clarify the results of our prior study on which this paper is based. A very helpful and constructive suggestion!

Table 1: The slip-rate and age estimates are presented in a range of different formats, including: a±b, a-b, and with a range of different decimal places. All of this may well be deliberate to reflect varying amounts of precision and various bounds on the ages of landforms that are offset, but this is not clear from the table description. I'd recommend either explaining why the formats are different or present them in a consistent format.

We use these different styles because we wanted to retain the original authors' style of presenting their data. We have clarified in the table's caption that the presented values are in the same format in which they were presented in their respective publications, unless specified otherwise in the table. In other words, we don't want to put words in the original researchers' mouths. For our own perspective in reporting such rates in our studies, we note that saying a slip rate is 12±4 mm/yr is subtly different from saying the slip rate has a range of 8-16 mm/yr. The former suggests a preferred value for the rate and the latter suggests less confidence in being able to suggest a preferred rate within that range.

Line 122: The title of this subsection does not seem grammatically quote correct, and it does not give much information about what's contained within the following paragraphs. Maybe consider a new title for this subsection? Possibly "Selection of Geodetic Estimates of Slip-Deficit Rates"?

We agree. In response, we have split this section into two (sections 3 and 4), and changed the title.

Line 184-185: This is an example of where the interpretation (first sentence) comes before the description of the observations. The issue with doing it this way around is that it was not immediately clear why the loading rate (and not, say, the fault strength) must vary through time to explain why there is a different between the geologic-geodetic rate scaling. I'd recommend removing this sentence, or move it to somewhere in Section 5, to more clearly separate observations from interpretations.

Good catch by the reviewer. He is absolutely correct, and we have deleted our interpretation from this section of this paper, and moved it to the following section, where it is appropriate. Good suggestion.

Line 212-214: Here is another place where the mechanical interpretation is mixed in with the description of the observations. I'd recommend removing this sentence or move it to somewhere in Section 5.

As noted above, we have removed the interpretation from this section and moved it to later in the manuscript.

Line 221: spelling errors, should read: "… than …"

Typo. Corrected.

Figure 3: I like this figure – it really captures the key point the authors are trying to make regarding the dependence of geodetic/geologic slip rates on the CoCo values. However, one thing I don't necessarily understand, is why there is such a sharp change in the geodetic/geologic rate from ~1:1 at CoCo > 0.002 – if mechanical interaction were important then wouldn't we expect to see more of a continuous variation with increasing CoCo? It is possible that there are just too few examples to properly delineate the trends, and that the result is somewhat sensitive to the radius used to compute the CoCo, but this is just a thought for the authors to consider.

This is a very interesting point, we thank the reviewer for bringing this up. We actually disagree that this is not a continuum. We think the impression that this is an abrupt change at CoCo~0.002 is largely controlled by the Garlock fault, however as noted with the dashed arrow on Figure 3, the Garlock fault could lie anywhere within the bottom half of the plot. Ignoring the Garlock fault, there is a gradual increase in the dispersion starting at values >0.0015. However, although we do view this as a continuum, the increase in dispersion at ~0.0015-0.002 is rather sharp, and we think this reflects plate-boundary fault systems in which at higher CoCo values, they start to appear significant secondary faults that allow fault activity to switch back and forth. We have added this idea to the manuscript in lines 244-246.

Section 5: I would recommend just adding a little more in the way of explanation as to why the relationship between CoCo and geodetic/geologic slip rate indicates time-dependent variability in the rate of elastic strain accumulation in this section. I'd recommend this to be the place to address my "General Comment" about clarifying the assumptions in the study.

Thanks to the reviewer to pointing out that our reasoning was not as clear as we had intended it to be. Inasmuch as this is one of our key points, we have very carefully rewritten this entire paragraph to explain our reasoning (revised section 6 – former section 5). We think this revised paragraph does a much better job of explaining what we mean and we thank the reviewer for flagging this key issue. A very good suggestion.

Line 262: Replace colon with a period.

We adopt the American punctuation format.

Line 299: I would recommend not referring to the 'in review' paper, and rather just explain explicitly what is meant by the "crustal strain capacitor". If it is the idea that faults only release part of the elastic strain stored in their surrounding wall rocks, and therefore multiple episodes of strain release can occur that do not necessarily balance the strain accumulation, this is not necessarily a new idea and other citations could be found [e.g. Mencin et al., 2016].

Dolan et al. is now published, and we now cite it as Dolan et al. (2023). We thank reviewer Wimpenny for noting Mencin et al. (2016) as another study that used a similar term. We have added a citation of this work (line 330).

Line 300: Is it not the shear strain that is stored within the crust, which exerts a shear stress onto the fault plane?
Good catch! This was a typo, since the term we've long been using is "crustal *strain* capacitor"!

Lines 280-303: Here time-dependent changes in fault strength are suggested to play an important role in modulating the slip rate when viewed over a small number of earthquake cycles. What has not been addressed at this point is why the authors think that such time-dependent changes in fault strength, which I agree have the capacity to lead to pulses of a few earthquakes and therefore strain release, are not to blame for the differences between the geodetic and long-term geological rate discrepancies. Elsewhere the authors state that the differences "must" be caused by time variability in the rate of elastic strain accumulation – I have not yet seen the argument (in this manuscript at least) for why this is the case. See my point in the "General Comments" section.
Thanks again to the reviewer for pointing out that we were not clear enough in explaining our reasoning behind this key point of our study. In response, we have revised and expanded paragraphs 6.1 and 6.2 (formerly 5.1 and 5.2). Specifically, we note:

"Moreover, although the mismatch between geodetic slip-deficit rates and small-displacement geologic slip rates could conceivably be due to short-term variations in fault slip rate, the mismatch between geodetic slip-deficit rates and large-displacement geologic slip rates, which are averaged over >50 to hundreds of meters of slip (see Table 1) and numerous individual earthquakes and will thus average over any shorter-term/smaller-displacement accelerations or decelerations of fault slip, indicates that elastic strain accumulation rates on the high-CoCo faults must vary through time. Specifically, at these large-displacement scales, the fault slip rate spanning numerous earthquakes will provide a robust estimate of the average rate of strain release on that fault through time. Insofar as the elastic strain accumulation rate must equal the elastic strain release rate (i.e., fault slip) over long time intervals, the mismatch that we document between geodetic slip-deficit rates and geologic slip rates averaged over large displacements requires that elastic strain accumulation rates as measured by geodetic slip-deficit rates must vary through time."

Line 315: Change to "… as the dashed arrows in Figure 3a illustrate" ✓
Line 330: Change to "… the lack of significant coseismic slip in 2016 on the Hope Fault…" ✓
Line 407: Replace "SM" with "Supplementary Material" ✓

Line 415-419: This sentence provides the key thesis of the paper – though I am still not entirely sure that it follows from the preceding paragraphs, unless we make the assumption that the large-displacement slip rate estimates represent the time-average strain accumulation rate and not the time-average strain release rate that is affected by changes in fault strength.

See our comment above. This is indeed our contention.

Section 6.1: Much of the discussion here relies on references to, as yet, unpublished works. I'd recommend either pre-printing these papers so they can be properly cited (as there's no way as a reviewer for me to check what these papers say in them), or just explain what is said in those pre-published papers.

See our responses to previous similar comment above. We no longer cite any "unpublished studies.

Line 466: Please could the authors explain what is meant by "the primary fault is forced to slip at small-displacement scales"? My reading of this would be that tectonically isolated faults slip with small amounts, but I think the authors mean something else.

After re-reading this section, we can see why the reviewer found this confusing! We have reworded this to note that: "the primary fault is forced to respond to the imperative of maintaining a constant overall system-level rate by releasing any accumulated elastic strain at relatively short time and small displacement scales".

Line 493: Not sure the "(and currently missing?)" is needed here – I would suggest just removing it.

We have rewritten this sentence to say: "Such data may prove to be useful for more accurate future PSHA."

Section 7: The argument here is qualitative, referring to "higher" and "lower" future seismic hazard. I think the authors make a clear conceptual point about how geologic slip rate estimates might under/over predict hazard, but it is not clear how the methodology presented could be implemented in a quantitative way into the PSHA framework.

We already suggest an approach that could be applied in PSHA (section 8 – former section 7). Suggesting specific numerical/statistical/modelling methods to implement our approach is far beyond the scope of this paper.

Conclusions: The conclusions are mostly a repeat of the findings from the main manuscript and could be shortened somewhat to extract the key points. As a reader, I always think short and sharp conclusions are the most effective, but I will leave it up to the authors to decide on the format.

We have slightly reduced the length of our conclusions.

Review of Supplementary Materials:
Section 1: Typo where colons are followed by a capital letter – should read "… brought. Instead of using … "

A colon is not normally followed by a capital letter in British usage, though American usage often prefers to use a capital. Although the first author of the manuscript is not interested in entering this battle, both authors have agreed on using the American spelling and capitalization of the word preceding a colon.

Section 1: I am not entirely sure why the slip rates are assigned a median within the centre of a given range in the CoCo calculation, as opposed to just using the range in fault slip rate (or uncertainty) and propagating these through the summation to get a CoCo estimate plus its range/uncertainty. Maybe I am missing something here. Having read Gauriau and Dolan 2021 it seems to me that this approach could neglect the broad uncertainties associated with slip rate estimates, which can be large, and might therefore lead to CoCo estimates that could have large uncertainties too. See my comment in the "General Comments" section.

See our earlier response to your comment in the General Comments section.

Section 3: Change section header to "Comparison of Geodetic Rates with Geologic Rates"
✓

Section 6: The text explanation for how the authors calculated the dispersion of the data was difficult to follow. I think it would benefit from explaining the calculation using formulae, or the authors could also simply rely on the visually clear change in the dispersion of the data from the Figures to make their point, particularly given that the dispersion metric has no obvious physical meaning or statistical relevance.

We have added a figure (S2) that provides a better explanation of the dispersion calculation.

Section 7: I cannot find Figure 1c, presumably this is a typo and refers to Figure 3c given that the section explains why the authors think their analysis is not biased by absolute slip rate? This piece of text is also so short it could well be put in the main text.

Thanks for catching this typo. This section was referring to Figure 2c, which shows the dispersion of high-CoCo faults' data points. We decided to keep this in the Supplementary Materials, to keep the main text more straightforward in that regard.

References:

Mencin, D., Bendick, R., Upreti, B. *et al.* Himalayan strain reservoir inferred from limited afterslip following the Gorkha earthquake. *Nature Geosci* **9**, 533–537 (2016). https://doi.org/10.1038/ngeo2734