Dear Editor,

We are pleased to submit the manuscript 'Assessing creep event rheologies using their temporal evolution' to Seismica. We can confirm that this manuscript has not been previously published elsewhere and is not currently under consideration by another journal. Our research study aligns well with the aims and scope of Seismica, as our work focuses on near-surface aseismic slip, a crucial component of the overall earthquake cycle.

Since the 1960s, creep events have been identified in USGS creepmeter records from the San Andreas Fault. Despite this, there is no consensus on the fault properties that create these events. Previous work has focused on modelling whether certain rheological models can produce creep event-like bursts of slip (e.g., Scholz, 1998; Wei et al., 2013; Wesson, 1988), yet the resulting shapes of these events have not been compared to real events. Here, we use creepmeter data from Harris Ranch, CA, at the northern end of the creeping section of the San Andreas Fault, to test whether five plausible but simple rheological models for near-surface slip can reproduce the observed shape of creep events by minimising the misfit between the observed data and model predictions. We find, somewhat surprisingly, that most events are best described by either a power-law flow or by rate and state friction below steady state. "Standard" velocity-strengthening friction models do not match the data well. Our results could have one of several implications: (1) power-law creep somehow dominates even at near-surface conditions, (2) the near-surface is weak and mostly tracks the rheology of evolving slip at depth, or (3) a more complex rheology is required, perhaps including dilatancy or pore pressure.

We thank the reviewers and the editor for their comments and feedback about our work. We have carefully considered their suggested revisions and acted upon them accordingly to improve and clarify our manuscript. Both reviewers recognize the value of our quantitative approach but raise important questions about our simplifying assumptions regarding state variable evolution and request additional clarity on our methods and interpretation of results. We have addressed these concerns through adding extra context in Section 3 when we define the rate-and-state models which we have used. We have also added in a new Figure 4 which shows how the different rate-and-state models evolve in both log(slip rate) and stress. In Section 4 we addressed concerns about how the covariance matrix used in the model fitting was calculated by clarifying the steps taken to produce them as well as adding more detail about the model fitting parameters. We have also expanded on the discussion in Section 7 to explain further what the results suggest about the dynamics of creep events and how further investigation is required on with more complex fitting (multiple phases for acceleration and deceleration) or rheological models (e.g., shear induced dilatancy). In adding this extra discussion to Section 7.1 we also fixed the contradiction between it and section 3.4 and moved some of the details from 3.4 into this section of the discussion.

This work is funded by NERC, and as part of our funding requirements, we also need to deposit a copy of the paper in Open Access Oxford.

We would appreciate your support in publishing this exciting research in the format submitted to advance the research on creep events and shallow aseismic slip.

Yours faithfully,
Daniel Gittins & Jessica Hawthorne

Editor comments

The "non-technical summary" is still quite technical. I know it is difficult to rephrase things like "frictional model above steady state" but this is still jargon.

We have attempted to remove the jargon from the non-technical summary as best as possible while still conveying the same meaning.

Related to (1), I think a more "reader-friendly" description of the three rate-and-state models would be helpful. The concepts of above and below steady state aren't clear to readers who don't have a lot of experience with rate-and-state friction. I think it would also be helpful to add a figure showing the general forms of the curves from the 5 models, something analogous to the "viscosity plot" illustrating Newtonian and non-Newtonian fliuids.

We have added some more descriptions of the rate-and-state regimes in Section 3 and tried to make it more reader friendly. We have also added in a new Figure (now figure 4). Which shows the log(slip rate) against stress for the different rate-and-state regimes.

For power law fits, there is a test to check whether the full dataset really follows the power law, by finding a value "xmin" and doing some statistical analysis. Did the authors conduct such analyses? I am not completely familiar with this myself, but it can be found for example in Clauset et al. 2009 SIAM review.

This sort of statistical analysis isn't actually suitable for our application. The analyses of Clauset et al. 2009 are designed to test whether a *distribution* follows a power law. But we don't have an unordered distribution of creep values to model. We have a set of creep values d that are a function of an independent variable (time).

So we're essentially fitting y as a function of x. It just so happens that the proposed relationship between x and y is a power law, but from a fitting perspective, there's minimal difference between testing a power-law relationship and some other proposed relationship (a sinusoid or exponential relationship, for instance).

The way to test a relationship between x and y is to assess whether the predicted values match the observed data using the appropriate uncertainty on the data. That's what we do.

We haven't added anything to the text on this front, as introducing distribution modelling seems more likely to mislead readers than to resolve clarity.

I found it interesting in Figure 7m that velocity-strengthening friction is permissible in these models. This is very briefly mentioned but I think it deserves further discussion and some other studies have mentioned this.

We agree that it is strange to get a velocity strengthening fraction result for the below steady state model for 3 out of 50 events (as shown in Figure 8 (formally 7). This would be possible if stress were rapidly reduced. That can happen if the surrounding region slipped a lot and carried the surface along with it. We have added this to the discussion on the best-fitting model parameters in Section 6.

Minor: (L184) The description of tau_l(dot) should come after Equation 9, where it is first seen.
We have changed where the definition of tau_l(dot) is described following Equation 9.

The text in L237-246 should come in the discussion.
We have moved this text to the discussion (L491–518). We also reworded this section so that this text is more obviously consistent with that in the discussion. (See the related comment from reviewer C.).

Reviewer B

Section 2 Data:

It would be helpful to explain how/what time windows before and after the event onset are chosen for the 50 events, because the number/duration of the time series data are relevant to later inversions. Furthermore, when does this subset of events happen? Throughout 1991–2019, or more clustered in time?

We have added that the 50 events are not clustered in time and occur throughout the period 1991–2019 in Section 2. In section 4.4 we added the explanation for the time windows around the event start and end times in the fitting.

Section 3 Rheological laws:

Line 154: "If the step in stress is relatively abrupt, but the slip rate evolves slowly compared to the timescale for state to evolve, the fault patch will remain close to a 'steady state' condition, where θ is small … Alternatively, if the step in stress is abrupt …" => This sentence is unclear to me. The term "abrupt stress" is mentioned twice, which may be a typo. In any case, a step in stress should lead to an instantaneous step in slip rate, while state evolution is slow and may not have a characteristic timescale. Hence, I don't understand here how "slip rate evolves" relative to the "timescale of state." Also, you should clarify that the fault is at steady state before the stress step.

We have changed the description of the rate-and-state models to better describe how state evolves and have clarified what is meant by a step in stress by changing the wording from "step in stress" to better explain how stress, slip rate and state evolve. This more complete description of the model has been added to Section 3.3. We hope the addition of a new Figure 4 will also help the reader understand what is happening in each of the rate-and-state models.

Line 164: "Suppose the change in slip rate following an increase in stress occurs slowly enough relative to the timescale for the state evolution. In that case, the fault will remain close to but below steady state. Here, the state rate remains constant but not zero (Helmstetter and Shaw, 2009)." => How does this paragraph relate/transition to the next paragraph, where the steady-steady assumption is adopted? Some rephrasing could be helpful.

We have also rephrased this part of the text to address more fully how each of the rate-and-state models works, improving the flow of the text. We have stated more clearly that the evolution of state is very close to steady state but not at steady state.

Section 4.2 Noise Estimates:

Line 272: "To estimate the temporal covariance of the noise, we select 50,000 segments of creepmeter data that are four days long. … Cnm is the average value of (δ(t n) – δ(t 0)) × (δ(t m) – δ(t 0)) … t0 is the interval start time. We calculate one data covariance matrix per creepmeter iteration." => I do not follow this paragraph. Can you clarify that (I presume) these time series data segments do not exhibit any trends or transients and appear random? What does the "average value" mean? I guess the delta functions refer to displacements, but I would use some symbol to distinguish data from theoretical predictions in Section 3. The definition in Equation 15 should appear earlier here and use consistent notation. Could you write out the exact definition for the covariance matrix C_mn?

We have rephrased this section to make the calculations more explicit.

The data segments do sometimes have trends, but those trends are just noise—that's what we seek to quantify. We've added that.

The average is the average over the 50,000 segments. We changed the notation and now write out an equation for C_mn, as suggested.

Figure 4: I don't understand why the noise variance-covariance matrix looks like this. If you are only estimating random zero-mean observational noise, I would expect higher covariance between closer data points. Your covariance matrix means that covariance increases with time. Have you performed detrending of noisy time series data, or are you using data that contains creep events? More explanations are needed here.

With random walk noise, the covariance should definitely be smaller for more closely spaced data points. That's what we observe.

Perhaps there's a confusion here between covariance and correlation? More closely spaced data points are certainly more correlated—that's another way of saying that the covariance between closely spaced data points is smaller. Correlation is another way to quantify noise, but we prefer the covariance, as it is standard and lends itself to data misfit calculation as in equation 15 (now 16).

We've added a bit more physical motivation explanation in the text, but the nature of covariances with random walk noise is quite well established and seems like a distraction for most readers, so we tried not to expand too much. We added a few references for those interested.

Line 280: "To regularise the covariance matrices and make them invertible, we add a component of white noise to the covariance matrices, with an amplitude of 0.1 × C_22." => Does number 22 here indicate the 2nd day? That seems inconsistent with the use of tn/tm (Lin 274).

C_22 is the 2nd value along the diagonal, i.e., the value in row 2 column 2. We have edited the text to show this accordingly at the end of section 4.2.

Equation 15: observed slip is also a function of t (Line 274).

We have made it clear that observed slip is also a function of time.

Section 4.4 Parameter Search:

Line 298: Please provide references for the Nelder-Mead algorithm. A very brief explanation of SLSQP and Nelder-Mead algorithms would be helpful. SLSQP cites Virtanen for scipy, but a primary source would be better.

For the Nelder-Mead algorithm we have cited (Nelder & Mead, 1965). For SLSQP we now cite (Kraft, 1988; Lawson & Hanson, 1995; Nocedal & Wright, 2006). We have not added extra explanation of the algorithms as we believe these would be bulky and distracting for the reader

What is the initial range of parameter values for the SLSQP inversions? The effective parameters in these rheological laws can vary widely across cases and span orders of magnitude. Please elaborate on the rationales and selected values to help readers understand the process.

We have added extra details on the initial ranges of the important fitting parameters in section 4.4 and have also explained why we chose them and why the ranges were left quite large.

Section 5 Model fits:

Line 306: “However not all models are created equally and during the fitting have extra free parameters (m).” Here, “m” could be confused with the unit m when not italicized. I think it is better to specify “m is the number of model parameters” following Equation 16.

We have now defined *m* following Equation 16 and made sure all mentions of it are italicised.

Line 326: “This equation is mathematically equivalent to Equation 17 under Gaussian noise assumptions.” => If the covariance matrix (precise definition unclear to me) has units of length squared, then Equation 17 is not equivalent to 18 because the units don’t match.

We have corrected the statement within the manuscript to reflect that Equation 19 (formally 18) is a scaled version of Equation 18 (formally 17).

Add the references for Akaike Information Criterion (AIC) and Akaike weights.

We have added the relevant references: Akaike, 1973, 1998; Wagenmakers & Farrell, 2004

Minor comments/typos:

Line 90: “… though to resist accelerations in slip.” => “thought”

Fixed, thanks.

Line 334: “bets fitting” => “best fitting”

Fixed, thanks.

Line 371: “70 % confidence” => “70% confidence”

Fixed, thanks.

Line 513: This paragraph reads informal, with “somewhat”, “some”, “perhaps”, “But”, etc.

Fixed these typos, thanks.

Reviewer C

First of all, I understand the reasons for making limiting assumptions regarding the state variable in order to derive analytic predictions for the creep, but it creates a weak point in the argument since it is not clear to me that the state variable would not be evolving significantly throughout the event. This is mentioned in the discussion (section 7.3.1), but quite briefly - to me this seems like a potentially very plausible interpretation of the results; in particular I wonder if it could explain the ‘acceleration’ phase in some events (especially e.g. Figure 5d) which has been completely ignored by simply fitting a line to the early part of the curve. I understand that fitting a complete numerical solution to the RSF equations is beyond the scope of this paper, but I suggest expanding the discussion on this point, and wonder if it would be possible to evaluate (qualitatively or quantitatively) which models could fit various portions of the timeseries - e.g. could an above-steady-state model fit the first half of the creep events while a below-steady-state model fits well to the second half?

To be clear, state does evolve over the course of the creep event; it just evolves according to very particular models. Hopefully this is more intuitive with the simplified rate and state explanations along with figure 4.

We did try an experiment with fitting the creep event in two halves (acceleration and deceleration). In practice however, doing enough to make sure the results would have been robust would have taken a lot of time and isn't practical for this paper. One major issue is that the acceleration phase of the creep event may only be 2–3 data points long meaning all models can fit it well.

Note that the straight line that we fit in Equation 14 is not to account for the acceleration either but rather to accommodate the preamble that occurs before many observed creep events related to the strain of the approaching dislocation.

We have added a paragraph in Section 7.3.1 explaining why modeling the acceleration and deceleration phases of the creep event is challenging and explaining that there is likely a lot more complexity still to be unravelled.

Regarding the viscous flow models - in Figure 6 it seems intuitive to me that by excluding linear flow most of that support goes to the generalized power law; I'm not convinced this is really adding much new information since the best fit of n is typically close to 1 already (Lines 433-440; Figure 7e). A more interesting question to me relates to the cases where n is not close to 1 (e.g., where the best fit is n>2): is there a different secondary model preferred in these cases? If so, might this indicate there are different classes or types of creep event? More generally, how do the best-fitting parameters for the other models trade off with n? I would love to see some multi-dimensional scatter plots that could help elucidate these correlations between the models.

We agree with the fact that is seems intuitive that by excluding the linear flow models that most support goes to the generalised power law. However, we feel this is still worth pointing out in the paper.

Mutli-phase creep events do occur, but we try to remove these as they require forward projection of the previous phase into the following ones which is beyond the scope of this study and needs a more complex treatment of the creepmeter phases that performed here.

We have not identified any groups of parameters. However we did investigate the trade off between b/a and n and have included this as a supplementary figure S1. We have added a short paragraph in Section 6 discussing this trade off.

Minor comments:

Some informal phrasing in the abstract/summary could be improved:

- ambiguous use of "we" to refer to the whole community (line 10, 24) but also to the authors

We have removed the ambiguous use of "we"

- "surprisingly" or "surprising" (line 18, 29) - better to avoid subjective statements (I think)

We have removed the colloquial language from the non-technical summary.

- phrases like “try and” (line 26) which are too colloquial.

We have removed the colloquial phrses.

Line 65: “events each rheology” - accidentally a word?

Fixed, thanks.

Figure 1 - the inset map just looks like a vague gray box to me. Could the contrast be enhanced or some more features added? Also, on the main map, the lat/lon text labels are unnecessarily large, and the XHR label is not very near the triangle.

We have modified Figure 1 to make it clearer what the inset map shows as well as fixing other issues with the map regarding font sizes, and labelling. We also changed the colour scheme to make it easier to identify important features.

L143, 148: unnecessary uses of “apparently” or “appears” here

Removed the unnecessary words, thanks.

L269: Not sure why “as a result” makes sense here

We have reworded to make this sentence more clear.

L275: what does “iteration” mean here? Is it the version of the creepmeter design?

We have removed this confusing phrase.

Equation 19 is identical to Equation 16, except for the subscript _w. Is this equation needed?

We have removed Equation 19 as it is not required.

L334: “bets” -> “best”

Fixed, thanks.

L352: “the the” -> “be the”

Fixed, thanks.

Line 469 seems to disagree with Line 245 (regarding whether pressure solution creep is able to explain creep events at slip rates of 1e-7 to 1e-6 m/s). Perhaps one or the other could be rephrased to be more in agreement?

We have rephrased this section following moving it (Line 245) to the discussion to make sure it is in agreement.

L484: “fault be” -> fault may be

Fixed, thanks.

L509: extra “slip”

Fixed, thanks.

L511: decelerate -> decelerates

Fixed, thanks.