

Peer Review File

Manuscript Title: Fault Network Geometry Influences Earthquake Frictional Behavior

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1:

In this paper, the authors relate the geometry of fault networks to the slip behavior. While most faults are locked most of the time, leading to the accumulation of elastic energy in the crust, some other faults slip aseismically, quietly accommodating all or part of the motion imposed by tectonic plates. Current models explaining the slip behavior of faults over the so-called earthquake cycle consider a fault as a frictional interface mostly governed by the constitutive properties of the friction law. In such framework, the rheology of the rocks forming the fault material hence control the slip behavior. Although quite successful at explaining first order features, lots of issues remain and seem to point that the direct link between rheology and slip behavior is not that obvious.

Here, Lee et al propose that the geometrical complexity of the fault network is a major controlling factor and that, in particular, regions with low geometrical complexity are prone to host aseismic slip while more complex regions will host earthquakes. The authors first describe the correlation they find in the data before discarding other possible correlations. They then discuss the possible implications for the fault slip behavior. The formal analysis in this paper only includes a few statistical methods, very classic, and nothing fancy so I haven't identified significant flaws in the methodology. The figures convey adequately the message of the paper which, in itself, is very well written which makes it a very pleasant read. I think that exploring new ways of interpreting occurrences of aseismic slip globally is exactly what should be done to move forward and this is exactly what this paper is trying to do.

However, I have some significant concerns that require some work and in my opinion some, even partial, re-writing of the paper here and there, in order to make it more convincing and potentially enlarge the scope of the discussion.

- One big issue is that there is a significant focus on the San Andreas Fault, once again, and the correlation found is halfway convincing. Although this might be difficult, such correlation should be found along other creeping faults in the world and this would strengthen the argument a lot. The amount of work to extract creep rate is not that big considering a lot of creeping faults have now been mapped by InSAR (see Chaman fault, Leyte fault, NAF, El Pilar fault, ...). The more challenging task could be to get accurate enough fault mapping to extract fault misalignment along these sections but it should be at least tried in order to avoid a too much California-centered interpretation of fault behavior.

- The paper is not always clear in the interpretation of the correlation. Sometimes, it reads as if there was an established causal link from geometry to fault slip behavior while the paper "only" demonstrates a correlation. For instance, the title seems to indicate a direct, one-to-one relationship

between geometry and slip behavior while it is written later on that some low misalignment faults are locked. If misalignment was the “predominant controlling factor” as written in the abstract, then we should not see locked segments with low misalignment. There is also mention of a “strong explanatory power for the occurrence of fault creep” on Line 53, which sounds like a direct relationship not really verified by the data. Figure 4 writes that “geometrical locking controls slip behavior” again, which is only an interpretation for now.

This is potentially some unnecessary bragging (sorry, probably not the right term but english is not my first language) which could lead to some confusion from the correlation shown by the data to a potential, but not demonstrated, causal link which should be investigated in further studies. I don't think the paper should cross the line of such confusion to justify its impact as the sole correlation, if strengthened, would be enough to make it an impactful paper, in my opinion.

- On line 188, the authors mention the possibility of a feedback loop between the slip behavior of faults and the geometrical configuration of the mapped faults. This point led me to consider two things. First, there could be no causal link at all between fault geometry and aseismic slip but simply the fact that fault geometry derives from the slip behavior. In this paper, the proposed mechanism is that geometry controls slip. It could be as well that slip controls geometry and that there is no feedback loop at all. In such case, an independent cause (material rheology?) explains the presence of aseismic slip and where slip is seismic, fault geometry gets more and more complex with repeating ruptures damaging the medium and creating new fractures without any influence on the slip behavior.

In such case, fault geometry does not control aseismic slip but the opposite. Second, and this is more important and less debatable, if there is truly a correlation between aseismic slip and fault misalignment, this would mean that aseismic slip is a long term geological feature. It would mean that segments would hold aseismic slip for periods on the order to the characteristic time scale for fault geometric re-organisation. I am not sure such time scale has been precisely pinned down, but it probably relates more to geological time scales (>100 kyrs ?) than to seismic ones (1s - 100 yrs). This is a very interesting point to discuss, in my opinion, in the frame of the ongoing discussion about relating seismic to geologic time scales.

- I find it quite frustrating to not have a first attempt at a physical, quantitative explanation. Although some elements are proposed in the paper, it is also written that models considering geometry are inconclusive, with contradicting conclusions. What would be the effect of locking geometrical complexities in the dynamics of slip pulses? Why would these slip pulses become dynamic while they would remain slow and potentially coalesce in time to become continuous slip along smooth fault structures? Should we consider the stress at fault tips and its summation over many small faults onto a main fault plane to describe the stress field in which these slip instabilities would have to advance? Is that something that could be explored analytically?

In conclusion, I think this paper presents some very intriguing observations that deserve to be published in a wide audience journal to re-ignite the search for mechanisms explaining slow vs fast slip (a search that has been stuck in the same corner for too long in my opinion). However, I would expect some additional proof to substantiate the main claim.

Looking forward to read an improved version of this interesting manuscript, if possible.

Cheers
Romain Jolivet, PhD
École normale supérieure, Paris

Referee #2:

This paper addresses the question of why some faults fail slowly (creep), instead of failing in large earthquakes. This is one of the most important questions in earthquake science today, as the slip style completely changes the potential hazard that the fault presents to society. Here, the authors hypothesize that fault complexity, which they define as how parallel the fault of interest is to the nearest surrounding faults. Using the Quaternary fault database, they find that creeping faults tend to be more parallel to surrounding faults, making the fault system less complex. The idea that complex fault networks would be harder to deform is not new, but the application specifically to creeping faults is interesting and the authors have developed a way to quantify it.

It is always a little dicey to assume that surface fault expression extends to depth, but that is something we do in earthquake science a lot. The authors then use the fault database to examine whether other parameters – fault roughness, S-wave velocity and heat flow – have any correlation with fault creep. Roughness does, which is not surprising as this is related to fault complexity, but the others do not. This part of the paper is much weaker and I would recommend cutting or changing this section significantly to make this suitable for publication. Otherwise, the text is well-written and clear, and the references are mostly appropriate.

The most popular hypothesis for fault creep right now is the presence of weak mineral phases in fault gouge that lead to low coefficient of friction and low frictional restrengthening over time, which the authors explain well in the paper. The authors use the S-wave velocity in the rocks surrounding the fault as a proxy for fault lithology but this is flawed for a few reasons. First, the gouge lithology of interest that could control fault creep is only 10s or maybe 100 m wide in places, and the S-wave velocity is smoothed over greater length scales than that. Plus, the amount of the weak phases of interest within the fault gouge can be a small fraction of the lithology and still weaken the fault, thereby making them nearly undetectable.

Second, S-wave velocity is a pretty blunt tool to approximate lithology at all, as was famously shown at SAFOD where they anticipated drilling through granite until they reached the fault, only to find a sandstone body that was indistinguishable from the granite through seismic data. Third, changes in the S-wave velocity around the fault may represent the presence of damage rather than lithological changes. The authors acknowledge this proxy “is significantly incomplete as a characterization of geology” and I would wholeheartedly agree with that. The fact that they did not find a correlation between S-wave velocity and creep does not rule out the role of gouge lithology in controlling fault creep.

Similarly, the comparison to heat flow feels a little off the mark as this isn’t one of the main hypotheses for creep in the literature right now. The authors cite a paper for the heat flow argument, which I was unfamiliar with, but when I read that paper it is actually arguing that weak mineral phases and fluids are more likely to be present in hydrothermal areas, like volcanos, and was

the likely source of fault creep in their study. So that paper is really arguing more for lithology being the controlling factor again. As mentioned above, the heat flow paper also discussed the potential importance of high fluid pressures in making faults creep, something that was not tested at all in this paper (and for good reason, the data don't exist, but that should be kept in mind when considering whether fault complexity is the "controlling factor".)

A more minor issue I had with the paper is that a figure showing where the bins are drawn to determine fault complexity (as shown in the original Chu et al (2021) paper) should be added to the supplement.

In summary, although this paper makes a good argument that fault complexity is a key piece in determining whether a fault can creep, it does not rule out that other factors are equally as important. This being geology, there is probably not one thing that determines whether a fault creeps, and the authors acknowledge this to some extent in the discussion. For this paper to be published, the text needs to be edited to reflect this more nuanced view of what controls fault creep.

Author Rebuttals to Initial Comments:

Referee 1: In this paper, the authors relate the geometry of fault networks to the slip behavior. While most faults are locked most of the time, leading to the accumulation of elastic energy in the crust, some other faults slip aseismically, quietly accommodating all or part of the motion imposed by tectonic plates. Current models explaining the slip behavior of faults over the so-called earthquake cycle consider a fault as a frictional interface mostly governed by the constitutive properties of the friction law. In such a framework, the rheology of the rocks forming the fault material hence control the slip behavior. Although quite successful at explaining first order features, lots of issues remain and seem to point that the direct link between rheology and slip behavior is not that obvious.

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We thank the referee for the positive comments.

However, I have some significant concerns that require some work and in my opinion some, even partial, re-writing of the paper here and there, in order to make it more convincing and potentially enlarge the scope of the discussion.

One big issue is that there is a significant focus on the San Andreas Fault, once again, and the correlation found is halfway convincing. Although this might be difficult, such correlation should be found along other creeping faults in the world and this would strengthen the argument a lot. The amount of work to extract creep rate is not that big considering a lot of creeping faults have now been mapped by InSAR (see Chaman fault, Leyte fault, NAF, El Pilar fault, ...). The more challenging task could be to get accurate enough fault mapping to extract fault misalignment along these sections but it should be at least tried in order to avoid a too much California-centered interpretation of fault behavior.

We appreciate the reviewer's concern against being too California-centric, and we have taken their suggestion to analyze the best characterized creeping faults elsewhere in the world. We found the fault and creep data to be sufficient for the North Anatolian Fault and the Chaman Fault, and have added analyses of these two fault segments (see new Extended Data Figure 7). Although the data are not as good as in California, we found similar negative correlations between fault misalignment and surface creep rates at both of these faults. The additional analysis is now outlined in the Methods section (new section titled 'The North Anatolian Fault and Chaman Fault'). Discussion is added on Lines 95-100 of the manuscript. While a comprehensive global study would be useful, unfortunately the data are not yet good enough worldwide for this to be feasible. We have chosen to keep our focus on California, while presenting the data from the two previously mentioned fault strands as supporting evidence that the phenomenon is potentially universal.

The paper is not always clear in the interpretation of the correlation. Sometimes, it reads as if there was an established causal link from geometry to fault slip behavior while the paper “only” demonstrates a correlation. For instance, the title seems to indicate a direct, one-to-one relationship between geometry and slip behavior while it is written later on that some low misalignment faults are locked. If misalignment was the “predominant controlling factor” as written in the abstract, then we should not see locked segments with low misalignment. There is also mention of a “strong explanatory power for the occurrence of fault creep” on Line 53, which sounds like a direct relationship not really verified by the data. Figure 4 writes that “geometrical locking controls slip behavior” again, which is only an interpretation for now. This is potentially some unnecessary bragging (sorry, probably not the right term but english is not my first language) which could lead to some confusion from the correlation shown by the data to a potential, but not demonstrated, causal link which should be investigated in further studies. I don’t think the paper should cross the line of such confusion to justify its impact as the sole correlation, if strengthened, would be enough to make it an impactful paper, in my opinion.

We apologize that our original manuscript could have been clearer and less overstated in a few instances. We agree with the reviewer that the primary evidence we provide is a correlation between fault geometry and fault slip behavior and that all causative links are suggestive. To address these concerns, we changed “the predominant controlling factor” in the abstract to “a primary controlling factor” (Line 19), and note that later in the abstract we acknowledge that “frictional behavior is determined by a combination of geometric factors and rheological yielding properties”. We have further edited the tone of our manuscript in discussing the implications for fault slip behavior, such as changing “the dominant factor” to “a dominant factor” (Line 152), “is the primary controlling factor” to “may be a primary controlling factor” (Line 154) and “indicate” to “intimate” (Line 161).

There are two places, however, that we would like to retain our original language. Firstly, we prefer our original title “Fault Network Geometry Determines Large-Scale Earthquake Frictional Behavior” (now shortened as “Fault Network Geometry Determines Earthquake Frictional Behavior”) since it concisely encapsulates the focus of our paper in a straightforward manner, consistent with the common practice in papers published in Nature. We note that a careful reading of the title does not imply geometry is the “only determining factor”. Despite our preference, we are open to alternatives such as “Fault Network Geometry Influences Earthquake Frictional Behavior” if the current title is still deemed misleading.

Secondly, at the end of the first section of our manuscript (Line 53-55), we write that ‘metrics of geometrical fault network complexity have strong explanatory power for the occurrence of fault creep’. We hold that this statement does not inherently suggest causality and instead suggests that the fault complexity variable can serve as a reliable predictor of fault creep, providing a useful proxy (variable) with explanatory capability, as in the sense of a regression (which does not imply causality). We understand that the reader/referee may not have read our statement in a statistical sense, so we have clarified by adding the word ‘statistical’ to that sentence.

On line 188, the authors mention the possibility of a feedback loop between the slip behavior of faults and the geometrical configuration of the mapped faults. This point led me to consider two things. First, there could be no causal link at all between fault geometry and aseismic slip but simply the fact that fault geometry derives from the slip behavior. In this paper, the proposed mechanism is that geometry controls slip. It could be as well that slip controls

geometry and that there is no feedback loop at all. In such case, an independent cause (material rheology?) explains the presence of aseismic slip and where slip is seismic, fault geometry gets more and more complex with repeating ruptures damaging the medium and creating new fractures without any influence on the slip behavior.

In such case, fault geometry does not control aseismic slip but the opposite. Second, and this is more important and less debatable, if there is truly a correlation between aseismic slip and fault misalignment, this would mean that aseismic slip is a long term geological feature. It would mean that segments would hold aseismic slip for periods on the order to the characteristic time scale for fault geometric re-organisation. I am not sure such time scale has been precisely pinned down, but it probably relates more to geological time scales (>100 kyrs ?) than to seismic ones (1s - 100 yrs). This is a very interesting point to discuss, in my opinion, in the frame of the ongoing discussion about relating seismic to geologic time scales.

We agree with the referee that our findings raise questions for which definitive answers remain elusive. We have incorporated the referee's questions and comments into the discussion (Lines 200-209), and rearranged the paragraph to indicate that other explanations are also possible.

I find it quite frustrating to not have a first attempt at a physical, quantitative explanation. Although some elements are proposed in the paper, it is also written that models considering geometry are inconclusive, with contradicting conclusions. What would be the effect of locking geometrical complexities in the dynamics of slip pulses? Why would these slip pulses become dynamic while they would remain slow and potentially coalesce in time to become continuous slip along smooth fault structures? Should we consider the stress at fault tips and its summation over many small faults onto a main fault plane to describe the stress field in which these slip instabilities would have to advance? Is that something that could be explored analytically?

We acknowledge the importance of the questions raised by the referee. However, the primary objective of this paper is to present firsthand observational evidence suggesting a new factor that explains fault creep, and the significant questions that these observations raise cannot be fully answered until this observational evidence is documented. We do agree that we could speculate slightly more than in our original manuscript, and we have added clarification about one possible physical mechanism that would explain the suggested new framework for why faults creep (Line 163-166). We think that a comprehensive modeling effort to explore these possibilities is beyond the scope of the current contribution, but would be a very interesting follow up study.

In conclusion, I think this paper presents some very intriguing observations that deserve to be published in a wide audience journal to re-ignite the search for mechanisms explaining slow vs fast slip (a search that has been stuck in the same corner for too long in my opinion). However, I would expect some additional proof to substantiate the main claim. Looking forward to reading an improved version of this interesting manuscript, if possible.

We thank the reviewer for the positive comments. As detailed above, we have provided additional evidence from the North Anatolian Fault and Chaman Fault. While our study is still focused on California, due to the availability of high-quality data, these other data are consistent with our claim.

Referee 2: This paper addresses the question of why some faults fail slowly (creep), instead of failing in large earthquakes. This is one of the most important questions in earthquake science today, as the slip style completely changes the potential hazard that the fault presents to society.

Here, the authors hypothesize fault complexity, which they define as how parallel the fault of interest is to the nearest surrounding faults. Using the Quaternary fault database, they find that creeping faults tend to be more parallel to surrounding faults, making the fault system less complex. The idea that complex fault networks would be harder to deform is not new, but the application specifically to creeping faults is interesting and the authors have developed a way to quantify it.

We thank the referee for the positive comments.

It is always a little dicey to assume that surface fault expression extends to depth, but that is something we do in earthquake science a lot. The authors then use the fault database to examine whether other parameters – fault roughness, S-wave velocity and heat flow – have any correlation with fault creep. Roughness does, which is not surprising as this is related to fault complexity, but the others do not. This part of the paper is much weaker and I would recommend cutting or changing this section significantly to make this suitable for publication. Otherwise, the text is well-written and clear, and the references are mostly appropriate.

We have responded in more detail to the referee's next comment related to the lack of correlation with other parameters, but note here that while we have not removed any of the results, we have modified the text to de-emphasize the importance of the lack of correlation found with S-wave velocity and heat flow. We fully agree with the referee that we cannot use these results to exclude the possibility that lithology partial controls frictional behavior. Nonetheless, we think it is important to document the (lack of) correlations.

The most popular hypothesis for fault creep right now is the presence of weak mineral phases in fault gouge that lead to low coefficient of friction and low frictional restrengthening over time, which the authors explain well in the paper.

The authors use the S-wave velocity in the rocks surrounding the fault as a proxy for fault lithology but this is flawed for a few reasons. First, the gouge lithology of interest that could control fault creep is only 10s or maybe 100 m wide in places, and the S-wave velocity is smoothed over greater length scales than that. Plus, the amount of the weak phases of interest within the fault gouge can be a small fraction of the lithology and still weaken the fault, thereby making them nearly undetectable.

Second, S-wave velocity is a pretty blunt tool to approximate lithology at all, as was famously shown at SAFOD where they anticipated drilling through granite until they reached the fault, only to find a sandstone body that was indistinguishable from the granite through seismic data. Third, changes in the S-wave velocity around the fault may represent the presence of damage rather than lithological changes. The authors acknowledge this proxy “is significantly incomplete as a characterization of geology” and I would wholeheartedly agree with that. The fact that they did not find a correlation between S-wave velocity and creep does not rule out the role of gouge lithology in controlling fault creep.

We agree with the referee and acknowledge that using shear wave velocity as a proxy has limitations in accurately characterizing lithologies with fault gouge, due to both low resolution and uncertainty in data interpretation. However, despite attempts to identify fault lithologies in creeping faults (e.g., Moore et al., 2015), it remains true that there is a lack of available data to directly compare fault lithologies across all of California. Therefore, while we acknowledge that shear wave velocity offers an incomplete characterization of fault lithologies, it currently stands as the sole method available for quantifying the impact of lithology on fault creep at the scale we are investigating, and for comparing it with the influence of fault complexity.

Moreover, the lack of correlation between shear wave velocity and fault creep does provide some insight, suggesting that ‘local’ crustal properties may not play a significant role in fault behavior – an insight that is still valuable despite its significant limitations. For these reasons, we have decided to keep this section, despite the criticisms, and have added text to explicitly clarify the significant limitations of our proxies as well as to emphasize that the role of fault gouge lithology in controlling fault creep should not be disregarded (Line 141-144). We hope this clarifies to readers both the limitations and implications of our analysis.

Similarly, the comparison to heat flow feels a little off the mark as this isn't one of the main hypotheses for creep in the literature right now. The authors cite a paper for the heat flow argument, which I was unfamiliar with, but when I read that paper it is actually arguing that weak mineral phases and fluids are more likely to be present in hydrothermal areas, like volcanos, and was the likely source of fault creep in their study. So that paper is really arguing more for lithology being the controlling factor again. As mentioned above, the heat flow paper also discussed the potential importance of high fluid pressures in making faults creep, something that was not tested at all in this paper (and for good reason, the data don't exist, but that should be kept in mind when considering whether fault complexity is the “controlling factor”).

While the impact of heat flow on creep may not be straightforward, it is one of the few hypotheses for creep where the geophysical data to test the association are available. Our decision to include this proxy was motivated by a prior study (Hauksson, 2015) exploring various factors potentially affecting earthquake rupture behavior. Our intention was to highlight that the negative correlations observed between fault misalignment and creep is not found with any of the other available geophysical measurements (that have been previously suggested to explain creep). This in turn implies that these other geophysical parameters and paradigms may not be as significant, while acknowledging limitations in the data. Since this intention may not have been clear, we have revised the Correlation Analysis section (Line 141-146) to clarify the difficulty of assessing the relative impact of many different individual factors, while also acknowledging the significance of geology.

A more minor issue I had with the paper is that a figure showing where the bins are drawn to determine fault complexity (as shown in the original Chu et al (2021) paper) should be added to the supplement.

We have taken the referee's suggestion and illustrated the regions for fault complexity measurements in the Extended Data Figure 2a.

In summary, although this paper makes a good argument that fault complexity is a key piece in determining whether a fault can creep, it does not rule out that other factors are equally as important. This being geology, there is probably not one thing that determines whether a fault creeps, and the authors acknowledge this to some extent in the discussion. For this paper to be published, the text needs to be edited to reflect this more nuanced view of what controls fault creep.

We thank the referee for the positive comments. We agree completely that we have not ruled out that other factors can be important. However, we do think that the section comparing correlations with other metrics is still useful even though it does not accomplish this, since we think it highlights the challenge of utilizing some traditional arguments at a California-wide scale. We have edited this section to clarify the nuanced view referred to by the referee,

including clarifying that fault gouge properties may still be important for whether faults creep or not.

Reviewer Reports on the First Revision:

Referee #1:

The authors have nicely addressed my comments and I do not have further questions. Once again, although I am not certain of the validity of the main claim at a global scale, we cannot stay stuck with rate-and-state models and their very low predictive ability (in the sense of a forecast). Therefore, I believe there needs to be a new push for alternative models explaining the occurrence of aseismic slip and such puzzling observations are exactly what we need.

I just have one minor point. I would prefer the word “influences” rather than “determines” in the title, as suggested by the authors. Additionally, in the abstract, line 19, it should be made clear that all factors invoked in the literature could not be tested, unfortunately. For instance, no one can test the influence of lithology at 10 km-depth along a strike slip fault with such accuracy although the influence of fault gouge composition is still one of the main models for aseismic slip along continental faults.

Looking forward to read this article published in Nature,

Cheers,

Romain Jolivet, PhD

École normale supérieure, Paris

Referee #2:

I enjoyed the updates that the authors made to the paper regarding adding more nuance to the discussion. I think the text of the paper is now much stronger. I would love at this time to recommend for review but unfortunately the decision to keep the comparisons of correlation to shear wave velocity and heat flow keeps me from recommending this paper. It is disingenuous to suggest that these are proxies for the popular current hypotheses of what makes faults creep (fault lithology and pore pressure). Shear wave velocity (especially over the scale shown on the California map in Extended Figure 2) is not an indication of fault gouge lithology. Full stop. The resolution on that map is at the scale of the Coast Ranges. If the authors were making a point that shear modulus of the surrounding wallrock does not correlate with fault creep, I would be fine with that. I think this section really distracts from what is otherwise a very interesting paper.

Author Rebuttals to First Revision:

Referee #1:

The authors have nicely addressed my comments and I do not have further questions. Once again, although I am not certain of the validity of the main claim at a global scale, we cannot stay stuck with rate-and-state models and their very low predictive ability (in the sense of a forecast). Therefore, I believe there needs to be a new push for alternative models explaining the occurrence of aseismic slip and such puzzling observations are exactly what we need.

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We thank the referee for the positive comments. We have incorporated your suggested changes to the title and abstract.

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We appreciate the referee’s feedback. In response to the concerns, we have removed the sections and figure panels that compared fault creep to shear wave velocity and heat flow. To address the difficulty of these comparisons, we have clarified the challenges in lines 109-113 of the manuscript: “Fault network misalignment does not account for all of the observed variability in creep rates. However, robust correlations between fault creep behavior and properties such as gouge mineralogy, heat flow and pore-fluid pressure are hard to assess due to the difficulty in resolving local fault-scale properties at depth.” We trust that this modification adequately addresses the reviewer’s concerns.