Reviews

Editor

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "The normal stress dependence of rock friction: The friction of rocks". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some revisions. Both reviews are very constructive, and I think the comments are very clear. Most comments are about clarification of statements and some added discussion - there is little need for additional analysis, and I therefore consider the revisions relatively minor.

I do not have anything substantial to add. One editorial point is that our guideline is to keep figures and tables to no more than 10 if possible. I note you have some extra - I wonder if some of the plots can be combined, or potentially made supplementary? That also includes the suggested additional figure from Reviewer B, and I like their idea of clarifying what friction quantity you are measuring with a figure.

I also agree with Reviewer B that using “I” rather than “we” makes sense in a single-author paper for instances where you describe something you did, or you are making acknowledgments.

Reviewer A:

Review of “The normal stress dependence of rock friction” by Barbot

This manuscript is a review of historic literature on the normal stress dependence of frictional strength and the coefficient of friction, where the author argues for a power law dependence of sliding friction and effective normal stress. Their compilation reveals that friction reduces with increasing normal stress for most natural rock and synthetic materials, but in a limited number of fluid-saturated phyllosilicates, friction actually increases at higher normal stresses. Finally, one outcome of this compilation is the inference that when this observed power-law dependence is accounted for, cohesion drops to zero (or nearly so) for rough fractures.

This manuscript represents a nice and thorough compilation of rock friction data across a range of normal stresses spanning nearly 9 orders of magnitude. My recommendation is that several discussions and arguments are somewhat strong or unclear and once they are more thoroughly explained/clarified, this manuscript will be a nice addition to literature on the role of normal stress on friction.

Requested revisions:

1. Abstract Lines 17-18: While I do not disagree with this sentiment, I would argue that it is pretty common knowledge that the linearity of friction only holds for a small range of normal stresses, and I doubt many practitioners of friction actually believe that friction can be represented as a linear function of normal stress over ~9 orders of magnitude. This sentence ultimately feels like an unnecessary “hook” where there probably is none.

2. Lines 63-65:"*Although some studies…inadequately constrained*" This statement implies to me that this manuscript is going to somehow constrain the underlying physics but I'm not sure that this has been done in the manuscript.

3. Lines 76-77, Lines 280-281, and other discussions of cohesion: A minor question is whether the author could further quantify what the "macroscopic scales where fault roughness dominates" are. A second, more substantive comment on cohesion is the following. While I agree that these datasets show negligible cohesion in the power-law plots, independent measurements of non-zero cohesion have also been made. For example, Ikari and Kopf (2011) and their subsequent works estimate non-zero, albeit low values of cohesion directly as the shear strength of clay-rich sediment samples when the normal stress on them is zero. This cohesion also appears to be normal stress or consolidation history-dependent, with higher values of this cohesion for samples that have experienced greater normal stresses in the past. Further, Volpe et al. (2025), Affinito et al. (2025), and Thomas et al. (2025) all speak to the role of cementation-induced cohesive strengthening under a variety of conditions. Taken together, I am curious as to how one can reconcile these observations in light of the author's interpretation that cohesion is vanishingly small in a power-law model. Personally, I question whether "cohesion" in a shear-normal stress space is useful as anything beyond a mathematically convenient y-intercept. In other words, it occurs to me that cohesion as we directly estimate in experiments it is not the same as the quantity that is inferred by extrapolating a friction-stress relationship to zero effective normal stress.

4. Lines 83-84: This appears to be a rather strong statement. I don't disagree that this is a very comprehensive compilation of normal stress dependent friction, but it does not include, say, Kilgore et al. (2017), Hong and Marone (2005), Shreedharan et al. (2019), Bedford and Faulkner (2021), just to name a few that I found from entering "normal+stress+friction" on Google Scholar. These studies are all "high quality" and document friction v. normal stress for "atleast three normal loads". The bar for the author's statement is high, and I am uncertain that it has been fulfilled at this point. I encourage either a more thorough review of published friction data or to soften this statement particularly as there are only 1-2 post-2018 studies in this compilation at least according to Table 1.

5. Lines 106-107: It appears that the compilation does not discriminate between apparatus types (triaxial, double-direct shear, rotary shear). Keeping in mind that apparatus type can certainly affect friction, has the author explored or quantified uncertainties in observations, say their stress-dependence exponent, arising due to apparatus types?

6. Power law exponent "beta":

1. How were errors in the power-law exponent beta calculated? Is this simply from a summation or were the datasets somehow bootstrapped?

2. Given the broad range in number of data points per dataset, what is the effect of uncertainties in any number of points on the uncertainties in “beta”? How robust is the estimation of “beta”?

3. If the power-law relationship is true, “beta” likely encodes physics, i.e., information about mineralogy, osmotic pressures etc. What is the effect of mineralogy on beta beyond the broad categorical relationship between beta and the presence/absence of phyllosilicates and framework silicates? Additional analyses or development in this direction could add new discussions to this manuscript beyond merely a review of other studies on this topic.

References cited in review:

Affinito, R., Volpe, G., Calzolari, L., Mittal, T., Pozzi, G., & Marone, C. (2025). Fluid-driven cohesive strengthening: critical role of reaction kinetics as the determinant for frictional stability. *Authorea Preprints*.

Bedford, J. D., & Faulkner, D. R. (2021). The role of grain size and effective normal stress on localization and the frictional stability of simulated quartz gouge. *Geophysical Research Letters*, *48*(7), e2020GL092023.

Hong, T., & Marone, C. (2005). Effects of normal stress perturbations on the frictional properties of simulated faults. *Geochemistry, Geophysics, Geosystems*, *6*(3).

Ikari, M. J., & Kopf, A. J. (2011). Cohesive strength of clay-rich sediment. *Geophysical research letters*, *38*(16).

Kilgore, B., Beeler, N. M., Lozos, J., & Oglesby, D. (2017). Rock friction under variable normal stress. *Journal of Geophysical Research: Solid Earth*, *122*(9), 7042-7075.

Shreedharan, S., Rivière, J., Bhattacharya, P., & Marone, C. (2019). Frictional state evolution during normal stress perturbations probed with ultrasonic waves. *Journal of Geophysical Research: Solid Earth*, *124*(6), 5469-5491.

Thomas, A. M., Watkins, J. M., Beeler, N., French, M. E., Behr, W. M., & Reed, M. H. (2025). Rapid fault healing from cementation controls the dynamics of deep slow slip and tremor. *Science Advances*, *11*(47), eadz2832.

Volpe, G., Affinito, R., Calzolari, L., Pozzi, G., Marone, C., & Collettini, C. (2025). The influence of cementation on fault stability. *Earth and Planetary Science Letters*, *671*, 119674.

Reviewer B:

Review: The normal stress dependence of rock friction

Summary: The author reviews the state of the art in terms of normal stress dependence in rock friction and suggests an alternative model of power-law dependence of friction coefficient of normal stress that resolves some of the limitations of linear and piecewise linear models. There is substantial supporting evidence that the power-law dependence provides a better fit to experimental data, and this description fits with theoretical arguments that the normal stress dependence is related to the real contact area of contacting surfaces. If think that most of my concerns can be addressed with some additional discussion, and possibly an additional introductory figure. I elaborate on these points below.

Recommendation: Minor/Moderate Revision

Main Points:

1. In the interest of making sure we are comparing apples to apples, the manuscript could be strengthened significantly with a discussion – a la Byerlee (1978) - of exactly what friction quantity is being considered in the paper. Byerlee was very careful in his seminal paper to describe exactly which point in frictional evolution he was considering (Point D in his Figure 2), and this paper uses that paper as an important reference point. However, it seems that the current paper may be mixing static friction and sliding (kinetic) friction quantities in its analysis, or at least a careful discussion of this choice is missing.

2. While the improved fit provided by the power-law relationship eq 1 is convincing, and its relationship with the real contact area is a meaningful improvement over the purely empirical description provided by a linear or piecewise linear fit, I would like to see a direct discussion of the high residuals that still exist at low pressures for both the piecewise linear fit of Byerlee and the power-law relationships of the present manuscript. Byerlee's "low pressure" linear fit comes from his "moderate pressure" dataset, and he argues the poor fit at low normal stresses (less than 10MPa) where scatter is very large is due to surface roughness. I notice in many of your plots where data extends below 10MPa, the residuals are much larger than the higher normal stress portions as well. This doesn't seem to be adequately described by the power law model, so some discussion is warranted.

3. I do wonder about the general utility of the model. Even though it is purely empirical, the linear and piecewise linear models were elegant in both their simplicity and general applicability, once the domain of consideration reached normal stress values of geological and geophysical interest (>> 10MPa). Most rock types and most common rock forming minerals could be described with a very narrow range of coefficients, which is powerful when considering that our ability to constrain rock composition at those depths is difficult to do with any certainty. The power-law model presented in this manuscripts requires coefficients that vary up to several

orders of magnitude and in sign (prefactors from 0.004 to 7.18 and exponents that vary from -0.72 to +0.30) depending on composition. Given that most of the improved fit occurs in the lower end of the normal stress range, does this affect the value of a power-law description of normal stress dependence?

Line-by-line comments:

Line 59: By "kinematic friction", do you mean "kinetic friction"?

Line 61: Certainly in textbooks and I'm sure some papers, authors refer to the y-intercept of the linear fit as a "cohesion" term, but it also is important to note that Byerlee never made any such assertion. Any reference to cohesion should refer to specific primary sources (other than Byerlee (1978)).

Lines 62: This is where it seems there is possibly some intermixing of different definitions of friction. Since Byerlee factors prominently in this paper, it is worth mentioning explicitly that his formulation is not for sliding friction, but for static friction, and he is quite careful to define exactly what he is talking about. I recommend in this line inserting the word "static" before the word "friction" and adding a brief discussion – and perhaps a figure – that provides the exact definition of the friction quantity you are discussing and that considered by the "prevailing models"

Line 82 (and elsewhere): I am by no means an authority on this since ALL of my papers have at least one other author, but my expectation in a single authored paper like this is that you would use "I" rather than "we" in any instances that refer specifically to something you did. "we" is appropriate when including the reader as in observing a trend in a figure.

Line 107: "surface" should be plural in "…engineered surface(s) made of glass microspheres."

Figure 3: Note that even with the power law fit, the residuals are still relatively high at lower stresses (~1-5MPa) for siltstone (B), mudstone (C), and shale (D), and Darley Dale sandstone (A) does not include any data for normal stresses <10MPa.

Line 275: "Analyzing rock friction data in linear coordinates can be misleading, ….": And yet, I don't see any specific refutation in this paper of Byerlee's original interpretation that in the low normal stress regime that roughness is dominant.

Dear Editor Prof. Fagereng,

Thank you for your positive comments regarding this manuscript. I am grateful for your comments and the two additional reviews. I provide my detailed response below. The key modifications in the revised manuscript are the following: I moved 2 figures and 1 table to the Supplementary Materials so that only 1 table and 9 figures are found in the main text, meeting the *Seismica* standards. I have added friction datasets from a few other studies. I have added 2 panels in the last figure to better illustrate the physical origin of the power-law model for rock friction. The changes to not alter the original conclusions of the study.

Best Regards,

Sylvain Barbot

Editor: Based on reviews I have received, your manuscript may be suitable for publication after some revisions. Both reviews are very constructive, and I think the comments are very clear. Most comments are about clarification of statements and some added discussion - there is little need for additional analysis, and I therefore consider the revisions relatively minor.

Reply: Thank you for your positive assessment.

Editor: I do not have anything substantial to add. One editorial point is that our guideline is to keep figures and tables to no more than 10 if possible. I note you have some extra - I wonder if some of the plots can be combined, or potentially made supplementary? That also includes the suggested additional figure from Reviewer B, and I like their idea of clarifying what friction quantity you are measuring with a figure.

Reply: I moved 2 figures and 1 table to the supplementary materials. I added an inset in Figure 1 to illustrate the focus of the article on sliding (kinetic) friction.

Editor: I also agree with Reviewer B that using “I” rather than “we” makes sense in a single-author paper for instances where you describe something you did, or you are making acknowledgments.

Reply: I have changed the text accordingly.

Editor: Reviewers should be recognised in the Acknowledgements.

Reply: I now acknowledge the reviewer’s comments in Ackowledgements.

Editor: All author information is provided (ORCID, affiliation, contribution) and is coherent with the information entered at submission (OJS metadata).

Reply: The manuscript readily provides my ORCID number.

Editor: Data & code availability and reproducibility statements should be provided. Check that any Zenodo repository is linked to the Seismica community.

Reply: I have submitted a Zenodo repository for review by Seismica. I also contacted Seismica to inform them of the review request.

Editor: Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

Reply: Thank you. I hope you will find the revised manuscript suitable for publication.

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

Reply: I have added acknowledgements accordingly.

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

Reply: I appreciate the swift process.

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

Reply: Thank you for your kind comments. Best wishes.

Reviewer A: Review of “The normal stress dependence of rock friction” by Barbot

This manuscript is a review of historic literature on the normal stress dependence of frictional strength and the coefficient of friction, where the author argues for a power law dependence of sliding friction and effective normal stress. Their compilation reveals that friction reduces with increasing normal stress for most natural rock and synthetic materials, but in a limited number of fluid-saturated phyllosilicates, friction actually increases at higher normal stresses. Finally, one outcome of this compilation is the inference that when this observed power-law dependence is accounted for, cohesion drops to zero (or nearly so) for rough fractures.

This manuscript represents a nice and thorough compilation of rock friction data across a range of normal stresses spanning nearly 9 orders of magnitude. My recommendation is that several discussions and arguments are somewhat strong or unclear and once they are more thoroughly explained/clarified, this manuscript will be a nice addition to literature on the role of normal stress on friction.

Reply: Thank you for your constructive comments. I appreciate your time and efforts.

Reviewer A: Requested revisions:

Abstract Lines 17-18: While I do not disagree with this sentiment, I would argue that it is pretty common knowledge that the linearity of friction only holds for a small range of normal stresses, and I doubt many practitioners of friction actually believe that friction can be represented as a linear function of normal stress over ~9 orders of magnitude. This sentence ultimately feels like an unnecessary “hook” where there probably is none.

Reply: I suspect some workers believe strongly in the validity of the linear model with cohesion. I agree that others may find the results readily obvious. There is no strong consensus on the phenomenon nor on the underlying physics.

Reviewer A: Lines 63-65:”Although some studies…inadequately constrained” This statement implies to me that this manuscript is going to somehow constrain the underlying physics but I’m not sure that this has been done in the manuscript.

Reply: I have added explanatory data in Figure 9cd. The theory is otherwise described in detail in Barbot (2024a) and in a review paper (Barbot 2025).

Reviewer A: Lines 76-77, Lines 280-281, and other discussions of cohesion: A minor question is whether the author could further quantify what the “macroscopic scales where fault roughness dominates” are.

Reply: I changed the sentence to "*Notably, cohesion becomes negligible at macroscopic scales where fault roughness dominates, as a great number of micro-asperities bear the shear and normal loads.*"

Reviewer A: A second, more substantive comment on cohesion is the following. While I agree that these datasets show negligible cohesion in the power-law plots, independent measurements of non-zero cohesion have also been made. For example, Ikari and Kopf (2011) and their subsequent works estimate non-zero, albeit low values of cohesion directly as the shear strength of clay-rich sediment samples when the normal stress on them is zero. This cohesion also appears to be normal stress or consolidation history-dependent, with higher values of this cohesion for samples that have experienced greater normal stresses in the past.

Reply: The "cohesion" described by Ikari & Kopf (2011) is for the peak strength after a long period of consolidation in truly stationary contact. In addition, their concept of cohesion involves a normal stress dependence, contrarily to the definition of normal-stress independence used in my work. My paper focuses on the residuals strength or kinetic friction. I added an inset in Figure 1 to clarify the focus of the study.

Reviewer A: Further, Volpe et al. (2025), Affinito et al. (2025), and Thomas et al. (2025) all speak to the role of cementation-induced cohesive strengthening under a variety of conditions. Taken together, I am curious as to how one can reconcile these observations in light of the author's interpretation that cohesion is vanishingly small in a power-law model.

Reply: The data from Volpe et al. (2025) is better described by a power-law model (Barbot 2026a). However, the revised dataset provided by Volpe et al. (2026) requires a small cohesion strength. I added Figure S6 to describe their extended dataset from 0.5 MPa to 80 MPa. Furthermore, I added the following text: "*However, cohesion may still play an important role in natural faults, especially under the high-temperature, high-pressure conditions of the middle crust where cementation and lithification may be efficient at the time scales of interseismic period of the seismic cycle, as suggested by Thomas et al. (2025). Experiments on wet Pozzolanic mortar involving holds longer than 3,000 s in quasi-stationary contact indicate a contribution of cohesion of the order of tens of kPa (Volpe et al. 2026) (Figure S6).*" The paper by Affinito et al. (2025) is not yet peer-reviewed nor published.

Reviewer A: Personally, I question whether "cohesion" in a shear-normal stress space is useful as anything beyond a mathematically convenient y-intercept. In other words, it occurs to me that cohesion as we directly estimate in experiments it is not the same as the quantity that is inferred by extrapolating a friction-stress relationship to zero effective normal stress.

Reply: I agree that the concept of cohesion may not be fundamentally captured by a y-intercept in the strength equation. Fundamentally, we need to better understand the role of cementation and lithification. This question is also related to peak strength before first failure and the transition from intact to fractured state. This is a matter of importance for earthquake mechanics but it goes beyond the scope of this study.

Reviewer A: Lines 83-84: This appears to be a rather strong statement. I don't disagree that this is a very comprehensive compilation of normal stress dependent friction, but it does not include, say, Kilgore et al. (2017), Hong and Marone (2005), Shreedharan et al. (2019), Bedford and Faulkner (2021), just to name a few that I found from entering "normal+stress+friction" on Google Scholar. These studies are all "high quality" and document friction v. normal stress for "atleast three normal loads". The bar for the author's statement is high, and I am uncertain that it has been fulfilled at this point. I encourage either a more thorough review of published friction data or to soften this statement particularly as there are only 1-2 post-2018 studies in this compilation at least according to Table 1.

Reply: The studies by Kilgore et al. (2017), Hong and Marone (2005), and Shreedharan et al. (2019) do not provide residual strength as a function of normal stress, but change in friction as a function of normal stress perturbation. I added the data from Bedford et al. (2021). I cite Kilgore et al. (2017), Hong and Marone (2005), and Shreedharan et al. (2019). Upon a more thorough literature search, I added the datasets from 16 studies (Jaeger 1959; Murrell 1965; Lilly 1982; Morrow & Byerlee 1989; Maksimovic 1992; Mutlu & Bobet 2006; Carpenter et al. 2009, 2016; Jang & Jang 2015; Mehrishal et al. 2016; Orellana et al. 2018; Hirauchi et al. 2020b; Chang et al. 2024; Rast et al. 2024; Volpe et al. 2025; Volpe et al. 2026). The materials involved are listed in Table 1.

Reviewer A: Lines 106-107: It appears that the compilation does not discriminate between apparatus types (triaxial, double-direct shear, rotary shear). Keeping in mind that apparatus type can certainly affect friction, has the author explored or quantified uncertainties in observations, say their stress-dependence exponent, arising due to apparatus types?

Reply: I changed the sentence to "*A comprehensive analysis of experimental rock friction data reveals a power-law relationship between frictional strength and effective normal stress across broadly different materials, including silicates, ice, natural and synthetic gouge, and engineered surfaces, spanning a stress range over seven orders of magnitude, irrespective of testing apparatus*."

Reviewer A: Power law exponent “beta”:

How were errors in the power-law exponent beta calculated? Is this simply from a summation or were the datasets somehow bootstrapped?

Reply: I inspect the covariance matrix of the estimated parameters and I conduct a bootstrap analysis. I added the following statement: “*Bootstrap analyses by leaving out 5 to 10 samples indicate uncertainties of the order of 0.05 and 0.02 for* μ_0 *and beta, respectively. The uncertainty on beta is reduced to ±0.01 when only one sample is randomly taken out. Expectedly, datasets with fewer measurements produce more uncertainties during parameter estimation*.”

Reviewer A: Given the broad range in number of data points per dataset, what is the effect of uncertainties in any number of points on the uncertainties in “beta”? How robust is the estimation of “beta”?

Reply: I qualify the effect of number of parameters by selecting out 1, 5, and 10 datapoints. See the reply above.

Reviewer A: If the power-law relationship is true, “beta” likely encodes physics, i.e., information about mineralogy, osmotic pressures etc. What is the effect of mineralogy on beta beyond the broad categorical relationship between beta and the presence/absence of phyllosilicates and framework silicates? Additional analyses or development in this direction could add new discussions to this manuscript beyond merely a review of other studies on this topic.

Reply: I do not see systematic changes in beta beyond the difference between water-saturated phyllosilicate-rich gouge and other samples. Presumably, systematic experimentation on different mineral to address this question is still missing.

Reviewer A: References cited in review:

Affinito, R., Volpe, G., Calzolari, L., Mittal, T., Pozzi, G., & Marone, C. (2025). Fluid-driven cohesive strengthening: critical role of reaction kinetics as the determinant for frictional stability. Authorea Preprints.

Bedford, J. D., & Faulkner, D. R. (2021). The role of grain size and effective normal stress on localization and the frictional stability of simulated quartz gouge. Geophysical Research Letters, 48(7), e2020GL092023.

Hong, T., & Marone, C. (2005). Effects of normal stress perturbations on the frictional properties of simulated faults. Geochemistry, Geophysics, Geosystems, 6(3).

Ikari, M. J., & Kopf, A. J. (2011). Cohesive strength of clay-rich sediment. Geophysical research letters, 38(16).

Kilgore, B., Beeler, N. M., Lozos, J., & Oglesby, D. (2017). Rock friction under variable normal stress. Journal of Geophysical Research: Solid Earth, 122(9), 7042-7075.

Shreedharan, S., Rivière, J., Bhattacharya, P., & Marone, C. (2019). Frictional state evolution during normal stress perturbations probed with ultrasonic waves. Journal of Geophysical Research: Solid Earth, 124(6), 5469-5491.

Thomas, A. M., Watkins, J. M., Beeler, N., French, M. E., Behr, W. M., & Reed, M. H. (2025). Rapid fault healing from cementation controls the dynamics of deep slow slip and tremor. Science Advances, 11(47), eadz2832.

Volpe, G., Affinito, R., Calzolari, L., Pozzi, G., Marone, C., & Collettini, C. (2025). The influence of cementation on fault stability. Earth and Planetary Science Letters, 671, 119674.

Reply: Thank you for your thoughtful review.

Reviewer B: Review: The normal stress dependence of rock friction

Summary: The author reviews the state of the art in terms of normal stress dependence in rock friction and suggests an alternative model of power-law dependence of friction coefficient of normal stress that resolves some of the limitations of linear and piecewise linear models. There is substantial supporting evidence that the power-law dependence provides a better fit to experimental data, and this description fits with theoretical arguments that the normal stress dependence is related to the real contact area of contacting surfaces. If think that most of my concerns can be addressed with some additional discussion, and possibly an additional introductory figure. I elaborate on these points below.

Reply: Thank you for your constructive comments.

Reviewer B: Recommendation: Minor/Moderate Revision

Main Points:

In the interest of making sure we are comparing apples to apples, the manuscript could be strengthened significantly with a discussion – a la Byerlee (1978) - of exactly what friction quantity is being considered in the paper. Byerlee was very careful in his seminal paper to describe exactly which point in frictional evolution he was considering (Point D in his Figure 2), and this paper uses that paper as an important reference point. However, it seems that the current paper may be mixing static friction and sliding (kinetic) friction quantities in its analysis, or at least a careful discussion of this choice is missing.

Reply: The review is careful to consider exclusively sliding (kinetic) friction. I added an inset to Figure 1 to illustrate the focus of the study.

Reviewer B: While the improved fit provided by the power-law relationship eq 1 is convincing, and its relationship with the real contact area is a meaningful improvement over the purely empirical description provided by a linear or piecewise linear fit, I would like to see a direct discussion of the high residuals that still exist at low pressures for both the piecewise linear fit of Byerlee and the power-law relationships of the present manuscript.

Reply: I do not identify systematic higher residuals at low normal stress in Figures 2-5 or in the review Figure 6. The piecewise linear fit of Byerlee predicts a constant friction coefficient at low pressure, firmly inconsistent with observations that showcase a continuous dependence of the friction coefficient on effective normal stress. I changed the statement "*However, the empirical model features discontinuous derivatives at the boundary of stress regimes, making it ill-suited for numerical applications, and implies a* *constant friction coefficient at low normal stress, in stark contrast with the continuous dependence on normal stress observed experimentally.*"

Reviewer B: Byerlee's "low pressure" linear fit comes from his "moderate pressure" dataset, and he argues the poor fit at low normal stresses (less than 10MPa) where scatter is very large is due to surface roughness. I notice in many of your plots where data extends below 10MPa, the residuals are much larger than the higher normal stress portions as well. This doesn't seem to be adequately described by the power law model, so some discussion is warranted.

Reply: In general, the accuracy of stress measurements of deformation apparatuses scales with the maximum stress toleration of the machine. For example, the tabletop apparatus used by Schellart (2000) provides more reliable accuracy near 1 kPa (Figure 4). I added the following statement in the caption of Figure 3b: "*The increase in data variance at low normal stress may be caused by the lower accuracy of triaxial apparatuses at diminishing pressure.*"

Reviewer B: I do wonder about the general utility of the model. Even though it is purely empirical, the linear and piecewise linear models were elegant in both their simplicity and general applicability, once the domain of consideration reached normal stress values of geological and geophysical interest (>> 10MPa).

Reply: Any simplification of more thorough and complete model can be useful. The key consideration is to not confuse a physical model with its simplified approximation. Byerlee (1978) is extremely careful to describe the nonlinear dependence of the friction coefficient to normal stress, providing 3 equations that capture it precisely, including Equation (1) used in my paper. His piecewise linear approximation is useful for practical purposes.

Reviewer B: Most rock types and most common rock forming minerals could be described with a very narrow range of coefficients, which is powerful when considering that our ability to constrain rock composition at those depths is difficult to do with any certainty. The power-law model presented in this manuscripts requires coefficients that vary up to several orders of magnitude and in sign (prefactors from 0.004 to 7.18 and exponents that vary from -0.72 to +0.30) depending on composition. Given that most of the improved fit occurs in the lower end of the normal stress range, does this affect the value of a power-law description of normal stress dependence?

Reply: The normal stress dependence of the friction coefficient is most dramatic in the range of effective normal stress relevant to the seismic cycle in the brittle crust. I added the following statement: "*As earthquakes and slow-slip events may operate under low effective normal stress due to over-pressurized pore fluids, capturing the evolution of frictional* *stress at low pressure is paramount*." The wider range of parameters in the power-law model is due to the larger dataset than the one available in 1978, covering a wider range of minerals, rocks, and other materials.

Reviewer B: Line-by-line comments:

Line 59: By "kinematic friction", do you mean "kinetic friction"?

Reply: Thank you. I corrected accordingly.

Reviewer B: Line 61: Certainly in textbooks and I'm sure some papers, authors refer to the y-intercept of the linear fit as a "cohesion" term, but it also is important to note that Byerlee never made any such assertion. Any reference to cohesion should refer to specific primary sources (other than Byerlee (1978)).

Reply: I agree. I added: "*Accordingly, the piecewise linear model proposed by Byerlee (1978) does not include cohesion at vanishing normal stress, consistent with the data available at the time (Barton 1973, 1976, Byerlee 1968, Handin 1969, Hoskins et al. 1968, Jaeger 1959, Murrell 1965).*" The criticism of the cohesion model does not apply to the work of Byerlee (1978).

Reviewer B: Lines 62: This is where it seems there is possibly some intermixing of different definitions of friction. Since Byerlee factors prominently in this paper, it is worth mentioning explicitly that his formulation is not for sliding friction, but for static friction, and he is quite careful to define exactly what he is talking about. I recommend in this line inserting the word "static" before the word "friction" and adding a brief discussion – and perhaps a figure – that provides the exact definition of the friction quantity you are discussing and that considered by the "prevailing models"

Reply: I added an inset in Figure 1 to describe the focus of the paper on sliding (kinetic) friction. My paper does not criticize the work of Byerlee (1978).

Reviewer B: Line 82 (and elsewhere): I am by no means an authority on this since ALL of my papers have at least one other author, but my expectation in a single authored paper like this is that you would use "I" rather than "we" in any instances that refer specifically to something you did. "we" is appropriate when including the reader as in observing a trend in a figure.

Reply: I agree. Accordingly, I changed all instances of "we/our" to "I/my".

Reviewer B: Line 107: "surface" should be plural in "…engineered surface(s) made of glass microspheres."

Reply: Thank you. I changed accordingly.

Reviewer B: Figure 3: Note that even with the power law fit, the residuals are still relatively high at lower stresses (~1-5MPa) for siltstone (B), mudstone (C), and shale (D), and Darley Dale sandstone (A) does not include any data for normal stresses <10MPa.

Reply: I added the following statement in the caption of Figure 3: "*The increase in data variance at low normal stress may be caused by the lower accuracy of triaxial apparatuses at diminishing pressure.*" Although data variability is higher, the residuals and not systematic. The data on Darley Dale sandstone from Murrell (1965) is useful for covering almost two orders of magnitude, even though the smallest stress is greater than 10 MPa.

Reviewer B: Line 275: "Analyzing rock friction data in linear coordinates can be misleading, ….": And yet, I don't see any specific refutation in this paper of Byerlee's original interpretation that in the low normal stress regime that roughness is dominant.

Reply: The study does not criticize Byerlee's work. I do not refute his interpretation.

Reviewer B: Recommendation: Revisions Required

Reply: Thank you for your valuable comments that allowed me to improve the quality of the article.

Editorial Review

Thank you for the detailed response to the reviews, and thorough revision of the paper. I think it basically ready for acceptance, but I have a few minor comments that I would like you to consider. I consider this a very minor revision. Comment one is editorial and really about making sure the language is clear, and I'll accept if you decide not to make a change. The other three comments follow up on reviewer comments - I accept the points you make in your rebuttal, but it would be good to make similar clarifications in the paper itself, as readers may have the same questions as the reviewer did.

Minor editorial comments:

1) There is a bit of confusion (at least to me) introduced by the sign convention for beta and discussing beta in some places and the power-law exponent (negative beta) in others: Specifically, in the results, wet phyllosilicates are described as having a negative beta (e.g. Line 273 regarding the most negative values). On the other hand, the power-law exponent is described as positive for wet phyllosilicates in the discussion (line 375). This isn't wrong - of course a negative beta gives a positive exponent when the exponent is equal to negative beta. However, it might remove some confusion if the manuscript consistently talked about beta, or consistently about the power law exponent (assuming there is good reason that Eq 1 defines the exponent as negative beta).

2) I realise the paper is not meant to criticise Byerlee (1978), but it is consistently criticising linear models, and could be inadvertently seen (as it was by R2) as critical. I wonder if, for the sake of being explicit, a statement could be added to the conclusions - such as making an addition to Line 380, e.g. 'Cohesion, often invoked in linear models, is negligible at macroscopic scales, as already recognised by Byerlee (1978).' Or in some other way - what I am looking for is a mechanism to avoid the manuscript being accidentally seen as opposition to the original Byerlee model, but rather an improvement of that model. Your arguments are clear in the rebuttal, but the paper remains somewhat unclear, in places, on which studies you mean when referring to common inappropriate use of a linear model.

3) Further on the above - I agree with R2's comment on the practicality of a linear model in some instances. I wonder if a few words could be added, for example in the paragraph starting on line 351, on the point that there are circumstances where a linear model may be a reasonable simplification (before then specifying a situation where this is a problem and a better model is needed). In your rebuttal you point out that

"Any simplification of more thorough and complete model can be useful. The key consideration is to not confuse a physical model with its simplified approximation. Byerlee (1978) is extremely careful to describe the nonlinear dependence of the friction coefficient to normal stress, providing 3 equations that capture it precisely, including Equation (1) used in my paper. His piecewise linear approximation is useful for practical purposes."

I'd appreciate if this point could make it into the paper - it could also address my point 2, i.e. an admission that Byerlee's approximation is reasonable, and useful for practical purposes, but is a simplification of the more physical model that you propose.

4) Thank you for the inset in Figure 1. The way it is designed, it looks like 'sliding friction' and 'residual strength' refer to the same set of lines - please clarify.

Ake Fagereng

Dear Editor,

Please find my reply below. I made the editorial changes you suggested and I updated Figure 6 to increase the line thickness. I am not in full agreement with the other comments, so I have clarified my position in manner respectful of other views. Please let me know if you find these changes acceptable.

Best wishes,

Sylvain Barbot

Editorial comments:

Editor: In addition, please add your Zenodo repository link to the Data Availability statement (as you already realised).

Reply: I have updated the Data Availability statement accordingly. However, the zenodo repository still shows "in review" on the website. Could you please check if this is being reviewed and if it can be approved?

Furthermore, the supplementary file still mentions "*Non-peer reviewed manuscript submitted to Seismica*". I am sharing the Latex file so this can be updated accordingly by your copyeditors.

Editor: 1) There is a bit of confusion (at least to me) introduced by the sign convention for beta and discussing beta in some places and the power-law exponent (negative beta) in others: Specifically, in the results, wet phyllosilicates are described as having a negative beta (e.g. Line 273 regarding the most negative values). On the other hand, the power-law exponent is described as positive for wet phyllosilicates in the discussion (line 375). This isn't wrong - of course a negative beta gives a positive exponent when the exponent is equal to negative beta. However, it might remove some confusion if the manuscript consistently talked about beta, or consistently about the power law exponent (assuming there is good reason that Eq 1 defines the exponent as negative beta).

Reply: I agree. I changed the text accordingly, but on my file the correction was made at line 341. (Line 375 is in the References.) The updated sentence reads "*The power-law friction exponent β is positive for most non-phyllosilicate rocks and engineered surfaces, but is negative for water-saturated phyllosilicates.*"

Editor: 2) I realise the paper is not meant to criticise Byerlee (1978), but it is consistently criticising linear models, and could be inadvertently seen (as it was by R2) as critical. I wonder if, for the sake of being explicit, a statement could be added to the conclusions - such as making an addition to Line 380, e.g. 'Cohesion, often invoked in linear models, is negligible at macroscopic scales, as already recognised by Byerlee (1978).' Or in some other way - what I am looking for is a mechanism to avoid the manuscript being accidentally seen as opposition to the original Byerlee model, but rather an improvement of that model. Your arguments are clear in the rebuttal, but the paper remains somewhat unclear, in places, on which studies you mean when referring to common inappropriate use of a linear model.

Reply: The piecewise linear approximation of Byerlee (1978) is an ad hoc approximation of the data that offers limited insights about the underlying process. Any practitioner can approximate the frictional strength of rocks in various ways, using polynomials, Fourier series, or a Taylor series expansion. The merit of these approaches must be verified on a case-by-case basis. The key point contribution of the paper is to highlight consistent observations and theory about the frictional behavior of rocks, not to condone or approve various approximations used previously. Note that I was very careful to not use the equation for any other approximate model in the manuscript, so that only the correct representation of the data is mentioned. Although I understand your sentiment about avoiding unnecessary criticism of previous work, I feel that explicitly justifying Byerlee's (1978) piecewise approximation goes against the spirit of the work.

Editor: 3) Further on the above - I agree with R2's comment on the practicality of a linear model in some instances. I wonder if a few words could be added, for example in the paragraph starting on line 351, on the point that there are circumstances where a linear model may be a reasonable simplification (before then specifying a situation where this is a problem and a better model is needed). In your rebuttal you point out that

"Any simplification of more thorough and complete model can be useful. The key consideration is to not confuse a physical model with its simplified approximation. Byerlee (1978) is extremely careful to describe the nonlinear dependence of the friction coefficient to normal stress, providing 3 equations that capture it precisely, including Equation (1) used in my paper. His piecewise linear approximation is useful for practical purposes."

I'd appreciate if this point could make it into the paper - it could also address my point 2, i.e. an admission that Byerlee's approximation is reasonable, and useful for practical purposes, but is a simplification of the more physical model that you propose.

Reply: I added the following sentence: "*Local linear approximations may be acceptable in some circumstances, particularly at high effective normal stress, but the parameters involved do not directly reflect the physical properties of rocks, as they vary with the point of expansion or the range of applicability. The friction coefficient is a continuous function of effective normal stress.*" The data and theory discussed in this manuscript make clear the power-law dependence of the friction coefficient with effective normal stress. Although this behavior was readily observed in numerous studies by the late 1970s, it was missed in the original rate-state equations of Ruina (1983) and in the formulation of Linker & Dieterich (1982) and Sleep (2005) on the effect of normal stress on rock friction. These developments incorrectly convinced several generations of earthquake scientists that the friction coefficient is independent of normal stress. It is important for the earthquake science community to move away from linear approximations and to embrace the fundamental nature of rock friction to build a physically sound understanding of earthquake physics and to accelerate the rate of discovery. For these reasons, I prefer to focus the paper on the fundamental nature of rock friction and not comment further on the potential merit of various approximations.

Editor: 4) Thank you for the inset in Figure 1. The way it is designed, it looks like 'sliding friction' and 'residual strength' refer to the same set of lines - please clarify.

Reply: Thank you for pointing that out. I removed “Residual strength” from the figure, focusing the schematic on “Kinetic friction”.