Reviewer 1: Round 1

Review on the Paper: "Statistical distribution of static stress resolved onto geometrically-rough faults" by Jeremy Maurer

This paper is discussing how does a uniform background stress field does project on a rough fault and how the shear to normal ratio is changing due to the statistic of the rough fault.

First of all, I enjoyed reading this paper, and I think the idea to calculate statistic for stresses on a rough fault is very interesting. However, I have two major comments, that should not prevent the author from publishing, but that may take a bit of time to correct:

- For the derivation of the main equation of the paper (15), the author is using equation (11) and (12) as approximation of cos and sin, assuming that the slope of the fault is small. I may be wrong on this, but I think that if one wants to obtain a second order expression like (13) and (14), he must derive the expression with the second order solution of eq. (11) and (12). I did the derivation with mathematica and my expression is changing. The problem is that all subsequent equations are based on this one.

- For figure 4, the author is showing the resolved stress ratio due to his expression assuming that the slope of the fault is small. I get his data, and check if the slope was small. This is not the case, at least for the figure 4.c. This is why I encourage the author to make an exact calculation of the resolved stress ratio, and then compare to the approximate expression given by equation (15).

While there may be a mistake, I think the result would not change too much. This is why I recommend the publication of this article after the authors has corrected/responded for the above comments.

Best regards,

Pierre Romanet

Please see detail comments below:

General

- I personally would prefer to see to full solution (not approximate one like equation 15) in figures. The approximation is very useful to show the gaussian behaviour but is breaking down with increasing fault slope. You can eventually plot on top the approximation as given by equation 15.

Introduction

- In the introduction, I think you should emphasize that your work is for the resolved stress on rough fault from a uniform background stress field. I am saying that because a rough fault does not only perturb the loading part, but also the reaction of the fault due to slip, that will also change the stress ratio on the fault. One solution is to introduce a bit of mechanics inside the introduction like:), explaining that you consider the case where the fault has not slipped yet.

- Line 46: please correct the typo "heterogenous".

- For example, line 46-47, "Fault prestress becomes…", it does not become heterogeneous only because of the projection of the uniform loading, but also because the fault slips and perturbs the stresses. I am actually working on the elastic response of non-planar faults if you are interested: https://essopenarchive.org/doi/full/10.22541/essoar.169444103.38619526

You do not have to cite it, as it is still under review, but I think this is complementary to your work.

Shear over normal stress ratio on 2D planar faults

- It is just a comment, you do not have to change anything here: not that s and n are not independent, and that you can write equation 6 only in terms of the normal vector to the fault n.

<u>Resolved stress ratio on a 2D geometrically rough fault</u>

- I think you should mention that you are assuming that the fault slope is small.

- I think you need to go to second order approximation for equation (11) and (12):

- My expression for the normal traction when corrected is (please see attached mathematica file):

- You then need to correct all the following equations in the paper.

<u>Statistical description of fault roughness</u>

- Equation 18 was not obvious to me, can you add a reference, or just add the key words: “function of a random variable” so that it is easy to find.

- Personally, I did not understand equation 19. Can you provide more details? Are you doing something like a Taylor expansion assuming that the slope m is small?

- What is the parameter D?

- Line 198: typo “gaussian”

<u>Results</u>

- Line 233 From “From equation 15 … (Fig. 5a)”. I don’t understand what you meant here. Equation 15 is a rational function so not linear. Which equation in Dunham et al are you referring to?

Reaching frictional failure on geometrically rough fault

- I agree with the paragraph, but I think it would be more convincing if figure 4 was done with the full solution and not the approximated one. Especially because it seems that you are breaking the small slope approximation when plotting fig. 4. Can you comment on this?

Characteristic of geometric rough stress

- Line 302 “and references in Fang and Dunham (2013)”. Could you follow the references instead of referring to Fang and Dunham?

- I liked the discussion about the pore pressure change. This is very interesting.

Fault roughness and earthquake

- Line 328: typo “observation”

- I need again to be convinced that figure 4 gives the same results with the full solution and not the approximated one.

Figures:

- Figure 1: The fonts are too small. Is it possible to make them bigger?

- Figure 3: The fonts are too small. Is it possible to make them bigger?

- Figure 4: I reproduce this figure thanks to the authors providing the data. However this figure was generated assuming the slope of the fault is small, which is far from being the case at least for figure 4c. Would it be possible to do the full calculation with no approximation, and then compare with the approximation given by equation 15?

- Figure 6: The fonts are too small. Is it possible to make them bigger?

Reviewer 2: Round 1

This manuscript examines the distribution of resolved tractions (ratio of shear to normal traction) on rough faults in a 2-D medium under uniform remote stresses and without slip. The main contribution of this work is the derivation of an approximate analytic solution for the probability density function (pdf) of the fault traction ratio on rough self-similar faults. Overall, the revised paper addresses my previous comments, and can be published in *Seismica* after the following minor comments are addressed:

1. Line 47. What do you mean by "nucleate specifically due to geometric features"?
2. Lines 88-89. Shouldn't it be Figure 2a?
3. Caption of Fig 3. Typo: "athe".
4. Line 160. You cite Fang and Dunham (2013), but I don't see this expression for the standard deviation (eq. 17) in their paper.
5. Line 162. "…roughness wavelengths, respectively." => "…roughness wavenumbers, respectively."
6. Lines 164 - 173. I think that those lines should be removed. You describe this at the beginning of the result section. Why also here?
7. Fig 5. What is the value of alfa in (a)?
8. Caption of Fig 6. I don't understand what you mean by maximum possible friction.
9. Lines 356- 360. You write in one sentence that rough faults have a range of fault orientations with at least some segments at or above the peak stress ratio. In the following sentence, you write that this is consistent with the idea of a keystone fault, but these are not equivalent. As you write in the discussion, the keystone concept is consistent with the case where "A geometrically rough fault, however, may have significant portions of the fault that are well away from failure (for a fault near optimal orientation) that could pin the well-oriented segments of the fault until it reaches the failure threshold."

Response to Reviewer comments For "Statistical distribution of static stress resolved onto 3 geometrically-rough faults"

Jeremy Maurer

April 22, 2024

1 Reviewer #1

1.1 Major Comments

1.1.1 Comment 1

For the derivation of the main equation of the paper (15), the author is using equation (11) and (12) as approximation of cos and sin, assuming that the slope of the fault is small. I may be wrong on this, but I think that if one wants to obtain a second order expression like (13) and (14), he must derive the expression with the second order solution of eq. (11) and (12). I did the derivation with mathematica and my expression is changing. The problem is that all subsequent equations are based on this one.

1.1.2 Response

I thank the reviewer for the comment. I think the reviewer may have slightly mixed up the part of the paper where the first- and second-order approximations were made. I will attempt to clarify, and please let me know if this does not make sense.

Eq.s 11 and 12 in the main text do not use the approximation for cos and sin; however, it is easy to see how this could be thought. Becuase the argument to both the sin and cos in this case is the arctangent, there is an exact analytic solution which is the result given in the paper. That this solution is exact for all slopes m can be seen by taking the two end-member cases, $m = 0$ and $m \to \infty$. By Eq. 11 in the main text, the unit normal for $m = 0$ is $[0, 1]^T$, as expected. Taking the limit as $m \to \infty$, we get $[-1, 0]^T$, as expected. The intermediate case $m = 1$ (a 45° angle), is also correct: $[-1/\sqrt{2}, 1/\sqrt{2}]^T$. Checking Eq. 12 for the same three cases gives $[1, 0]^T$ for $m = 0$ (ignoring the sign function), $[1/\sqrt{2}, 1/\sqrt{2}]^T$ for $m = 1$, and $[0, 1]^T$ for $m \to \infty$, and checking shows that that $\mathbf{n} \cdot \mathbf{s} = 0$ for each case. This shows that all slopes, not just small slopes, are appropriately handled by Eq.'s 11 and 12. Eq. 15 in the main text, which is the ratio of the shear to normal stress, has been derived without any approximations, and is in fact an exact solution to the stress ratio for a 2D fault. There is an approximation applied at for the derivation of the statistical distribution (Eq.'s 20-26 in the main text); the paper shows both first- and second-order approximations, which do use the small-slope assumption. The derivation of the stress ratio, however, does not use any assumptions about slope.

One final note is that, based on the attached Mathematica notebook, the reviewer presents the following equations as being the full solution for the unit normal vector:

$$n_1 = \frac{-m}{\sqrt{((1 - m^2)^2 + m^2)}} \tag{1}$$

$$n_2 = \frac{1 - m^2}{\sqrt{((1 - m^2)^2 + m^2)}} \tag{2}$$

However, taking a simple case of $m = 1$ (slope is 45°), using Eq. 1 we obtain $\mathbf{n} = [-1, 0]^T$, which is not the correct unit normal vector for a slope of 1 (should be $[-1/\sqrt{2}, 1/\sqrt{2}]^T$). If I've missed anything or am misunderstanding the reviewer comment, please let me know.

1.1.3 Comment 2

For figure 4, the author is showing the resolved stress ratio due to his expression assuming that the slope of the fault is small. I get his data, and check if the slope was small. This is not the case, at least for the figure 4.c. This is why I encourage the author to make an exact calculation of the resolved stress ratio, and then compare to the approximate expression given by equation (15).

1.1.4 Response

As noted in the above response, Figure 4 is not an approximation of the resolved stress and is in fact the exact calculation. The analytic solution has also been compared against numerically-computed shear and normal stress and found to be the same, even for large slopes.

1.2 Minor Comments

I have skipped the minor comments related to the approximation question; see response to Major comment 1.

Other comments:

2. In the introduction, I think you should emphasize that your work is for the resolved stress on rough fault from a uniform background stress field. I am saying that because a rough faultdoes not only perturb the loading part, but also the reaction of the fault due to slip, that will also change the stress ratio on the fault. One solution is to introduce a bit of mechanics inside the introduction like explaining that you consider the case where the fault has not slipped yet.

I thank the reviewer for the comment. I have modified the introduction as follows:

In this paper, I develop expressions for the statistical distribution of stress by 1) assuming a statistical distribution for fault roughness, that of a fractal geometry, 2) resolving a uniform pre-stress background field onto such fractal faults, and then 3) propagating an approximate form of the stress ratio through the transformation from the distribution for fault geometry to that for the stress ratio. This analysis assumes no slip occurring on the fault (does not account for stress changes due to slip). Doing this allows me to derive the probability distribution function (PDF) of resolved shear-over-normal stress on the fault prior to slip, which can be compared to those used in previous studies.

3. Line 46: please correct the typo "heterogenous".

Done!

4. For example, line 46-47, "Fault prestress becomes…", it does not become heterogeneous only because of the projection of the uniform loading, but also because the fault slips and perturbs the stresses.

Correct. The fault branch impacts slip in a systematic way so that the prestress becomes heterogeneous in the vicinity of the branch. I have modified the text to read:

Fault prestress becomes heterogeneous over multiple earthquake cycles near branches in earthquake simulations...

5. It is just a comment, you do not have to change anything here: not that s and n are not independent, and that you can write equation 6 only in terms of the normal vector to the fault n.

Correct, thank you to the reviewer for the comment.

7. Equation 18 was not obvious to me, can you add a reference, or just add the key words:"function of a random variable" so that it is easy to find.

I thank the reviewer for the comment. I skipped a few steps initially, so I have added those manuscript.

The fact that fault slope is normally distributed can be exploited to determine the statistical distribution of fault stress using Equation 15. To do this, we note that for a random variable m with probability density function $\Psi(m)$, and a monotonic and one-to-one transformation $R^l = g(m)$ between m and R^l, the inverse transformation $g^{-1}(R^l)$ can be used to derive the distribution of the random variable R^l, $\varphi(R^l)$. The general expression is:

$$\varphi(R^l) = \Psi\left(g^{-1}(R^l)\right)\left|\frac{dg^{-1}(R^l)}{dR^l}\right| \tag{3}$$

Substituting a Guassian distribution with variance σ_m^2 for $\Psi(m)$ gives:

$$\varphi(R^l) = \mathcal{N}\left(g^{-1}(R^l), \sigma_m^2\right) \left| \frac{dg^{-1}(R^l)}{dR^l} \right| \tag{4}$$

8. Personally, I did not understand equation 19. Can you provide more details? Are you doing something like a Taylor expansion assuming that the slope m is small?

This is a polynomial expansion of Eq. 15. Taking only one or two terms as I do in the following section of the manuscript does imply a small-m approximation. I have expanded the discussion to try to clarify:

Approximations of various orders can be derived using a polynomial expansion of Equation 15:

$$\begin{aligned} \operatorname{sgn}(\Psi)\frac{\left(1-m^2\right)\sin(2\Psi)-2m\cos 2\Psi}{(1+m^2)\gamma^{-1}-2m\sin(2\Psi)-(1-m^2)\cos 2\Psi} &= \operatorname{sgn}(\Psi)\sum_{k=0}^{\infty} a_k m^k \\ \rightarrow \frac{\sin(2\Psi)-2\cos 2\Psi m-\sin(2\Psi)m^2}{(\gamma^{-1}-\cos 2\Psi)-2\sin(2\Psi)m+(\gamma^{-1}+\cos 2\Psi)m^2} &= \sum_{k=0}^{\infty} a_k m^k \\ \rightarrow \frac{A-2Bm-Am^2}{C-2Am+Dm^2} &= \sum_{k=0}^{\infty} a_k m^k \end{aligned} \tag{5}$$

which, after expanding out and collecting terms proportional to powers of m, gives:

$$A-2Bm-Am^2 = a_0C+(-2a_0A+a_1C)m+\sum_{k=2}^{\infty}(a_{k-2}D-2a_{k-1}A+a_kC)m^k \tag{6}$$

for $A = \sin 2\Psi$, $B = \cos 2\Psi$, $C = \gamma^{-1} - \cos 2\Psi$, and $D = \gamma^{-1} + \cos 2\Psi$, and coefficients a_k to be determined.

9. What is the parameter D?

Added definition to the manuscript; see answer to minor comment 8 above.

10. Line 198: typo "gaussian"

Corrected!

11. Line 233 From "From equation 15 ... (Fig. 5a)". I don't understand what you meant here. Equation 15 is a rational function so not linear. Which equation in Dunham et al are you referring to?

I thank the reviewer for the comment. The text is not worded well; what it should say is that because Eq. 15 is a polynomial function of m, the resolved stress can be written as a lienar combination of powers of m (which is in fact how the approximation of Eq.'s 22 and following are derived). I have amended the text to clarify:

From Equation 15, resolved static stress is a polynomial function of slope m, and so can be written as a linear combination of powers of m. This implies that the spectrum is also a linear function of the transform variable $\hat{m}$

12. Line 302 "and references in Fang and Dunham (2013)". Could you follow the references instead of referring to Fang and Dunham?

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13. Line 328: typo "observation"

Corrected!

14. Figures 1, 3, 6 – fonts too small

Corrected!***

2 Reviewer #2

2.1 Minor Comments

1. Line 47. What do you mean by ”nucleate specifically due to geometric features”?

This is not worded well. I changed this to say:

...due to stress concentrations resulting from geometric features...

2. Lines 88-89. Shouldn’t it be Figure 2a?

yes, corrected.

3. Caption of Fig 3. Typo: “athe”.

Corrected.

4. Line 160. You cite Fang and Dunham (2013), but I don’t see this expression for the standard deviation (eq. 17) in their paper.

I thank the reviwer for the comment. The expression is their equations 1 and 2, but combining them and working out the integral. However, the actual derivation is from Dunham et al., 2011b, so I have added that reference to the manuscript.

5. Line 162. “. . . roughness wavelengths, respectively.” =¿ “. . . roughness wave numbers, respectively.”

Thanks, corrected.

6. Lines 164 - 173. I think that those lines should be removed. You describe this at the beginning of the result section. Why also here?

Thanks, done.

7. Fig 5. What is the value of alfa in (a)?

0.005, added to caption

8. Caption of Fig 6. I don’t understand what you mean by maximum possible friction.

This is not good wording, thanks to the reviewer for pointing this out. I have amended the text to read:

The change in the shape of the distribution reflects the fact that the maximum possible stress ratio and the highest resolved stress ratio both increase faster than the background.

9. Lines 356- 360. You write in one sentence that rough faults have a range of fault orientations with at least some segments at or above the peak stress ratio. In the following sentence, you write that this is consistent with the idea of a keystone fault, but these are not equivalent. As you write in the discussion, the keystone concept is consistent with the case where “A geometrically rough fault, however, may have significant portions of the fault that are well away from failure (for a fault near optimal orientation) that could pin the well-oriented segments of the fault until it reaches the failure threshold.”

Thanks to the reviewer for the comment. The sentence in question is not worded well and I have revised it.

The implication is that for any given background stress field, there may be a range of fault orientations that have at least some segments near the failure stress, depending on the roughness level of the faults. There may also be optimally-oriented faults that have segments well away from failure. This is consistent with the idea of a keystone fault, where a fault may be well oriented for failure with respect to the background stress field, but have some segments which pin the fault and prevent it from slipping until the stress reaches a high enough level to allow ruptures to propagate through those misoriented segments.

Reviewer 1: Round 2

Dear editor,

I have read through the response to reviewer and the manuscript. The mistake was coming from me, as I did not realise that it is possible to write exact expression of sin(arctan(m)) and cos(arctan(m)). I am sorry for the confusion I made.

I think the author did a good revision of his paper, and hence it should be published promptly.

Best regards,

Pierre Romanet