Reply to the comments

Cristiano Collettini (Editor)

cristiano.collettini@uniroma1.it Received and published: 8 May 2019

Dear Authors,

I have now received two reviews for your manuscript. While both reviewers think that your manuscript scientifically sounds, they also raise some concerns that should be addressed during the revision round. On the grounds of the reviewers' comments I suggest you to improve the structure of the manuscript by: a) providing a more detailed description of method and figures; and b) better supporting the conclusions in order to avoid the criticisms of overstated conclusions. I look forward seeing the revised version of your manuscript. Sincerely Cristiano Collettini

Dear Editor, we have made significant changes both in the text and the figures (Figures 1, 3, 4 and 7) to improve the method and understanding of the figures. To avoid overstatement in the conclusion, we have removed some text and irrelevant terms (e.g. proxy, probability, hazard assessment) and better supported some conclusions with additional explanation about fault displacement. These changes are tracked in yellow in the revised manuscript and are detailed and explained below, in the reply of the 2 reviewers.

Anonymous Referee #1

Received and published: 27 April 2019

The paper "Fault slip envelope: A new parametric investigation tool for fault slip based on geomechanics and 3D fault geometry" by Roger Soliva, Frantz Maerten, Laurent Maerten and Jussi Mattila, deals with the reactivation of complex fault systems as a function of a wide range of possible geological conditions and mechanical properties.

This topic is very interesting and the results potentially very useful for the scientific community, however most of the results (figures) are not well explained and some improvements are needed to make the paper more clear. The structure of the paper is sometimes circular, and figures are recalled several times along the text making the reading not easy.

We made significant changes in the figures and the text of explanations of these figures (see the specific reply below). However, we do not understand why the paper appears "circular", and recalling the figures several times along the text is very conventional and necessary in many articles and reports to allow discussion of the results or the method presented before.

My concerns regard two main aspects:

Definition of the method. Method and presents results are often defined very briefly. As examples: Figure 5 is explained in 1 line (line 252), while the reasons why the authors chose the parameters (stars) of Figs 4 and 7 are not or very briefly explained along the text.

In the new version, we provide more explanation in the text about Figure 5 in lines 271-277. We mention that although the range of friction, cohesion and angle of $\sigma 1$ with respect to the main fault trend (around 30°) is possible, the parametric conditions must be considered as non-realistic since the stress loading is purely uniaxial. We also explain that these conditions imply very little stress perturbation in orientation since this depends mainly on the Lode angle and fluid pressure (Kattenhorn et al., 2000 and Maerten et al, 2018). These two new references have been added to the reference list in lines 472-473 and 494-495.

This parametric modelling made with uniaxial loading actually allows better understanding and validating the dependence of fault slip to the stress angle. Deterministic analysis with realistic stress conditions is presented in Figure 6 and 7. These two last points are mentioned in the new text of the method section in lines 108-110 for better understanding of the method used and figures presented.

General clarity of the figures. Most of the figures are not clear and absolutely not well explained along the text. For example, colours in figs 3 a, b and C have apparently no meanings, Figure 6 B is never mentioned along the text, and Fig 7 has too many different scales to be easily readable.

Colours in (a, b and c) correspond to different fault surfaces and allow an individual fault to be identified. This is now explained in the caption text of the figure in line 593. Note that the colour in Figure 3b has been revised since a same colour was used for all the faults in the previous figure.

We forgot to refer to Figure 6b in the method section 2.2 about the Olkiluoto study. This is now corrected in line 156.

The colour bar scales for displacement in Figure 7 and also Figure 4 have been modified. In both cases, the variation of displacement is very large from one model to the other (e.g. Dmax varying over 3 orders of magnitude in Figure 7). We therefore gave a scale bar specific to each model result for simplicity. We understand that it would be easier for a reader to have a same scale for all model results to allow an easy comparison. We therefore modify both figures by adding a single colour bar varying along a logarithmic scale. We believe worth to mention that the bar scale is logarithmic, both in the main text and the figure caption for the two figures (see lines 263, 294, 600 and 615).

My general feeling is that the figures show much more respect to what is explained along the text. I strongly suggest clarifying this point by better matching what is shown in the figures and what is written along the text. This can be reached by both simplifying the figures and improve the explanations along the text depending on what the authors want to highlight more.

As explained above, Figures have been simplified and more explanation is given, especially for figure 4, 5 and 7 (lines 258-270, 271-277 and 293-297). For example, we better describe and define the source of fault displacement asymmetry observed in Figure 4. For this purpose, a new reference has also been added in lines 266-267 and lines 549-551.

Punctual comments: See the annotated pdf file. Hope this helps

Please also note the supplement to this comment: https://www.solid-earth-discuss.net/se-2019-61/se-2019-61-RC1-supplement.pdf

We have revised the manuscript following all the comments annotated in the pdf:

- Addition of the reference suggested in lines 66-67 and 443-444,
- Addition of information for how in-situ stress measurement were acquired (lines 175-176),
- Significant modifications of Figure 1 to get it easier to read, especially for fault dip.
- Origin of the real fault systems in Figure 3: the paper and data source of the 3 real fault system geometries were previously mentioned in the method section (previous lines 131-134), and this is now recalled and improved in the new version in lines 134-139 in the result section where the comment has been made in the pdf,
- Addition of explanations for Figure 5 (see the reply above),
- Addition of explanations for the choice of the model results shown in Figure 7 (lines 296-297),
- The best conditions for fault slip derived from the models seem to be the present day stress state, which does not mean that the fault must move in the present day. So we don't clearly see disagreement in the previous text in lines 293-300. To clarify this point we modified the first sentence of the paragraph in line 329. The following text in this paragraph explains why, even in this thrust fault Andersonian context, the faults are not prone to slip.
- A new sentence has been added in the discussion to clarify that the ranges of mechanical properties might evolve through time and space (lines 345-347).
- Additional text and precision have been added to better explain Figure 1 (lines 221-224),
- The colour bar has been normalized and the position of the stars has been explained (see reply above).

Anonymous Referee #2

Received and published: 6 May 2019

General Comments

"Fault slip envelope: A new parametric investigation tool for fault slip based on geomechanics and 3D fault geometry" presents a novel approach to explore parameter space and determine conditions under which a fault or fault system of arbitrary complexity is stable or prone to slip. The method assumes the fault system geometry to be known but accounts for large uncertainties in parameters such as rock properties and sub-surface stress state. The authors present a novel method for visualizing the space in which the fault system is stable or not. The underlying scientific foundation is sound, as is the application to the Olkiluoto fault system; however, it is my opinion that the conclusions are overstated, in addition to several opportunities to improve the clarity and focus of the manuscript.

Specific Comments

The first part of the conclusions is split into four bullets.

1) The first describes a calculation of "Fault probability to slip. . ." In fact, the method of the paper presents nothing probabilistic. The slip envelope is a binary function whether or not a Coulomb yield surface is exceeded for a given combination of parameters. To describe this method as a probabilistic assessment is an overstatement.

We agree that the term "*probability*" is not relevant. It has been replaced by "*ability*" in lines 24, 128, 288 and 367, and removed in several places.

2) I do not take issue with the second bullet

3) The final sentence of the third bullet states, "This is particularly useful to address uncertainties in input data for hazard assessment." However, the manuscript only peripherally discusses conventional seismic hazard assessment. To justify this as a key component of the conclusions, further discussion is needed in the manuscript of how this method might be used for hazard assessment.

The key component of this bullet point is mentioned in the previous sentence: "*first time to propose "fault slip envelopes" which quantify fault system strength magnitude and anisotropy*". Rather than developing a hazard assessment section in the paper, which is not the purpose, we prefer to revise the sentence by replacing "*is particularly useful*" by "can be useful"(line 27 and 385), which tempers the implications of our results for seismic hazard.

4) The fourth bullet ("we also propose a proxy. . .") is not clear to me. Yes, the method enables solution of displacement discontinuity for each element of the fault representation, but what is this a proxy for? Overall, the discussion of slip calculations in the manuscript is insufficient (or unnecessary).

To precise exactly what it is done, we replace "*propose a proxy*" by "*calculate fault displacement*" (line 387). The way fault displacement is calculated is briefly explained in lines 123-127 (also see lines 102-106) in the method text (section 2.1) and referred to the source paper of the 3D model used in line 124 (Thomas, 1993; Maerten et al., 2010; 2014; 2018) for full explanation. Since fault slip is more important than displacement in this paper, we prefer not to add more details about its calculation to balance the method text, but also because the explanations given refer to the use of the linear elasticity theory applied to 3D dislocation model, which is now quite conventional. Please tell us if you think that more details are needed.

The second paragraph of the conclusions is speculative and seems more suited to the introduction than to the conclusions.

We do not see clearly why this text is speculative since the reviewer does not explain why. This text is an extended discussion of the applications of the new tool, which opens to general and concluding remarks, based on the results presented in the paper and other works (see references therein). This especially concerns the main following finding that we must highlight and that is based on our results: fault system strength "results lower for complex fault geometry rather than simple one for equivalent frictional and remote stress condition". To support the fact that this text is mainly based on our results, we have added some references to our figures in the new version of the manuscript (lines 395, 396, and 399).

Another option would be to move this text to the end of the discussion, but we actually prefer such a type of opening text in the conclusion, which allow a conclusion not only summarizing the paper results, and then not redundant with abstracts. Please let us know if you think that we should remove this text.

The following comments are in no particular order beyond following (roughly) the progression of the manuscript.

The authors present their method as a new and improved alternative to the conventional slip tendency analysis. It is my opinion that this is something of an unfair comparison. If used appropriately, the two methods are based upon different assumptions and should be used for different purposes. Slip tendency analysis (e.g. Morris, et al. 1996) assumes no knowledge of fault orientation but assumes something about rock properties, whereas the method presented in this manuscript assumes fault geometry is precisely described but rock properties are uncertain. This comparison (paragraph beginning line 64 and first sentence of Conclusions, line 342) should be better explained or perhaps revised to discuss how the two methods might compliment each other.

A previous comment of an earlier version of this manuscript (submitted to Geology, GSA) suggested to introduce the work with respect to the slip tendency analysis tool and precisely clarify what is new in the presented tool. We actually found worth to better develop this point, although as mentioned here, these two methods are quite different. We therefore explain the limits of the slip tendency tool (Lines 68-75) that the tool presented can meet. The value of the slip tendency analysis tool is exposed in lines 64-67 and as suggested by the reviewer above, we revised the first sentence of the conclusion to clarify that the two methods are complementary. We however do not agree with the reviewer comment here about the differences mentioned above, since slip tendency assumes that we know fault orientation (otherwise the resolved shear stress would be impossible to calculate) and do not assume knowledge on rock properties (it is only the resolved stresses) (see Morris et al., 1996). The improvements of our method (suggested in the introduction in lines 70-72) are summarized in the revised 4 bullets of the conclusion and exposed briefly/differently here:
1 – Fault slip is calculated using frictional behaviour (not only a stress projection referred to as “slip tendency”),
2 – The tool allows to run thousands of forward simulations in very short time, and therefore to provide full parametric mechanical study,
3 – This tool allows to quantify fault system strength and especially its anisotropy,
4 – Quasi-static fault displacement can be computed for each parametric condition considered.

Discussion of the three real fault geometries to which the three real fault systems to which the tool is applied prior to the actual field case could be explained further.

1) In the Landers case, the referenced study is 2D. Presumably the fault geometry used in this 3D study is a vertical downward projection, in which case this should be stated. Are similar assumptions about the down-dip projection made for either of the other two examples? In all of these cases there is likely substantial uncertainty regarding subsurface 3D fault geometry that is not accounted for in this study.

These three geometries were used in earlier studies cited in lines 136. The Landers case was used in Lovely et al. (2009, cited in the previous text) and also Madden et al. (2013, now cited in line 136 and 489-491). The Landers geometry is a 3D surface extruded downward from a 2D Earth's surface trace. The Oseberg Syd geometry is derived from high quality seismic reflexion survey. Uncertainty about 3D fault geometry discretization is not accounted for in this study but is discussed as a limitation in section 4, lines 348-351. All this is now clarified in lines 135-139.

2) The plots of slip tendency for each of these geometric cases is interesting, but some constraint on far-field stress magnitude and orientation should be available in each of these cases. Of course there is uncertainty, but the results might be more geologically insightful if supplemented with some insights regarding what portion of the slip envelope is most geologically plausible.

The aim of this section of the paper and this Figure is not to provide a deterministic study, or geologically plausible case (see the Olkiluoto case for this), but to illustrate fault slip envelope and anisotropy strength as a function of fault system complexity using realistic fault geometry (i.e. an improvement of what is shown in Figure 2 with synthetic faults). A deterministic approach, to be presented in such work, requires more geological constraints, especially on triaxial stresses (not uniaxial like here) and rock properties, which are not available for these areas. Instead, we believe that these 3 fault system geometries illustrate in a relevant way some typical fault system geometry found in the Earth's crust. This is now summarized in lines 141-143.

3) What is the significance of the 5 points (A-E) selected for plotting displacement discontinuity (I believe "displacement" throughout the manuscript should be displacement discontinuity or slip)? Are they end-member examples? Is there further meaning?

We refer here to the four stars (B-E) from Figure 4, selected for plotting quasi-static "fault displacement" (which is the common term, whereas "displacement discontinuity" is reserved to fault surface in dislocation model and "slip" is unclear and generally mentioned in case where displacement is not known – i.e. slip tendency -). The term "displacement" has been precised in line 259. Computed quasi-static fault displacement distribution is shown (bleu stars) for end-member conditions of friction, cohesion and stress orientation with respect to the position of the fault slip envelope. This is now mentioned in lines 261-262 for Figure 4 and lines 296-297 for Figure 7.

4) The paragraph beginning line 237 refers to specific stress orientations. Presumably this is related to a specific fault model (Landers I think). This needs to be clarified.

As mentioned in the line 237 of the previous version, the text refers to "both synthetic and real fault system geometries" (Figures 1, 2 and 3). This is still present in the new manuscript in line 249 and we don't see how to clarify it better.

In the paragraph beginning line 246, the authors describe the slip distributions shown in Figure 4. Beyond being a product of the BEM model, the relevance of this paragraph (and Figures 4 & 5) is not clear to me. I think the authors intend to draw a conclusion out of the slip distributions (and stress perturbations in Figure 5), in which case additional explanation is required. Otherwise, perhaps the material could be omitted. The most extensive discussion of the slip distributions is their limitations, given that better distributions can be obtained from dynamic rupture simulations (Lines 332-338). The same comment applies to Figure 7 and limited discussion thereof.

The plot of displacement distribution is a way to analyse in which place fault is prone to slip with respect to different parametric conditions. This was not explained clearly an is now mentioned in lines 258-259. The sentence relative to this, now in lines 263-264, has been rephrased for clarity. Additional information about displacement (and stress distribution) is now presented in lines 266-277 (this text also answers to the comment of reviewer 1 above). Similar text for Figure 7, which is worth to understand why and where faults are prone to slip, was mentioned in lines 265-275 of the previous text and are slightly improved in lines 293-301 of the new version.

In lines 258-261, the authors describe other surfaces (pink in figure 6c) that are not slip envelopes

1) The text indicates similar surfaces were shown for the Landers model in Figures 4 & 5. I do not see any such surfaces in any figures relating to Landers, and neither were any surfaces of this sort discussed previously.

The text was not referring to these surfaces but to the displacement distribution. This is now clarified in lines 284-285 by removing the non useful and potentially confusing part of this sentence.

2) It is not clear to me from the two sentences discussion that follow what these surfaces add to the analysis or to the manuscript. Further discussion is needed or perhaps the surfaces could be omitted.

It was not obvious that the "displacement envelopes" (iso values of maximum displacement in the slip domain) mimic the shape of the fault slip envelope, especially because fault displacement is calculated as a function of fault interaction through their stress field (mentioned in line 125). This is a quite interesting result, which reveals the lesser influence of fault interaction compare to friction, cohesion and stress state considered in this study. The two last sentences now better explain why these envelopes are useful in lines 287-293.

For the Olkiluoto fault system case study, the authors make no mention of the orientation of SH or the difference between SH and Sh. The former was a critical variable in the synthetic studies, controlling the sinuous shape of the slip envelopes, and the latter was not discussed but would likely also be important. What are the values? How are they constrained? What are the implications of their uncertainty? These topics could be addressed in Section 3.2 and/or Section 4 and would contribute to the geologic value of the case study.

We missed to mention the E-W orientation of σ_H, which is now indicated in line 164. The values are presented in Figure 6a and the measurement method is fully detailed and discussed in the open access Posiva Report cited as Ask (2011), and briefly mentioned in lines 175-176. The largest part of uncertainty of these measurements presented in this report and mentioned in lines 335-338, is much below the expected stress variation due to the presence of an ice sheet. This was yet mentioned in the previous version of the manuscript and is still present in the discussion in lines 352-354.

Typographic corrections and lesser comments

Line 27: an = and ("and mechanical properties.")

Corrected

Line 63: The authors suggest “the strength of potentially large rock volumes. . . has never been clearly studied.” This is a very strong statement that could be easily contested. Perhaps it would be more appropriate to say there is an opportunity for advancement in this space?

“has never been clearly studied” has been replaced by “is still a challenge to quantify”.

Line 80: Use of “admitted” is ambiguous and confusing. Do you mean that Mohr Coulomb’s theory is well accepted or that its limitations are acknowledged?

“admitted” has been replaced by “accepted”.

Line 132- 133: Order the references such that there is a clear correlation between the three field sites and the three references.

This was yet the case.

Line 176: materiel = matierial

Corrected

Line 176-179: The sentence leading up to eq. 5 is confusing. I expect a sentence that calculates the tectonic constants.

We do not catch the issue here. In this sentence we explain how the tectonics constants were calculated (a difference). Please rephrase if you find a serious issue, we would be glad to improve the text.

Line 186: I think the reference to Figure 3a is incorrect as the text does not relate to the Landers event. It is not clear to me what figure should be referenced here. Paragraph beginning at line 186: may be more appropriate in the results, and perhaps should be illustrated in a figure.

Yes, “Figure 3a” has been replaced by “Figure 6a”.

Line 266 (and elsewhere): the authors refer to “slickenlines” in reference to model results. Slickenlines are technically a specific geologic feature from which slip orientation may be inferred. The lines in figure 7 are not slickenlines but a representation of the orientation of modeled fault slip.

The text now mentions that the faults contain “streamlines representing the orientation of fault slip, referred to as slickenlines” in line 294.

Line 311: Do the authors mean “The planar and vertical shape of the fault slip envelope...”? “... little dependence of fault strength on the vertical load...” implies that the slip envelope is not just planar but also vertical.

Yes, this has been added in line 342.

Line 319: The authors relate faults smaller than 100m length to displacement [discontinuity] less than 10-2m. Based on the ratio I assume 10-2m would be the seismic slip, but the authors should specify if this relation applies to seismic or geologic (cumulative) slip.

This ratio concerns the incremental displacement, not the cumulative. This is now mentioned in line 352.

Line 335: chapitre = chapter

Corrected

The authors would like to thank both reviewers and the editor for these constructive comments. We hope that the revision improves the quality of the manuscript.

Roger Soliva and Frantz Maerten