Reviewer #1

The manuscript presents a thorough study on the relationships between the distribution of fault sizes and of earthquake rupture sizes. This is used for understanding fault activation processes within the brittle crust.

The work is rigorous and precise, especially in the statistical treatment of the data.

My main concern lies on how fault segmentation was dealt during the mapping of faults at multiple scales. Faults can be formed by multiple close segments, hence what can appear as a single fault at 1:1000 scale may appear as multiple smaller segments at 1:100 scale. I believe it is important to clarify this passage as this could bias the results.

I have attached a pdf with other comments on the paper.

Kind regards

Francesco Iezzi

Thank you very much for your thorough review and the positive feedback on our manuscript. We greatly appreciate the time and effort you invested in providing such detailed and constructive comments.

We have carefully addressed all your comments provided in the PDF and incorporated the corresponding changes directly into the revised version of the manuscript. Inter alia, we have clearly defined the difference in tectonic origin between thrusts (thin-skinned tectonics) and faults (thick-skinned tectonics) and that we only consider the latter. Furthermore, we more explicitly defined the term "size distribution", as the word "size" refers here to the length of a fracture or the diameter of a circular earthquake rupture, respectively. We decided to consistently use "size distribution" throughout the manuscript for clarity.

Regarding your main concern about how fault segmentation was accounted for, we have taken this opportunity to elaborate further, as it aligns with some of the detailed points raised in the PDF, especially concerning the implications of a power law distribution.
We want to emphasize again the scale invariance of power laws, meaning that features governed by power laws lack a characteristic length. The fact that faults in our dataset follow a power law indicates that a fault does not possess a unique length; any measured length is inherently scale dependent. For instance, a fault mapped on a low-resolution satellite image might have a length measured in kilometers, while the same fault measured in the field could yield hundreds of meters due to observable segmentation. On a hand specimen or thin section, this length could further reduce to centimeters or smaller, again due to segmentation at smaller scales. Given this scale dependence, segmentation becomes irrelevant when employing a multi-scale approach, as we did in our study, where we focus on statistical metrics such as the power law exponent rather than individual fault lengths to capture the properties (i.e., size distribution parameters) of the fracture networks. To clarify

this critical aspect, we have revised the relevant sections in the introduction and methods sections.

We hope that these revisions address your concerns and improve the clarity of our study. Once again, thank you for your valuable feedback and constructive suggestions.

Kind regards,
On behalf of all co-authors,
Sandro Truttmann

Reviewer #2

Review of a manuscript by S. Truttmann et al. (2024), Ms No.: egusphere-2024-2975,

"The Size Distributions of Faults and Earthquakes: Implications for Orogen-Internal Seismogenic Deformation"

General comments

The manuscript by Sandro Truttmann et al. examines the relationship between fault size distributions and earthquake rupture patterns within the western Central Alps of Switzerland. They combined mapping of pre-existing discontinuities across multiple spatial scales (from outcrop to map scale; 1:10 to 1:1000) from UAV-based photogrammetry (for the outcrop scale) as well orthophotos and DEM'S available from Swisstopo (for the map scale) with earthquake data from the Swiss seismological network. After a statistical analysis of the fracture network (focusing on the fractal dimensions of the fracture network), and the earthquakes, they explored the statistical link between these independently obtained datasets and their implications for seismogenic deformation in the study area (Central Swiss Alps, characterised by pronounced post-collisional shortening, numerous highly strained tectonic nappes involving both Palaeozoic crystalline basement and Mesozoic-Paleogene sedimentary successions, and very moderate present-day seismicity and rather negligible horizontal motions as suggested by GNSS).

Their findings suggest depth-dependent differences in fault reactivation, highlighting the reduced likelihood of complete fault ruptures at shallow depths due to lower differential stresses. These insights are relevant for assessing the likelihood of induced seismicity in future geothermal energy exploration in the frontal Swiss Alps.

The paper is already very well structured and well written. Kudos to the authors for clearly elaborating their approach and methods with great detail, e.g., explain what kind of advantages the choice of a circular counting window for extracting fracture orientations and lengths has over an e.g., square counting window. The statistical analyses of the fracture network with the derivation of fractal dimensions using power-law distributions also appears very robust and reveal a large degree of details.

Overall, the study is very original and deserves publication pending minor revisions.

I have, however, a very minor comment pertaining to the nomenclature of a pivotal term in their study: the authors persistently call the discontinuities in the database they derived from the multiscale mapping approach "faults" rather than, more neutrally "fractures" (which would include all extensional discontinuities (joints, veins)).

First of all, we would like to thank you for your valuable feedback and detailed comments on our manuscript. Your feedback has helped us to further improve and clarify our manuscript.

Regarding the main concern raised about the nomenclature of "faults" for the mapped discontinuities. Although we clearly stated in the first version of the manuscript on line 147 ff. that we do not distinguish between fracture types, we agree that the use of the term "faults" for the mapped geological discontinuities is not strictly correct. To avoid potential misunderstanding by the reader (i.e., a structural geologist who might directly associate the term "fault" with a shear fracture), we have decided to accept your suggestion and use the term "fracture" throughout the manuscript, in the figures, and in the title.

Specific comments

Specific comments on the main text

line 15 ff: “we find that…”: I find this statement unclear. Which parameters around 3D fault networks show a power law behaviour? Pls clarify.
Line 15: We agree that this sentence could be phrased better and therefore wrote explicitly that it is the size distribution that follows a power law.

line 79: “(…) enhanced (…)”: My concern is a bit that “enhanced” might be misunderstood in terms that the events were induced, which is not what you mean to say, I guess (?).

I suppose you attempt saying that activity (i.e., frequency of events in a given timespan) is “above average” in the study area. But perhaps it’d be best to simply omit the word.

I think it’d be more important to elaborate whether all the events (with M as small as 0-1) are natural earthquakes, with e.g., quarry blasts or mining-induced events removed.
Line 79: We agree with the first comment and omitted the word “enhanced”. Furthermore, to clarify that we are here talking about natural earthquakes only, we added a statement in section 3.2.

line 133: “mode I fractures“: I’d suggest to simply say “extensional fractures” instead of mode I. You have nowhere introduced mode II and III fractures for shear fractures, i.e. faults s. str. either. In my opinion, the definition of mode I, II and III has no particular relevance beyond the rock mechanical community. In structural geology it usually suffices to discriminate between extensional and shear fractures.
Line 133: We agree and use the term “extensional fracture”.

line 137: “slickensides”: sorry for being an ultra-picky knowitall, but a slickenside per se does not form a kinematic indicator yet. It merely tells us that - assuming Bott’s paradigm - it formed parallel to the maximum resolved shear stress acting upon a plane. But it requires extra information from asymmetric structures along those slickensides (slickenfibres, slickolites, …) to tell the slip sense.

Cetero, “i.e.” (id est) implies that you’d consider kinematic indicators and slickensides synonymous terms or at least along same hierarchical categories, which is not the case.

Line 137: Thanks for the hint, we considered this comment and specified “slickenfibres, slickolites”.

line 202 ff: part of the analysis to obtain the mapped fracture’s fractal dimension D involved a Maximum Likelihood estimation (MLE) that was used for fitting datasets that were normalized with the area term L^D to a power law. Was this normalization done on the previously post-processed dataset with “truncated” and “censored” fractures below and above certain thresholds removed?

Line 202: The normalization was done after post-processing the data, meaning that we only used the “clean” data (without truncated and censored parts) for this step. To clarify this, we chose to slightly rephrase the corresponding sentence.

line 205: “faults” should be in plural form.

Line 205: Corrected.

line 252: please see my comment to Fig. 1, caption, below, referring to this text: Please also refer to the strike directions of dominant fracture sets in the caption, not only here.

Line 252: As mentioned at the comment to Fig. 3, we now refer to the strike directions consistently in this section.

line 285ff: Question: what if the alpha_F and the dimensionless fault density term c were calculated separately for different bedrock lithologies? Perhaps one would discern some control of the lithology after all?

Line 285: The analysis is already considering differences in lithologies, as the analysis was done for each site separately. Sites A and D are completely within the crystalline basement rocks, while sites B and C lie within the carbonate rocks of the Helvetic units. In our analysis of the four sites with differences in lithologies, we could not find any significant difference in power law statistics. This does however not mean that such differences in power law exponents do not exist, as different mechanical behavior of different types of rocks may lead to deviations in the power law metrics. However, with our data, we cannot prove (or reject) this hypothesis.

lines 312 ff: Please explain that this range of values for alpha_R results from your analysis regardless which of the three scaling laws were used (L14, T17 and WC94).

I could not find a value of 3.88 for alpha_R in any of the plots for subdomains A to D in Fig. 8 at first, until I realised that there are numbers in very delicate light grey font in each bottom left panel. Some visually impaired people might find that difficult to read. Please think of improving contrasts!

Please also consider my comment left in the caption to this figure 8 below.

Line 312: We agree with the first comment that we should state clearly that the mentioned alpha_R range is regardless of the used earthquake rupture scaling law, so we clarified this in the text. Furthermore, we also improved Fig. 8 by enhancing the contrast for better readability.

line 391 to 393: “active planes” and “active nodal planes”: in lines 391 and 393, you mention “active planes” and “active nodal planes”. I guess that you mean the same in both cases, i.e., the active fault plane as one of the two mutually perpendicular nodal planes of a fault plane solution. If so, please clarify that you refer to the fault plane and use the terms consistently.
Line 391 to 393: We agree that using two terms for the same thing (“active fault plane”) might be confusing, which is why we chose to stick to the term “active fault plane” in the updated version of the manuscript.

line 406: don’t you simply want to say “directions”? Directivity is sthg else.
Line 406: Corrected.

line 408: “(…) indicating a link across lithologies”: better perhaps to say more explicitly: “(…) indicating a link across various structural levels and different lithologies”.
Line 408: We agree and modified this accordingly.

line 414: “insensitivity of alpha_f to lithological variations”: again here, I’d suggest to insert that you refer to lithological variations across different structural levels (i.e. basement - cover separation) and not to any along-strike facies changes (N.B. even if unlikely, someone with a more stratigraphic background might erroneously assume so).
Line 414: We agree with this comment and wrote this more specifically.

line 422: “ranges” instead of “is ranging”.
Line 422: Corrected.

line 435: “brittle-viscous”: hello, the ultra-picky knowitall reviever is back…. ahem, perhaps better say “frictional-viscous” in my view, because this refers more to the physical deformation mechanisms in both cases, whereas “brittle” is description for the degree of spatial localisation of deformation, i.e. only deformation style rather than physically underlying principles.
Line 435: We agree that “frictional-viscous” is more appropriate and modified this accordingly.

Specific comments on figures

Fig. 1: Since you refer a lot to differently oriented nodal planes and actually rupturing fault planes, as well as their kinematics (left-lateral, right lateral, etc) in the discussion of your study (5.3), it would be very worthwhile to have ideas about the prevalent stress field in the study area. Refer to e.g. the work of U. Kastrup 2004 or Houlié et al 2018*. You already cited Kastrup et al, 2004 anyway, but perhaps some synoptic representation of her main results of stress direction determinations (and their variations) on the scale of your map would be helpful.

Fig. 1: We appreciate this suggestion and include information from Kastrup et al. (2004) about the recent stress field in the updated version of Fig. 1 in panel b). We also chose to incorporate the citation of the more recent publication of Houlié et al. (2018).

Fig. 3: imho, you should indicate that your orientation histograms show strike directions of fractures. Please note that in the corresponding text (lines 250 ff, section 4.1.1.) you refer to the dip directions of fracture sets A_Im A_II AND A_III rather than referring to strike directions.

Fig. 3: We clarified that the rose diagrams show strike directions in the figure caption of Fig. 3. Furthermore, we agree that referring to both strike and dip directions might be confusing for the reader, and thus tried to refer to the strike directions consistently wherever possible in section 4.1.1.

Fig. 8: (See also comment to text lines 312 ff)

Please explain that this range of values for alpha_R results from your analysis regardless which of the three scaling laws were used (L14, T17 and WC94).

I could not find a value of 3.88 for alpha_R in any of the plots for subdomains A to D in Fig. 8 at first, until I realised that there are numbers in very delicate light grey font in each bottom left panel. Some visually impaired people might find that difficult to read. Please think of improving contrasts!

Fig. 8: We improved the readability by enhancing the contrast in Fig. 8.

Fig. 10: What is the separation between shallow and intermediate depth earthquake hypocenters based on? Is it a geological reason or a seismological one? Also, I suggest adding the abscissa labels for depth BSL (km) in all sub-plots a, b, c, as this will increase their legibility and emphasise their importance as “stand-alone” figures.

Fig. 10: The separation is based on a geological consideration, as the proposed maximum depth of the Helvetic limestone units reaches about 3 km BSL. However, this should not be seen as a “strict” separation, which is why we choose to not mention this in too much detail in text, but it should simply help the reader to distinguish shallower and deeper parts. Regarding the second comment about adding depth labels to subplots a and b, we disagree, preferring to keep a simpler version with less text, which we believe improves readability. Simply adding depth labels to the other plots would not add any information.

Fig. 11: as this is your final and concluding figure summarizing what I think is your most important finding, namely the fact that you consider that shallow crustal faults down to c. 3-4 km depth are less likely to rupture along their entire length in contrast to deeper faults (depths 5 to 9 km). In view of this, I recommend to again explain to the reader the meaning of the terms alpha_r and alpha_f. What is the parameter n(l) on the ordinate axis again? Also, be more explicit in explaining that white are pre-existing fractures and red the seismically active patches along these.
Fig. 11: We agree with this comment and clarified all relevant parameters again both in the figure and in the respective caption.

Technical aspects

Should the authors consider my comments on Fig. 1b relevant (adding more info about the current stress state to more rapidly understand the focal mechanisms and which faults are possible active at present), they might think of adding this reference.

Houlié, N., Woessner, J., Giardini, D., & Rothacher, M. (2018). Lithosphere strain rate and stress field orientations near the Alpine arc in Switzerland. Scientific Reports, 8(1), 2018. https://doi.org/10.1038/s41598-018-20253-z

Technical aspect: As we added some more information in Fig. 1b about the recent stress field, we also incorporated a citation of Houlié et al. (2018) in the text.

Sincerely and best wishes,

an anonymous but happy reviewer ;-)

We hope that the points raised in the first review have been adequately addressed in the revised version of the manuscript and that the manuscript is of publication-quality.

Kind regards,
On behalf of all co-authors,
Sandro Truttmann