Revision of “Detailed investigation of multi-scale fracture networks in glacially abraded crystalline bedrock at Åland Islands, Finland" by N. Ovaskainen and coauthors

General comments

Dear Editor and Authors,

The manuscript by Ovaskainen and cohautors has been improved from its initial version. In particular, the introduction section is now more concise and organised, consisting in two sub-sections, one of the which focuses on the aims and scope of the manuscript. The cumulative distributions of all fracture lengths for all datasets are now shown in a supplementary figure. I still believe that this is an interesting study for the implications, impressive dataset, and the use of a new software for fracture network analysis. Therefore, in my opinion, the study deserves to be published on Solid Earth. However, I think that the manuscript still requires some revisions in the methodology.

I apologize, but I still have concerns regarding the methodology used for fitting single-scale and multi-scale cumulative length distributions. In my opinion, the reasoning for choosing the cut-off length and the assumption that the cumulative length distributions (above the cut-off length) are best fitted by a power-law is circular. The authors assume a priori that part of the cumulative length distribution can be fitted with a power-law, and as a result, the algorithm returns the power-law parameters and the cut-off length. The authors then only consider the lengths above the cut-off when comparing the power law fit with lognormal and exponential fits, concluding that the power-law fit is the best. Essentially, the cut-off length found in this way is taken as representative of the censoring bias.
I agree that the fit with different equations should be tested on the same range of lengths and that lengths affected by the censoring bias should not be considered in the cumulative length distribution fit. However, I do not understand why the authors only consider that specific range of lengths. Additionally, the fit is performed on only around 2% of the dataset, which may not be statistically representative. Another concern I have is why the authors give more weight to the truncation bias and not the censoring bias.

I suggest two possible solutions to address this issue:

1) Specify and convince readers that the assumption that cumulative lengths are distributed following a power law and that only lengths above the cut-off are important while the remaining lengths are not affected by a censoring bias.
2) Revise the methodological part of the manuscript. Here, I see two possible solutions:
   A. Conduct a comparison between the cumulative length distribution on **all** data (substituting Fig. 5 with the new fig B1 plus the power law fit). The authors can discuss the fact that the power-law can only be applied to a small range of lengths, or alternatively, the cut-off can be set as equal to the minimum trace/branch length. Negative exponential and lognormal distributions fit the cumulative length distribution better (looking at Tab B1 and Fig. B1). Additionally, the fit for the multi-scale cumulative length distribution should be performed

on all data, not just on the data above the cut-off length (i.e. the gray data points in Figure 6).

B. Apply the Maximum Likelihood Estimators - Kolmogorov-Smirnoff (MLE-KS) test considering various ranges of lengths, as described in some works (e.g., Dichiarante et al., 2020; Ceccato et al., 2022).

I acknowledge that option 2B, although very accurate, would be very time consuming and the topic is not the focus of the work. My suggestion to the authors is to evaluate option 2A, highlighting that the effect of censoring and truncation has been not considered in the fitting procedure.

The number of lines refers to the clean version of the manuscript.

Marco Mercuri

Specific comments

1. Lines 89-92: Yes, but please anticipate here that the decrease in connectivity with increasing scale could be a a methodological issue.
2. Lines 363-366 These lines should be revised depending on whether and how the authors decide to revise the methodology.
3. Line 374. The authors might be interested in a recent paper which shows the use of Bing Maps for fracture network characterisation: Mercuri, M., Tavani, S., Aldega, L., Trippetta, F., Bigi, S., and Carminati, E. (2023). Are open-source aerial images useful for fracture network characterisation? Insights from a multi-scale approach in the Zagros Mts. Journal of Structural Geology, 104866
4. Lines 489-493 These lines should be revised depending on whether and how the authors decide to revise the methodology.

Technical corrections

- Lines 9-10: “The best fit to model the lineaments and fracture lengths with a common power-law resulted in an exponent of -1.13”. The sentence is not easily understandable during a first read. Please rephrase it.
- Lines 11-12: I suggest removing “could”
- L 24-25 There is a repetition of lines 19-20. I suggest removing these lines.
- I suggest rephrase this sentence into something like “requires collecting fracture and lineament data using a combination of methods and preferably from multiple scales of observation” for improving conciseness.
- L76 Please remove “e.g.”
- L126. I suggest changing “e.g.” with “due, for example, to” or something similar.
- L144. Maybe the reference in the text should be written like “published by Ovaskainen et al. (2022)”. Please check the guidelines for authors.
- L217. Please rephrase in “represent non connected nodes” for better clarity