Dear Reviewer 1,

Thank you for the constructive review and comments. Specifically, the criticism on the lack of clear motivation in the introduction resulted in a major revision of the introduction to better highlight the motivation and to link the contents of the introduction to the rest of the paper. See below for specific responses to your comments. All comments are numbered to allow referencing from other responses.
Line number references are to the PDF document with the changes highlighted.

On behalf of all the authors,
Turku, Finland, April 2023, Nikolas Ovaskainen

The paper is well written, the methods up-to-date and the related results very interesting and statistically robust. The methods are adequately described, the limitation discussed and their reliability and representativeness of the obtained results considered as well. However, some further discussions on methods/results reliability and robustness are needed.

There is a main problem which undermines the possibility to publish the present manuscript in Solid Earth: the aim of the research is not clearly stated and thus it is not clear if the presented methods and results are significant or have any relevant implications for the case study or what are the broader implications for the general analyses of lineament maps from remote sensing. Stating clearly what are the aims and how the presented methods/results can answer to the research questions will also help in reorganizing logically the discussion sections. The paper would be also good if it was clearly focused on the presentation of the new analytical tools and the results adopted for the characterization of a case study, but still the case study need to have a clear aim. I think that the Authors can re-arrange the present manuscript with little effort in order to solve this main issue, otherwise I would suggest to redirect the manuscript to a more specialized journal.

Main issues

The aim of the research and the conclusions/implications of the results need to be explained a little bit more, they are not immediately understandable from reading the present manuscript. Especially in the Abstract, Introduction (where the aims are sparsely presented in bit and pieces) and in the conclusions. There is a discrepancy between the "results and conclusions" (sparsely) reported in the introduction section and the final Conclusions of the paper in Section 6. Are the methods and results presented here resolutive for the problem they aim to solve?

1. We have clarified our motivation and aim primarily in the introduction and added discussion on data, method and statistical reliability based on comments by both Reviewers. The methods for multi-scale analysis of fractures and lineaments have been developed for decades but there still

lacks any easily usable common convention for the analysis. Therefore, some "method development" text must be included in the manuscript to explain the methods we used. Furthermore, there does not exist any multi-scale datasets from Finland outside of our very limited prior research. So there is demand for both datasets and method development and therefore the focus is on both goals in this manuscript. Multi-scale data collection, geologic setting and the analysis all cause uncertainties in the results and in the discussion section we therefore have to consider all with limited ability to completely separate the discussions about them as their effect in the results cannot be separated either.

The cut-off issue needs to be properly discussed in depth. What is the statistical significance of a results based on the analysis of only the 3% of the total dataset? Is it representative of the whole distribution? This needs to be discussed from the methodological point of view (is the analytical method adequate to analyse our dataset?). Is the results based on the 3% of the data representing the entire length distribution?

2. This issue was also pointed out by Reviewer 2. See the answer we gave there in response number 11.

Minor comments

Some part need to be rephrased to be a little bit more concise (the part of the discussion regarding glacial erosion)

3. We have made the discussion section more concise around the discussion on the effect of glacial erosion.

Reconsider the use of “e.g.”,

4. The copernicus template for latex submissions that we used had "e.g." with the comma in numerous examples. If this paper enters the typesetting phase we will fix these details as instructed.

Rephrase the section Lines 92-99 : to be moved to the method section and integrated with the exiting description of the data source.

5. We moved much of the repeated information about the data to the method section where we added any missing information that was only in the introduction. See lines 120-136 in the introduction.

Line 206: “?”?

6. The reference is fixed to Rohrbaugh et al. 2002 as was intended.

Additional fixes unrelated to specific referee comments:

- Modified Figure 3 to include the second, missing, eastern 1:20 000 circular target area.
- Fixed small reference errors.

- Modified all figure sub-labels to use "(a)", "(b)", ... instead of "A.", ... in figures captions and in text.
- Increased font size in Figure 4.
- Headers were fixed to only have the first letter capitalized.

Dear Reviewer 2, Marco Mercuri,

Thank you for the constructive review and comments. The comments on the data analysis resulted in the additional analysis in the Appendix to satisfy a missing part in our length distribution analysis. Furthermore, it resulted in more clear explanations in the text for our decisions regarding the analysis. Your indepth line-wise comments further resulted in a number of small fixes that overall make the paper more cohesive. See below for more specific responses to your comments. All comments are numbered to allow referencing from other responses.
Line number references are to the PDF document with the changes highlighted.

On behalf of all the authors,
Turku, Finland, April 2023, Nikolas Ovaskainen

General comments

The manuscript by Ovaskainen and coauthors deals with a multi-scale analysis of the fracture network affecting the crystalline bedrock at the Åland Islands in Finland. The main aim of the study is to contribute to filling the gap of data sets dealing with multi-scale fracture network analyses of crystalline rocks, which might have important applications, including nuclear waste disposal. The multi-scale approach also allows filling the knowledge gap on the local fracture network at an intermediate scale length range (i.e., 100-500 m). The authors highlight that certain fracture network properties, such as length distribution and connectivity might be scale-dependent. I think this study represents an interesting example of a multi-scale analysis of a fracture network. The study is very well performed, particularly for the collection of a very robust dataset and for the usage of up-to-date methods for fracture analysis, including a new software. Consequently my opinion is that the study deserves to be published in Solid Earth.

A point which does not fully convince me is the analysis of length distribution. As described in lines 249-251 (see also Figure 5) the power law, lognormal, and exponential fit hypotheses for fitting length distribution have all been tested on the range of the lengths (traces or branches) above the power-law cut-off length. For testing the power law fit, I have no doubts on the procedure, but I would like to see also the goodness of fit for exponential and lognormal distributions calculated for the whole data set. Moreover, the data (black dots) below the cut-off are not showed on the figures (Figure 5). Such a procedure also impacts the multi-scale analysis of length distribution (Figure 6). In my opinion, all the cumulative length data should be shown in the Figures 5 and 6. To summarize, I

think that the procedure for the best fitting equation should be revised or better motivated, and some Figures (5 and 6) should be revised accordingly.

1. We acknowledge the issue of representation for all length data in the individual scale length distribution plots (Figure 5). The truncated data, i.e. lengths below the cut-off, are not well represented. This is intentional and it is done to focus the plots on the data above the cut-off where all the statistical fits are done. We remedy this lack of representation in this revision by adding a figure in the Appendix with all length data and associated lognormal and exponential fits. However, such analysis is difficult to tie to multi-scale analysis without confusion for the readers (and writers) and the fits are not comparable to the fits done to cut-off truncated data (Clauset et al. 2009; lines 346-348) and
consequently, the discussion around the appendix results is minimal.

In the multi-scale plot (Figure 6), all length data is shown. The length data points above the cut-offs are colored while the greyed out points represent the data below the cut-offs. Some data points are covered by others.

I suggest publication in Solid Earth after moderate revisions. Please find below specific comments and technical corrections.

Marco Mercuri

Specific comments

The Introduction section is well written and the contents are appropriate for this work. However, I find it quite long and composed by a single sub-section (Review). I suggest reorganizing the Introduction section into subsections and, if possible, reducing its length to better highlight the main aim of the work. Perhaps some parts could be moved to the Discussion section.

2. We have revised the introduction based on this comment and Reviewer 1 comments. Text was made more concise and some was moved to the method section to remove repetition. The length has been reduced and the research questions and aims are better highlighted in the new structure. The introduction starts with a review subsection which is followed with the "Agenda of our study" subsection which should make apparent the aims of this work. The word count of the introduction was shortened from circa 1400 words to circa 1100 words.

Very interesting part of the introduction at lines 56-64

Line 83; 103-104; Please refer to literature and/or briefly explain with text or a table or a figure the different topological parameters (connection per branch, fracture intensity, dimensionless intensity . . .)

3. We added references to the parts in the introduction where we refer to these parameters.

I am not an expert in this area, so I find lines 127-130 not easily understandable. What is the kinematics of the E-W faults? I assume it is left-lateral from the presence of NE-SW extension fractures in their damage zone. Is it?

4. We clarified the text to specify the varying kinematics of the E-W faults, and specified that the sinistral faults were associated with the extension fractures, as you pointed out.

Data & Methods. Looking at the different scales of observation, I noticed that a scale similar to 1:1000 has not been considered. I suppose a "basemap" for mapping fractures at such a scale does not exist for the study area, and perhaps this should be stated in the text. Just a curiosity: have you considered the usage of Bing Maps/Google Maps, or is there too much vegetation?

5. We have tried using satellite images from different sources and found Google Maps to be the most accurate and easily available dataset. However, as you pointed out, the vegetation is the problem. We are only able to use the images in almost perfectly exposed areas without any vegetation. In comparison, in LiDAR data the vegetation is (mostly) filtered so it can be used in more extensive areas.
We have clarified this in text on lines 441-451.

Table 1. What do you mean by "cell size"? Please explain it better in the text

6. We clarified "cell size" on line 189.

Table 3. What is the difference between Number of Traces and "Number of Traces (Real)". Please, explain it in the caption or in the text.

7. We added the "b" symbol to the parameter and explained it in the caption of Table 3.

Table 3. I suggest accompanying the absolute number of each type of node and branch with the relative percentage for higher clarity.

8. We have included the relative percentages of node and branch counts in Table 3.

L295. "Same trend is seen with Dimensionless Intensity B22". The values of B22 are quite similar at different scales, but the trend of P21 with scale is actually the opposite. Please fix the sentence.

9. This was an error and we fixed the sentence.

L295-297. "Connections per Trace and Connections per Branch display a trend with values decreasing as the scale increases with the 1:10 scale having the lowest value". I agree, but values of connection per Branch at 1:20.000 and 1:200.000 scales are actually pretty similar (1.75 and 1.85). I suggest underlying this point.

10. We have noted this point in text but did also point out that the range of values for Connections per Branch is limited between 0.0 and 2.0. This amplifies these numerically small differences so the difference should be considered as at least somewhat significant.

L316-317 and Fig. 5. "The cut-off proportion (i.e., amount of data removed by the application of the cut-off) for the 1:10 scale is very high with 97.82 % of data being cut off." Maybe I do not understand this sentence correctly. Do you mean the power law fit is performed on the remaining data (i.e. about 2% of data), or that the fit is performed on 97.82% of data? I am not sure that a fit performed on 2% of data is statistically significant.

11. The fits are performed on only ~2 % of the fracture data so your interpretation is correct. The majority of the digitized fractures are seemingly affected by the resolution truncation effect (Pickering et al. 1995). Though this makes the fits statistically very uncertain we wished to be consistent in the analysis we performed for all data. Even though the fracture length distribution by itself might not adequately follow a power-law, it does not mean that in a multi-scale length analysis they could not still follow a common trend with the lineament data, at least partly. Therefore we wished to keep the analysis results for the individual scales to use as reference to the multi-scale results. Furthermore, another motivation for the use of the power-law comes from the physical rationale (Bonnet et al. 2001) that fractures follow some scale-independent and possibly fractal law as we mention in text on lines 319-321.

Have you tested the exponential and lognormal fit on the whole dataset, or only on lengths longer than the power law cut-off length? If not, my suggestion is to compare the PL fit as it is with the exponential and lognormal fit performed on the whole datasets.

12. We refer to Clauset et al. 2009 on why we cannot compare the fits to the full data to the fits on the truncated data. Clauset et al. 2009 state that it is not statistically valid to compare fits that have been conducted on different data. However, we added both a figure and a table in the appendix which contains the lognormal and exponential fits to the full length data for both traces and branches (See also response 1.).

L363-365: "This cut-off can be estimated to be the lowest length lineaments which we can consistently interpret without truncation effects caused by resolution of the LiDAR DEM, assuming that the lineament trace lengths follow a power-law." In my opinion, such an assumption should be better justified.

13. We have expanded the discussion around this assumption after the sentence which you pointed out on lines 428-433.

Section 5.3. I think that all the data should be shown on the plots and not only the data which are above the cut-off power law length. I think that (and it is

also somewhat stated in section 5.3), as presented now, the effect of the left tail (truncation) on the potential multi-scale fit is considered (too much?), but there is also a censoring effect due to the fact that the cumulative number of longer fractures is affected by the size of the sampling window. My suggestion is to plot all the data, trying to manually fit them with a power-law which is tangent to the central part of each distribution (see also Ceccato et al., 2022).

14. See response 1. in regards to the missing data points (all data should be visible in the multi-scale plot). You are correct that the censoring effect of the sampling area also affects the results. We touch this issue on lines 522-527 in the "Multi-scale analysis" discussion chapter and issue recommendations for method development. We are skeptical on the use of a manual method to fit the power-law to the multi-scale data as these kinds of methods lack reproducibility and are not statistically consistent. Instead, as we discuss on lines 527-530, we recommend the development of a multi-scale fitting method which considers both the head and tail of the distribution. Furthermore, we recommend the use of the density distribution in place of the complementary cumulative number that we use in this paper. We do however still claim that there is some use in publishing the results that we have as is, as the complementary cumulative number method is used in many other studies and the results are therefore comparable to them and are more reproducible without manual fitting.

L467-469. In my opinion, this option is very reliable. Isn't it part of the "source raster differences" (L466)?

15. We did not fully understand your point here, as we do already state "Another option related to the raster differences ..." i.e. we already link the option to the source raster differences. We do not wish to amplify this option in our study as we provide no examination of its possibility in our digitizations but just point it out from prior studies.

Conclusion no 4 (Lines 491-495). OK, but you also said before that such type of analysis can be affected by a lot of biases. In my opinion, it's hard to imagine that the connectivity of a fracture network decreases with increasing detail of the analysis. I suggest removing or toning down this conclusion.

16. We appended the conclusion with the suggestion that it might be related to methods rather than nature itself. This is the point we wished to bring out and it should be more clear now thanks to your suggestion.

Technical corrections

Line 28. Consider using "multiple" instead of "multi-scale" to improve readability.

17. We changed the word to "multiple" as per your suggestion and

brought up the importance of using multiple scales.

Lines 35-36: There is repetition from Line 27. Please remove one of them.

18. We removed the repeated lines which were originally at 35-36.

Line 111: The wording "The main part of Åland Islands bedrock is comprised of the 1.58 Ga Åland Batholith" is odd. Please rephrase.

19. We rephrased to clarify that we mean by the main island.

Figure 1: The text in the image is very small. Perhaps enlarging the figure in the final version could fix this.

20. We enlarged the figure in the pdf and the text is consequently more clear.

Line 132 (and in many other parts): "e.g." should not be capitalized.

21. Thank you, we fixed the capitalizations.

Line 138: Consider using "As an example" or similar instead of "e.g." at the beginning of the sentence.

22. We have changed some uses of "e.g.", particularly at beginnings of sentences to use "for example". This should improve readability.

Lines 141-142: Please use consistent nomenclature here with Table 1 for better clarity.

23. We have added clarifications both to the Table 1 caption and to the text. We specify by what terms we refer to the resolution and areal extent. However, we believe including the specific terms we use ("Cell Size" and "Total Target Area") along with the more general terms is justified. Resolution is a more general term compared to the "Cell Size" which more specifically states how we defined the resolution for raster datasets. We also refer to "target areas" many times in text.

Line 206: There is a "?" in the brackets. Please remove it.

24. We fixed the citation to Rohrbaugh et al. 2002 as was originally intended.

Lines 215-217: Please use P22 instead of P21.

25. We fixed the reference from "Dimensionless Intensity P21" to "Dimensionless Intensity P22" as you pointed out!

Figure 3: The white text in the image is not easily readable. Please change the color of the text.

26. We changed to color of the labels in Figure 3 to have a orange foreground. This makes them stand out more in the images. We also increased the size of the points that were associated with the labels as they were almost imperceptible before as you pointed out.

Figure 6: Please provide an explanation of the acronyms (e.g., ANCCM) in the caption.

27. We included explanations of both ANCCM and MSLE in the Figure 6 caption.

Line 392: Please rephrase as "[. . .] with high geophysical but low topographic signals."

28. We removed the "and" word to fix the sentence.

Lines 481-483: Please rephrase the sentence to be more concise.

29. We made the conclusion more concise as suggested.

References: Please pay attention to the formatting of some references (e.g. capital letters for the reference at Line 610).

30. We fixed the capitalized letters in the titles of some references including the referred Middleton et al. 2015 reference on line 610.

Additional fixes unrelated to specific referee comments:

- Modified Figure 3 to include the second, missing, eastern 1:20 000 circular target area.
- Fixed small reference errors.
- Modified all figure sub-labels to use "(a)", "(b)", ... instead of "A.", ... in figures captions and in text.
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