Introducing standardized field methods for fracture-focused surface processes research

Response to reviewers.

Reviewer comments in plain text. **Author Responses in bold.**

The authors would like to note that in addition to the revisions outlined below, we also reached back out to Dr. Stephen Laubach – who had previously provided a 7+ page review of the 1st version of the manuscript. In doing so, we received another round of suggestions from him that we have incorporated into the current version. Given his significant contributions, we invited and he accepted to come in as a co-author. We would be happy to provide his additional review if requested.

As background, Eppes met Laubach at the recent PRF2022 Penrose Conference on Progressive Failure of Brittle Rocks. All 90+ attendees were invited to provide input on this manuscript during the open review process. Laubach provided his reviews in response to that invitation. We had no previous collaborations.

Editor Comments:

As reviewer #2, I agree that this contribution is of interest for the community as it starts an effort into standardize mapping procedures. As reviewer #2, I also regret that existing efforts in rock mechanics have not been listed and discussed.

See comments to Reviewer 2 (Colin Stark) below.

Reviewer #2 has made a list of requirements that I engage you to implement. I won't send it back to review if this done seriously.

See comments to Reviewer 2 (Colin Stark) below.

Additionally, I would like you to emphasize that this contribution is more the start of a lengthy process of standardization more than a final state.

We have added additional verbiage throughout the document – and reworded existing verbiage to be clearer about this point – including in the abstract and introduction that emphasizes this work is expected to be a starting point, as well as providing examples in further sections (e.g. the end of 3.5; first paragraph of section 5; conclusions).

Comments from Colin Stark

This paper proposes a scheme for the field description of rock fractures tailored more to the needs of geomorphologists than to those of structural geologists or rock mechanics engineers. The authors propose that a standardized approach – of the kind arguably well established in fields such as soil science and sedimentology – will help make field datasets more consistent and comparable. They claim that such standardization may significantly facilitate scientific advance.

The main text outlines a mix of quantitative and qualitative observations that need to be made at a field site. The authors strive to be make their recommendations systematic and comprehensive. The text rounds off with a brief section suggesting some analytical tools and a short case study.

Main comments:

This is a noble effort at improving how a particular class of field geomorphological data is collected. My sense is it springs from long practical experience of such data collection frustrated by the lack of standardization in the discipline. I have no doubt there will be a select audience for it, particularly among those tackling projects with a strongly descriptive theme, or those teaching field methods for surface process studies.

I can see some merit in having the manuscript published more or less as is, with some improvements to the quality of the figures, and some technical fixes. It has already spent a while in the grind of the review process, and a recommendation of substantial revision at this point might be a little churlish. There is nevertheless the question of whether the paper is a good fit for ESurf, given that it is a purely technical contribution; in fairness to the authors such a decision should have been made considerably earlier.

I do have some reservations though.

A proposal (because that's what the authors are making) of this kind ought first to review the state of the art. Here this should include a survey of methodological standards set out in the literatures of structural geology and rock mechanics. Instead, there is a brief introduction that makes passing reference to some of those standards. I was genuinely expecting to learn much more about how things are done in these disciplines before reading about how the authors mean to adapt them. Omitting such a review makes it hard to critically assess the authors' contribution. To be fair, section 1.2 (p.112+) makes some effort in this direction, but it doesn't go very deep or far enough.

We have completely rewritten section 1.2 to expand the review of literature about existing – disparate - methods for fracture characterization of exposed rock.

In order to ensure this latest attempt was more complete, we reached out to Stephan Laubach – who had provided a 7+ page thorough review of the 1st draft of the manuscript – to ask what he suggested as far as an approach. He replied that in the context of natural outcrops and geoscience applications: "I agree that for outcrop fracture characterization there are no formal procedures that I know of".

We have thus incorporated more detailed examples of the basis of our overall approach from both structural geology and geotechnical engineering literature, trying to increase the

breadth and depth of the literature review, while trying to avoid making this paper over-long.

At least one of the technical rules is dated to the point of being incorrect. On l.385-386, the authors suggest collection of "coordinates to 0.00000 dd", which is not a recommendation I would make: locations should be recorded as meters E and N in a specified coordinate reference system (specifically indicating the datum). Simple recording of "lat/long" is risky without making clear the CRS and the geoid used. And why use decimal degrees at the outcrop scale? Typically such locations are collected with a handheld GPS and transferred to a GIS, which often but not always preserves such metadata, but this workflow is not mentioned: suggesting all that's needed is a precise lat/long is a little scary. I worry that some of the other guidelines/rules may be similarly flawed, but that I'm not sufficiently expert to judge them.

We could not agree more about this error. It is an important oversight that we did not mention always adding the projection to the recorded coordinates. We are glad that this was caught and is now added, and the use of a meter based coordinate system is also added.

The section suggesting power-law analysis of fracture data is a little odd. It would be enough to mention the common practice of treating fracture patterns as self-similar, and to cite methods for quantifying the self-similarity, without going into laborious detail about some of the specific mathematical steps.

We now frame the analytical discussion more generally before diving into the math. However, we feel that it is necessary to provide some best practices on how to perform the analysis correctly in this methods paper rather than to refer the reader to a series of lengthy and complicated papers – all of which settle on the methods summarized.

Determining the number of fracture measurements using a power law analysis is needed to provide a statistically representative population for a given outcrop, and is a crucial step to characterizing fracture populations and is necessary for every site examined. This is outlined in that section and we now emphasize the distinction between that and data analysis in the 'mathy' section.

For data analysis however, more detail is required and the suggestion by the reviewer is not appropriate. First, not all fracture sets are self-similar, and also, for power-law distributed fracture networks, some common approaches for finding the exponent have proven inaccurate, so it is important to outline current best practices which we have done.

Further, the math we provide is detailed, similar to the level of the detail of other methods, so that users don't have to second guess best practices that others have already determined. Thus, we left this section as is other than to provide some new explicit justification of the presented math.

The case study is too cursory. Two paragraphs are not enough to assess the benefit of the methodology.

To go into more detail of additional case studies would overlengthen the paper and to add a full case study is its own paper and beyond the scope of this manuscript. We have changed wording to indicate this explicitly and now acknowledge that a full case study would be required to fully test the presented procedures.

I think it's worth mentioning that in the rock mechanics literature, there is a lot of activity in the mapping and characterization of fractures at the outcrop scale using combinations of multiphotogrammetry, lidar scanning, 3D solid geometry modeling, and semi-automated image processing. At the very least, it would have been helpful to see the authors address – in a discussion section – that such techniques are in development, how they help address the challenge of handling inherently 3D fracture information, and how they might eventually be incorporated into a standard toolkit.

This comment is similar to those of a prior reviewer. In the first set of revisions, we had added information about these ideas in the next-to-last paragraph of the introduction. We now give this idea its own paragraph in the introduction and describe it more clearly and in more detail. We also further specify how that type of technology could benefit from many of our methods. We also now include citations to those statements.

Minor comments: **All of the following are now addressed.**

936: Typo: should be "Claire Bossennec"

1330-1331: Typo and missing part of title (date range 2007-2014): should be e.g.

"Ulusay, R (ed.), 2015. The ISRM suggested methods for rock characterization, testing and monitoring: 2007–2014. Springer, Cham, Switzerland. DOI:10.1007/978-3-319-007713-0."

Figure comments:

L.969: “Visual aid” not “aide”

Corrected.

Fig.1: What is H1-19, SS2?

This was an internal note in Figure 2. We removed it from the figure.

Fig.6: The choices of quantity classes seem odd to me.

This figure is derived from a long-used percent estimator (Terry and Chilingar, 1955) that has been well-vetted in the geomorphology and geology community. We left as is.

Fig.7: The sphericity vs angularity images are problematic for me: they are barely distinguishable from their neighbors, and their shape variation makes compare/contrast that much more difficult. Are the numbers derived from analysis of these images? Or are the images hand-drawn and the numbers estimates?

Again, these images as well as the numbers are directly derived from a long vetted comparator used commonly in sedimentology field work (Krumbein and Sloss (1951)). We left the figure as is, but emphasized this idea in the figure caption.

General note: it's 2023 and we are *still* sending out review manuscripts with figure captions on one set of pages and the figures themselves on a different set of pages. This makes it that much harder to read the manuscript efficiently. Please don't do this if you can possibly avoid it.

So noted. We added figure captions to the figures in this version.

Referee #3 – no comments to address