A compilation and review of fracture analysis field methods for surface processes research ought to be a valuable contribution and within the scope of this journal.

I enjoyed this MS and I think it's a valuable contribution.

The paper is well written and clearly illustrated.

There are several places in the text, noted below, where clarifications are needed. A clarification that should be added right at the outset of the paper is: how much of this methodology applies to 'outcrops' and how much to 'clasts'. It seems like the MS aims to be relevant to both, but this should be spelled out. And later in the text, where the text is more germane to outcrops or clasts, care needs to be taken to make this clear to the reader.

A key part of the methodology selection recommended here, it seems to me, comes down to a preference for 2D (window) sampling methods compared to 1D (scanline) methods. In my opinion, some of the contrasts that are made are too strong and ought to be more nuanced. Both methods have their strengths and weaknesses, and in some cases the best method may depend on what type of exposure is available. If the fracture size range is large and patterns are arranged in simple sets and the outcrop is large, scanlines may provide the most robust and readily collected data. If orientation patterns are unorganized and exposures are small (or the object to be measured is a clast), then maybe 2D methods are the only ones that will work. Aperture measurements from scanlines give conceptually unambiguous results, whereas methods that rely on length measurements run into the problem of defining length. Some of these problems are indeed discussed in the text, but currently I think this aspect of the text is a bit misleading and could be better.

It seems to me that at last pointing to recent methods to characterize spatial arraignment would be worth doing; the clustering and connectivity of patterns a key attributes. With drones etc many of these attributes can be readily measured and quantified. There is the 2018 Journal of Structural Geology special issue on spatial arrangement, and several papers in 2022 extending 1d techniques to 2d. From a geomorphic perspective, some of these methods might be a real advantage to go beyond the limits afforded by outcrop size, by comparison with 2 measures of topography, vegetation, etc. For a link to the literature see R. Correa et al. 2022, J. Struct. Geol.

A more specific statement of claims at the end of the Introduction would be helpful.

As noted below, changing the 'crack' and 'fracture' terminology usage would make the paper clearer and more readable.

Some care needs to be taken in words ending in -ing. Both 'fractures' and the process of 'fracturing' could be meant in some circumstances, but in some cases its not clear which is meant.

29 Fracture terminology needs to be used with caution. Terms should be descriptive, which means that relations to stress states (which need to be inferred) should be avoided. 'Opening-mode' is fine, widely used, and better than the alternatives ('joint', 'vein'). The term 'shear fractures' has been criticized in a widely cited review (Pollard and Aydin, 1988, GSA Bull.); a better term is 'fault'. 'Compression mode' should be omitted. This is a stress term, and 'compression mode' cannot be determined by looking at a fracture in the field (see the discussion in Laubach et al. 2019, Reviews of Geophysics). All modes of fractures can form in compression or extension. Compression is one of the most common loading

conditions that lead to opening-mode fractures, for example (Hancock, 1985; Engelder, 1985). These stress terms are not descriptive (so inappropriate for field terms) and should be restricted to where the loading conditions are known, for example in experiments and, I supposed, monitored fracture propagation in the field.

30-32 While it is true that some fractures form at or near the Earth's surface, many fractures form at depth (even at great depth) and some of these fractures make it into the outcrop. I think a casual reader here might mistake your meaning and (incorrectly) think that all fractures form in the near surface. I suggest adding a phrase to clarify this: "…bodies (Molaro et al. 2020), as well as at depth (e.g. Laubach et al., 2019, Rev. Geophy., which provides links to many other papers)." This at least alerts readers that near or at surface fractures are not necessarily the result of near or at-surface processes, on this planet or elsewhere.

32 On the use of 'crack' and 'fracture' interchangeably. Although this usage is widespread it has the potential to cause confusion, particularly where these may be language barriers. The text jumps back and from between 'fracture' and 'crack' and I found this distracting. In brittle structural geology a case has been made for restricting 'crack' to experimental and theoretical applications, and 'fracture' for features observed in the field. I believe this convention is stated in Anders et al. 2014, Microfractures: a review, J. Struct. Geol.) Maybe field-monitored examples you have described on fracture propagation in outcrops or clasts would fall into the category of 'cracks' by this convention. My advice is to make a distinction between these two terms along these lines and revise the MS accordingly. Even if the distinction has not been made in the past in this field, it would be useful to do so now.

I also note that in structural geology the preference in description is to distinguish 'opening-mode' fractures from 'faults'. In this literature, if one type or the other is the main focus, this may be stated at the outset, and subsequently the features are just called 'fractures'. Faults and fractures are usually readily distinguished in the field and doing so is commonly among the first steps in outcrop fracture analysis. For some commentary on these distinctions see papers by D. Peacock.

33 This seems strangely phrased, it makes it seem like this is possibly mistaken usage. Dikes and some veins *are* fractures; the veins that are 'filled' with secondary minerals (i.e., they are not replacement deposits) are also definitely fractures. This construction also misses that key observation that many fractures are only partly filled with mineral deposits. I hope that the field methods for fracture surface processes would include a step where such features are sought; in many cases all that is needed is a knowledge of what to look for and a hand lens.

34 The 'size, number, and orientation' doesn't capture all the controls, so I advise adding to this list. These are attributes at the same level as the ones you list. 'Connectivity' has long been recognized as a key to strength and fluid flow (e.g. Long and Witherspoon, 1985) and since the 1990's there have been useful methods for quantifying and documenting these attributes in the field (e.g. Sanderson and Nixon, 2015; Healy et al. 2018; see the reference list in Forstner & Laubach J. Struct. Geol. 2022). Connectivity is one aspect of spatial arrangement; another is the pattern of fracture arrangement in space (evenly spaced fractures, random, clustered in space). Fractures clustered in space are an extremely widespread phenomenon that often has an impact on landscapes, the locus of rock mass weakness, and fluid flow. There are quantitative methods to rapidly document these attributes in the field in 1d and 2D (see the reference list in Correa et al. 2022, J. Struct. Geol.). Finally, mineral deposits, even subtle inconspicuous ones, can dramatically affect strength, strength anisotropy, and fluid flow. Some of these deposits are

inherited from fracture formation and depth, other may form in shallow subsurface or in outcrop. I hope standardized field methods would aim to notice these.

35 An ‘e.g.’, needed here. The role of fractures on rock mechanical properties (and rock mass properties) goes way back.

44 I think you mean ‘…factors that control *near surface* rock fracturing…” Factors ‘controlling’ and the ‘rates and processes’ at depth will be different. Most of the standard methods, however, are for describing aspects of pattern geometry, etc. not necessarily rates and processes directly. So maybe the statement of the goals should be amended here (44) to ‘…factors that control near surface fracture and fracture pattern attributes, rates, and processes…’?

56 ‘detailed’ seems like a vague word. I suggest you mention specific scales or omit.

60-61 Although fair enough ‘microfractures’ are not features usually distinguishable in the field, as by definition (e.g. Anders et al. 2014, J. Struct. Geol. review) they require microscopy to document. But since the time of Dale (1920) it has been known that microfracture populations can control strength anisotropy and that this can affect how rocks subsequently fracture in outcrop or as building stones. In principle a simple unconfined axial point load test can reveal such a fabric (I’ve seen this done using a Schmidt hammer). So it is not outside the realm of possible field methods to attempt to make the distinction or to collect samples to investigate the presence of microfractures back in the lab. For certain rock types, like quartz arenites or quartzites and some granites, such fabrics are to be expected and a field method punch list that didn’t at least include the option of looking at this seems like it would be misleadingly incomplete. My suggestion is that in your list of preferred field methods that this be included as an option, with some references to reviews of methods.

96 The first clause of this paragraph needs clarification. It’s probably also an example of where a distinction between ‘crack’ and ‘fracture’ would be useful. I think what you are talking about here is standardized methods for ‘direct or monitored observation of crack propagation’ in outcrops or clasts. If that’s the case, the statement is fine (but needs clarification), but while there may not be a specific check lists for outcrop fracture characterization (some sort of ‘official’ standardization) it would be wrong to say that there are ‘limited studies’ of reproducible fracture characterization in outcrop. Much of the diversity of such studies in the literature has to do with the specific aims of the studies. Outcrop analog studies of subsurface fractures fossilized in outcrop typically identify (to the extent they can) and omit features that formed in near-surface environments.

107-111; 119-128 Some of this variance has to do with inherent ambiguities in the features being measured, for example length and connectivity. Some of this is discussed in Forstner & Laubach 2022, and before that Ortega and Marrett 2000. These built-in ambiguities are a reason 1D aperture measurement scanlines (e.g. Ortega et al. 2006) are valuable: aperture measurements on scanlines are reproducible; length measurements not so much.

In my opinion, this paragraph could use some work. Using comparators seems like it follows from your topic sentence. But comparator use like suggested by Ortega et al. is primarily for 1D scanline data sets. The rest of the concepts in the paragraph in its current form seem jumbled. Line 2 starting “For example, our approach…is preferable…” is only defensible in the context of some specific application; for many applications documenting separate, but mechanically linked fractures would be preferable, for example, in comparing outcrops to fracture growth models, for inferring stress states, for understanding connectivity and fluid flow, etc. Without further evidence or argument, I'm not even sure this is (always) the best approach in the context of geomorphology. So maybe this assertion should wait until you develop these arguments.

It seems to me that what you are trying to say here is along these lines: "We incorporate the suggested best practices from the two case examples above as well as from other published methods research. Some methods are well attested to be reproducible in field studies. For example, field measurements using comparators are effective for opening displacements particularly for sub mm widths (e.g. Ortega et al., 2006) (section 8.4.2). Window sampling tends to provide accurate measurements of networks (e.g. Zeeb et al., 2013) with the least user-variance (Andrews et al., 2019). Other measurements such as length and connectivity may have low reproducibility (Andrews et al. 2019) owing to various observational and conceptual problems including dependence on scale of observation (e.g. Ortega and Marrett 2000) and require construction of rules to assure reproducibility (Forstner & Laubach 2022). We recommend rules that are suitable to geomorphic applications."

All these aims need to consider the limitations dictated by the size and quality of exposure and the resolution limits and biases of outcrop documentation methods.

128 Wu and Pollard 1995 is not 'several studies' but is an account of an experimental study so it seems like a strange call out for a section on field methods. Field data has many ambiguities and challenges that simple experimental results avoid. In any case, earlier in the paragraph you recommend using a fracture size cut off, so that's not a 'complete inventory'. I think 127-8 can be omitted.

132-136 Some of this seems a bit garbled. The Milad and Slatt example is strangely specific (and probably not a 'common' one); the Hennings et al example as stated is quite vague. These both seem in the wider 'non geomorphology' uses category. The third example seems to be geomorphology adjacent, and so not parallel with the other two. I suggest that you make the non-geomorphology examples more general in scope but describe them a bit more specifically and move this up to right after your topic sentence. The move to the geomorphology adjacent topic and geomorphic aims.

Suggested revision from line 130 "We chose standardized methods optimized for collecting data relevant to geomorphology. These methods differ from those for outcrop fracture studies with other goals, such as using outcrops as guides (analogs) for deep subsurface fractures. Such studies aim to distinguish mechanical and fracture stratigraphy (e.g. references); corroborate fracture patterns related to various processes such as folding (e.g. references); obtain fracture statistics for discrete fracture models (e.g. references), or test efficacy of forward geomechanical fracture models (e.g references). For these applications, near-surface and geomorphology-related fractures are noise and need to be omitted (e.g. Sanderson, 2016; Ukar et al. 2019). For such studies, mineral filled fractures may be more useful or appropriate than open fractures, yet we discount such sealed fractures because they may have less impact on geomorphic processes. Our results are germane to near surface (shallow) studies such as validating geophysical measurements…" etc.

140 The Introduction seems to lack a clear statement of claims. I suggest adding some.

143-168 This section should be edited to make it clear that your focus and assertions are on the geomorphic setting. It has long been known, separately from subcritical crack concepts, that much fracture in the Earth is repetitive and protracted rather than a single catastrophic event.

155 I suggest calling out the 2019 Reviews of Geophysics paper ‘Role of chemistry in fracture pattern…’ here. This introduces some of the more recent literature on this topic.

164 It seems to me that you could call out some more recent measurement methods papers here; for sedimentary rocks, for example, the laboratory and analysis procedures have much advanced since 1963. See the 2019 Rev. Geophy. Paper for some more appropriate references.

172 Why not include a one-line explanation for what this approach is? This paragraph could use some work making it friendly to readers not up on the soils literature. Here’s a recommendation starting at line 170: “Parent material, topography (and other loads), climate, biota, and time all potentially impact initiation and propagation of surficial fractures in rocks. Consequently, as in soil analysis (e.g., Jenny, 1941; Phillips, 1989) a ‘state factor’ approach taking all these factors into consideration is appropriate for rock fracture analysis…’

This seems fine; but I’d be surprised if these concepts were absent from the rock mechanics literature. Maybe some additional reference checking is needed?

193-235 Is there a reason that these sections are presented in this order? It seems like a logical order starts with the material (maybe things are different in soils, where the soil is a byproduct of climate). I suggest you mention 2.2.4 ‘parent material’ first, then the loads, physical and chemical catalysts to fracture, and duration of loading.

Under ‘parent material’ you really ought to clearly note ‘pre-existing fractures’; it is a rare outcrop that lacks fractures that formed in some setting long prior to exposure at the earth’s surface. If your standard field methods do not take this into account, you stand a good chance of going astray. Some of this material is in 2.3, but that material is out of place there. Also, the criteria repeated in an old paper on fractures in tunnelling applications (Ewan et al 1983) is hardly a robust reference for criteria for identifying ‘tectonic’ fractures (this sounds like a straw man argument); better to cite Hancock, 1985, a review of brittle structure methods, and reviews of Geophysics, 2019, an updated review that explicitly points out the challenges and current methods for resolving these issues. The comment also also seems like discussion out of place here and should be taken up later instead.

Under parent material, before you start discussing the sizes and shapes of clasts, the first step should be diagnosing the parent material: Line 214—“The parent material (p) in the context of a fracture study refers to the specific rock type(s) containing fractures (and potentially undergoing fracture) in the geomorphic environment. Rock assessment should include the types and dimensions of material present (e.g. sandstone, siltstone, shale, granite etc.) and the types and spatial arrangements of interfaces within the material (beds; foliations). Many (perhaps most) rocks contain fractures that formed prior to exposure, either due to deep seated tectonics and fluid pressure loads (references) or to thermal and mechanical effect due to uplift towards the surface (e.g. references; Engelder; English & 2017). In sedimentary rocks fracture patterns (in some cases, fracture stratigraphy) varies with mechanical stratigraphy (e.g. Laubach et al. 2009, AAPG Bulletin) that can also influence near surface fracture. In many instances, mechanical properties variation may be reflected in fracture stratigraphy. Schmidt hammer measurements (references) is also a useful, fast, and inexpensive field approach to documenting mechanical property variability. Although pre-existing fractures may not always be easily separable from those formed under geomorphological influence, an early step in fracture assessment should be to use standard approaches to categories outcrop fractures based on preferred orientations, crossing and abutting relations, and evidence of mineral deposits (e.g. Hancock, 1985 J. Struct. Geol.; Laubach et al. 2019)…”

232-233 Although what you describe here likely happens in some cases, this is not universally true. I’m not aware of any studies that document this. Given the challenges of determining when and why fractures form, this is unsurprising. Nevertheless, there are certainly some fractures that formed at depth and have made it to the surface unchanged. So some nuance is needed in revising this paragraph.

It seems like you have elided three things here. (a) One is the loading path, which can be quite long for old rocks, and include a wide range of past tectonic settings, which could influence the fracture patterns in the rock. (b) Another is the last part of the loading path, the thermal and mechanical changes that happen as a rock goes from depth to exposure. These effects might include modification (as you describe) or fractures that formed at depth, but it also might not. This uplift and (eventual) cooling path could also result in new fractures (a process recounted in a theoretical sense in a lot of structure text books). The extent of this process depends on how deeply the material was buried, how rapidly uplifted, and material properties (some of this is made more explicit in English, J.M., and Laubach, S.E., 2017. Opening-mode fracture systems – Insights from recent fluid inclusion microthermometry studies of crack-seal fracture cements. In Turner, J.P., Healy, D., Hillis, R.R., and Welch, M., eds., Geomechanics and Geology: Geological Society, London, Special Publications, 458, 257-272. doi:10.1144/SP458.1 (c) And finally, there is the current tectonic setting of the outcrop, which might be such that tectonic loads drive fractures (as in some pop ups in the US mid continent). You don’t mention the concept of ‘residual stress’ but tht could also play a role here.

This section of your text doesn’t give geomorphic workers much guidance as to what to do about it. Maybe: (1) from rock age and tectonic history of the region, qualitatively assess likelihood rock have a complex/simple fracture and mechanical property history; comparing fracture stratigraphy (if present) with mechanical property stratigraphy (from Schmidt hammer) determine if there is a discrepancy between the two; (2) from published burial history accounts, assess the uplift path; (3) situate the study in its current tectonic setting. A place to start is the world stress map: Heidbach, O., Rajabi, M., Reiter, K., & Ziegler, M. (2019). World stress map. In Encyclopedia of petroleum geoscience (pp. 1-8). Springer.

249 The removal of pre-existing fractures in clasts seems straightforward. But it is a matter of observation that pre-existing ‘inherited’ fractures exist within clasts. Inherited fractures are more likely to persist if they are mineral filled. But partly open fractures that have persisted in clasts are known. So inspecting clasts for evidence of such inherited fractures should definitely be a part of clast assessment. This goes back to the comment I made above about the need to investigate microfractures. In some materials arrays of sealed microfractures can impart a strong strength anisotropy. If you have a material with a strong strength anisotropy it may fracture under environmental conditions with a preferred orientation. Make microstructural observations and axial point load tests a part of the procedure?

277-8 Do you mean that clasts this small are likely to move, or to have been moved?

288-289 ‘common’ and ‘sparse’ seem like vague relative terms. Can this be made more explicit?

310-311 some of this is redundant. Consolidate.

313-314 Hmm. Maybe Lapointe scanline undersampled small fractures, but this is not a general problem with scanlines. See for example Marrett et al. 2018, J. Struct. Geol. or Hooker et al. 2009, J. Struct. Geol.; some of these scanlines document minute fractures and cover three orders of magnitude in size. The distinction is between 1D and 2D sampling, but window sampling has its biases too, and lengths are subjective to define and harder to trace out for the small size fraction.

337 How does this compare with the standard rock quality indices from rock fracture analysis?

336 You may be constrained by the size of outcrop available. Also, in many cases the ‘best’ (cleanest, largest) outcrop may be selectively the least fractured. This is a well-known bias in fracture analysis and ought to be mentioned. Vegetables like rock having open fractures.

350-353 The use of both fracture and crack terms here make this confusing.

357 Many readers may wonder what you are talking about with these mineral ‘bridges’. I suggest that you call out a figure from the 2019 Rev. of Geophys. Paper. That way the meaning of the term will be clear (this is at least one example of this kind of phenomenon), and readers will be pointed to the literature on how such cement deposits form and how widespread they are.

601 Opening-mode fractures tend to grow in length via linkage, so determining ‘length’ where there is a hierarchy of linkage could be (usually is) a challenge. Measuring and quantifying the links and then prescribing a reproducible rule is helpful (e.g. Forstner and Laubach 2022).

617 Measuring apertures of where fractures cross scanlines is not subject to this bias. Outcrop studies show that for isolated mechanically linked segmented fractures the widest fracture will be in the center, where you would expect it based on fracture mechanics to be for a single strand fracture; but as patterns evolve and link the pattern can become complicated; (619) ‘keeping in mind that the “center” of the fracture may be separated from the tips by physically separate segments’.

627 Note that comparators are scaled in different ways. The logarithmically binned comparator of Ortega et al. 2006 is best for documenting the size ranges of narrow fractures (this should be the standard tool).

630 Hmm. Not sure how common this ‘misconception’ is. I’m not sure how helpful citing an unattested misconception is.

642-643 So you make no distinction between ‘open fracture network connectivity’ and ‘open fracture length’ This seems like in practice it will lead to trouble. Also, you are dealing with 2d surfaces and 3D objects that may commonly connect out of the plane of the observation surface. Hmm.

737 I would say in the ‘structural geology’ literature (P10); ‘density’ is also used (Narr). Maybe add a reference for this: Dershowitz, W. S., & Herda, H. H. (1992, June). Interpretation of fracture spacing and intensity. In The 33rd US Symposium on Rock Mechanics (USRMS). OnePetro.