The contribution proposes a 'complete and rigorous workflow' to acquire fracture information from outcrops to 'constrain models of subsurface fracture networks' by using the outcrops as analogs for the subsurface. The contribution is within the scope of the journal. The research methods are solid, the topic is of considerable practical and fundamental interest, and the MS is mostly well presented and has generally clear illustrations. The associated codes should be welcome by many. And this contribution should be widely read and used. There are a few places, however, where the text can be adjusted or added to so that the scope of the workflow is clearer and this workflow is situated relative to a really complete and rigorous description of outcrop fractures.

For example, the scope of the paper needs to be clearer. The Introduction text and the title give the impression that the work is a complete workflow for characterizing fractures in outcrop, but the workflow is actually narrower: the emphasis is extracting traces from 3D DOM representations of outcrops. This is a valuable element of outcrop fracture characterization but it is only part of the equation, and leaves out some key steps for acquiring fracture information from outcrops to constrain models of subsurface fracture networks. Possibly the authors allude to these other elements in what they call 'traditional direct geological and field survey' (line 116) but it is hard to tell because these supposed traditional methods are not adequately explained. This issue could be resolved by (1) revising the Introduction to more clearly state the scope and (2) provide at least a thumbnail sketch of the other elements in a complete outcrop fracture workflow. Much of this latter step can be accomplished by reference to the literature including perhaps a short synopsis of the steps mention in Peacock et al. (2022) and Elliott et al. (2025). I've provided further suggestions in the comments keyed to lines in the text.

The Introduction seems unfocused and could do better at setting the scope of the paper and drawing in the reader. It would be useful to revise the Introduction to end with a clear, explicit statement of claims (here we show that…). Currently the reader is not alerted to what the paper shows and so may be unmotivated to read further. Also, some of the material that is in the Conclusions is not obviously prefigured in the text or in the Introduction.

There are places in the text where the fracture trace map connectivity pattern is equated with flow pathways. This may be ok if the paper were about flow modeling on a hypothetical network where all the fractures were specified as being open. But it is a problematic step in an MS about collecting data from outcrops; the fractures in outcrop may or may not be open or partly open. So the text about connectivity and flow needs to include this caveat.

Comments keyed to lines in the text

29 The Introduction. Think about creating an opening line that points the reader to the focus of your paper and organizing the elements of the Introduction from general to specific. Line 36 is the most general and ought to be first: '*Fractures exert a fundamental control on mechanical and hydraulic properties of rock*' and '*knowledge of fracture attributes has application to many societally important engineering operations*' (cite a few references, or put all the references from lines 37-45 in a table. This long text list of applications goes off topic and some of it is repetitive.) Then cover what is important about fractures: '*The effects of fractures on strength and fluid flow depend on several factors, including mode (fault versus opening-mode), mineral fill, orientation, size, and spatial arrangement* (some references).' Then focus in on your topic: how fractures are arranged in space. You might want to consider how topic is covered in the 2018 J. Struct. Geol. review of spatial arrangements of faults and

opening-mode fractures: '*The arrangement of fractures in space and in relation to one another into networks*…' note that size and spatial arrangement are challenging or impossible to document using well data and then go to something specific about what your paper provides. Much of lines 68 to 109 seem distracting for an Introduction. These thoughts may be more effectively explored in the Discussion.

30 This definition of 'fracture' seems overly broad. Useful definitions to some extent depend on the application and how results are going to be used. The Schultz 2019 definitions might be useful in rock mechanics/excavation/engineering setting, but seem to me to be the wrong place to start if your objective is using the outcrop as an analog for the subsurface in the applications that you list next. And there are other uses for outcrop fracture characterization that might use different definitions. For example, for geomorphologic work—which uses many of the same analytic tools of topology, spatial arrangement, and aperture measurement—alternate categories are useful (see for example, the workflow paper by Eppes et al.).

So I suggest you adopt a more structural geologic definition (faults and opening-mode fractures) and mention that for other applications workers may need other terms or definitions.

Not all bedding constitutes discontinuities. And why do you say foliations are a primary feature and fractures secondary? Foliations are certainly in the same 'secondary' category as foliations. Later in the text you do not consider bedding to be 'fractures' so you don't seem to be following your own definition.

30 'where'?

35 for the 'filling' aspect these references seem inadequate and moreover, it has already been shown that using mineral deposits alone to help define sets is quite misleading for several reasons. I suggest you call out the Reviews of Geophysics 2019 paper here, which is already in your reference list (line 1185). As for the other citations, I think citing textbooks is not favored. And you already have the Hancock 1985 review in your reference list with its classic summary of fracture set rules.

46 And there are some papers that are examples of extracting fracture information from outcrop specifically for these applications. How does your work differ from or advance from these other studies?

47 'and rock strength.'

31, 35, 55, 130 A complete description of a fracture network and a method for extrapolation from outcrop to subsurface ought to have a step in it where the diagenetic state of the outcrop host rock and fractures is documented. What is the diagenetic state of the host rock and what kinds, if any, mineral deposits are in the fractures? This is not a hard step to add. There are a number of examples in the literature that describe how to do it. It's just a matter of describing (or even reporting) host rock and fracture properties; and if for some reason this cannot be done (not even one thin section?) then at least the 'complete and rigorous workflow' could mark this as a gap. The paper need not be made much longer by mentioning the need for such a step. The MS already cites one paper that makes this point (Forstner and Laubach 2022) so this is not a matter of adding a citation.

And the case has been made in the literature, and I think it is hard to dispute at this point, that key parameters such as connectivity and length are modified in essential ways by cement deposits. This information, if possible, should be included in the basic network description and the topological formulations. For example, there is a big difference between a network of two orthogonal joints sets, where all fractures are open, and a geometrically identical arrangement where the first set is filled (veins, for example) and the second is open, and a case where all the fractures of both sets are sealed. All of these cases have been found in outcrop. It's not helpful to the modeling or analog user community to report that all with connectivity indices based solely on the trace patterns.

As a brittle structural community and creators and users of fracture outcrop analogs we can't be satisfied with methods that ignore mineral deposits when core data shows that such deposits are a fundamental attribute of most of subsurface fractures that are of interest to practical applications. Moreover, the mineral deposits in fractures are one of the few features that can reliably be measured in both outcrop and subsurface. Such observations can be a useful part of relating outcrops to specific subsurface targets (Ukar et al., 2019; Elliott et al., 2025). Host rock composition and diagenesis is also something that you need to know in comparing mechanical and fracture stratigraphy from analog to target.

50 Table and 55-60 Text

This list of fracture properties needed is incomplete.

The table footnotes should do a better job of explaining what you mean by 'static/dynamic'. Overall the table does not seem very clear, and some aspects are questionable. For example, by starring 'network' for 'topology' but not 'sets' you imply that topology can (or should) only be documented for 'networks' but this way of thinking of the issue is limiting. For example, if there is only one set, you could still define the topology (it would be the topology defined by fractures in that set). Core data suggest that 'one set' is common in several basins (see papers by Laubach and by J. Lorenz from the 1990s). If you leave this circumstance out, you may be missing just what the analog is supposed to capture. And in many cases, the network in outcrop in not what you want to describe the subsurface. Instead, it would be better to isolate part of the outcrop network for topology analysis. The literature has plenty of examples of outcrop fracture studies where the first step is figuring out what weathering and other 'near surface' fractures ought to be omitted (or at least accounted for separately in topology analysis). There is a clear example by Lorenz. The studies of fractures in central Texas related to the SSC cite are another. In both of these examples, extracting the meaningful fractures to analyze has major practical implications for the usefulness of the analog study.

On the static versus dynamic, here you must mean 'on an engineering time scale', inasmuch as over geologic time scales all of these features are 'dynamic' as fractures grow and interact. But even on the engineering time scale how can you be sure that, for example, connectivity and length are static? And there is good evidence that in some reservoirs apertures are not particularly dynamic. So, while the static versus dynamic aspect is useful to think about, it seems like a red herring here where you point is that many of these extended attributes are difficult or impossible to measure in the subsurface with the kinds of probes we have. Do you need the static versus dynamic distinction later? Maybe better to omit and make the table about attributes you desire to measure.

64-66 This account of what can and cannot be measured could be more nuanced and do a better job of setting up what your paper contributes. Some aspects of fractures can be measured on the meso scale, like strike and dip, aperture, some aspects of abundance, and if the wells are deviated 1D spatial arrangement. That's not the same as 'cannot be measured'. The elements that can't be measured are length, height, and connectivity.

68 'fractures are not always…'

86 'fracture state'

90-109 This commentary on DFNs seems out of place here. Maybe it belongs in the Discussion. DFN's are something you build once you have information about the fractures (however incomplete) and so topically it seems out of place in a lead up to ways to improve characterization.

99 Is a fixed height/length ratio realistic? Doubtful.

111 Here and elsewhere where you mention 'in detail'; note that this is a vague usage. Omit or mention a scale range.

116-118 This is vague. I'm not sure what you mean. What are 'traditional, direct…' surveys? Outcrop fracture trace maps via surveying instruments and film have been acquired since at least the 1980s and although those methods are certainly slower than DOMs they may not be less accurate. Are you trying to say that previous studies of fracture statistics from outcrop that don't use your method are unreliable? If this is your point, then the comments belong in the Discussion after you have demonstrated this.

114 For comparing outcrop and subsurface, I don't think you want to say they 'underwent the same geologic history' since outcrops and subsurface targets by definition have different geologic histories, and the differences could have a material effect on what fractures are there. Uplift, contraction, weathering and a bunch of other processes must differ from the target to the outcrop. See English, 2012, Engelder, 1985, and Eppes et al. 2019 for discussions of various aspects of these differences. Peacock 2016 and Elliott et al. 2015 have text that describes how the inevitable differences can be accounted for. The reality is that outcrops and targets will always vary in important ways and one of the steps in a 'rigorous workflow' needs to be collecting data that will allow the nature of these differences to be identified. That way, the applicability of the analog can be judged, and, in some cases, the outcrop patterns can be adjusted to match the subsurface situation (as in the example in Forstner and Laubach, 2022, a study you cite).

116 I don't know what you mean by the phrase 'unavoidable as it is limiting' and how does the 'traditional direct geological and structural field survey' differ materially from flying the outcrop with a drone? In any case, 'relative chronology' if you mean crossing and abutting relations can be estimated from images and 'mineralization/filling' is best done in the laboratory with a thin section. And if by 'geometrical datasets with traditional techniques' you mean fracture trace maps, this was accomplished in the past for large outcrops without drones or digital outcrop models (see Barton from the late 1980s) although no doubt current technology makes collecting such information easier. So these sentences need adjustment so as not to be misleading.

Either way, access to advanced image collection methods do not solve two big limitations the use of outcrop fracture tracer mapping: the finite size of most outcrops and the potential for fractures in outcrops to be unrelated to the subsurface. These caveats ought to be mentioned.

Provide a definition of what you mean by 'traditional direct geological and structural field survey'.

130 The actual scope of the paper needs to be clarified here. And mention what parameters you are leaving out.

132 Define the 'traditional direct field…'

135 Is it really correct that factors such as connectivity are not incorporated in DFNs? Some DFNs can incorporated aperture size variation. See papers by Sweeney et al, for example, 2023.

137 Awkward wording: 'minimizes assumptions at each step'.

138-146 This would be more compelling if you could state your specifics claims: 'here we show that…'

163, 425, 638 Describing and classifying height patterns should be a step in outcrop network description. There is a useful height classification in Hooker et al. (2013. J. Struct. Geol.)

175 What do you mean by 'distinctly younger'? This makes it seem like you can detect a gradation in age, but all you have is an abutting relation but no information about how much younger. Geomechanical modeling shows that such relations can arise in a single deformation sequence or can reflect much longer times. Check the text and remove such unjustified qualifiers.

181 Forstner et al. 2025 GSL Energy Geoscience Conference Series, v. 1 explicitly investigates the effects of such patches on length distribution statistics. A good workflow needs to establish and describe how continuity across these features is treated.

183 'drowned by artificial fractures' is causal and vague. Can you restate this?

249 I think the text here could be confusing. You mean the best parameter to extract information from your image, but the way you put it ('most important parameter for applications in structural geology') it sounds like a geologic or fracture parameter and the reader asks The most important parameter for what? Most important probably depends. Is this qualifier even needed here? The remark seems more appropriate for the Discussion, where it can be defended.

This sentence for definition of 'surface density' could be clearer. I take it that this is an imaging parameter but it could be read as a fracture abundance measure. If what you mean by this is 'where the most fractures are' at least in terms of fluid flow there are examples in the literature where production data shows that numerous fractures does not correlated with, for example, high fluid flow (Wang et al. 2023, Marine & Petroleum Geology).

270 Add graphic bar scales to the outcrop images.

424 This sounds like a serious problem.

503 'trace connectivity' and their possible effects on 'fluid flow' are two different things and ought to be more carefully separated in your description. Fully connected traces may not imply any enhancement in flow (for example, if the traces are faults or sealed fractures); disconnected open fractures can enhance flow if the host rock is permeable. Please be more careful in how these parameters are portrayed.

510 Include contingent nodes in nodes list. These kinds of nodes go back to at least Barton and Hsieh.

515. I think you mean 'hard to recognize'. Since many fracture arrays grow by linkage such connections my be common; and in fact, most opening mode fracture traces have evidence at a range of scale that

they are made up of end-to-end links. See papers by Olson and Pollard and Lamarche et al. If you increase the image resolution, single traces bounded by I-nodes may reveal numerous low angle Y nodes (see Forstner et al. 2025, their figure 6c.

516 ‘C-nodes’, which you mention later, ought to be listed here. They certainly reflect the same level of abstraction as these other node types.

519 By ‘external processes’ do you mean ‘the size and shape of the outcrop’? Why not just say that? It’s less obscure.

540 This assumes that all the fractures are open. It’s one thing to talk about traces and how connected thy may be, but it’s quite a jump to assume ‘percolation’.

594 Omit ‘very’ as vague. This interpretation of relative ages by abutting and crossing is confusing. Crossing relations and abutting relations amongst have the same implication for relative timing: the abutted fracture is older and the crossed fracture is older.

605 Where do these patterns fall on a height classification scheme?

641 ‘rely’?

641-642 Hel the reader by succinctly defining what these distinctions mean.

562-564 This section of text could use some clarification.

650-655 The data sets are still constrained by the size of the outcrops. Some early studies captured complete fracture inventories within large and clean outcrops (e.g., Barton, others). So these claims about ‘massive’ data sets seem like they are missing key elements of the problem.

660-663 But this does not solve the conceptual problems of measuring lengths as laid out for example in Ortega and Marrett 2000.

666 These size/resolution issues can affect length distributions. This is obvious when you collect fracture information at different scales, and it follows from the segmented character of most fractures and the tendency for fracture length to grow by linkage. See the example in Forstner et al. 2025 where drone, hand held LiDar, and scanline datasets cover the same fracture array.

In any case, saying that you know where your data are truncated is not the same as being able to claim that it can be safely assumed that truncation bias is not affecting the dataset.

672 It might be random censoring. But it’s useful to wonder whether where you have continuous outcrop and where not is likely to be random, given that in many environments plants (and cover) may be localized in fractures (or in one case I know of, where the wide fractures are). It’s a geomorphology and vegetation issue that should not be lost sight of in fracture trace collection; see the comments in Eppes et al.

680 See the Discussion in Forstner et al. 2025 of this problem. If what you are doing is just connecting traces that look coplanar across outcrop gaps, that is problematic.

737 Here and elsewhere you can omit ‘very’; it is vague and not needed.

835 ‘implies’

849 The true scope of the workflow needs to be stated here and in the Introduction. What you are describing is really only part of a comprehensive workflow for describing fractures.

855-858 So how did you accomplish this filtering? This is a general problem, and not just restricted to outcrops in quarries. Spurious near surface fractures can be in regular sets. See the protocols in Eppes et al. and in the older literature.

865 The claim ‘complete characterization…’ of ‘fracture network parameters’ seems overstated and vague. What parameters? Do you mean ‘heights and correlated lengths’ and associated connectivity patterns? In sedimentary rocks the bed-normal patterns of heights are commonly quite different from length patterns. Heights may or may not be correlated with stratigraphic features that (depending on depositional environment, etc) may be on a meter or less scale, whereas sedimentary boundaries have been shown to affect length patterns but on a longer scale (again depending on the scale of the sedimentary features). An outcrop trace pattern example of the later is in Geol. Mag. 2016, v 108., p. 135, their fig. 2. This is one reason why classifying height *patterns* (i.e., height relative to stratigraphic features) should precede drawing conclusions about height/length relations.

866 ‘only one of these two data sets’ is confusing. It’s not clear what ‘datasets’ in the cited references that you mean. Do you mean ‘that rely on data collected on either bed-parallel outcrops or in cross section’?

859 ‘very high quality’ is vague; omit ‘very’; and ‘perfectly exposed’ seems to contradict your Conclusion, where you note that there are gas in the outcrop. Even the best outcrops have covered areas and exposure gaps, although there are some that are quite large and clean.

872 Is this typically done? I don’t think this is the protocol in Healy et al.

874 Awkward. ‘…huge areal extension…’ > ‘xxxx m^2 extent’ (provide an area rather than the vague ‘huge’ and it’s ‘extent’ not ‘extension’

866 On the other hand, some of these techniques provide information on key parameters that you do not address. Ortega et al. for example, provide aperture data over wide size ranges, but you apparently do not collect any width information so how can your technique be portrayed as ‘a complete characterization of fracture network parameters’. Your contribution can stand on its own without overselling it.

870 But you ought to note the limitations; for example, your DOM is all at one scale. What happens if you collect fracture data over a range of scales and resolutions (e,g,. Forstner et al. 2025; Elliott et al. 2025).

911 The criterial is scale and diagenetic considerations. The latter point relates to whether the fractures are open.

906 It’s not clear what your point is here. The text needs to be thought through a bit more. The contrast you draw between Sanderson and Nixon and Forstner and Laubach I think is ill posed. The problems with traces in 3D were already discussed by Ortega and Marrett 2000. The problem of *open* fracture continuity for opening mode fractures identified by Forstner and Laubach would be equally valid if you

could somehow see the 3d shape of fractures; this is not an artifact of looking at a 2D surface. The issues you need to address are on the most general level, that fractures intersecting a 2D surface (or fractures in 3D) may or may not be open. In other words, the fracture maps (and all the parameters derived from them) may be unrelated to fluid flow (or strength). Consequently, the only way to go from the outcrop analog fracture map to any kind of fluid flow estimation needs to account for whether the fractures are open. This can be done by simply assuming they are all open, as is usually done. Such an approach can lead to insights, but it contradicts geologic observations to say that the maps and derived parameters are all you need to estimate flow. Other geologic evidence can and should be brough to bear. The geologic evidence shows that what fractures (or parts of fractures) are open depends on scale (fracture width) so some fractures (or parts of fractures) may be sealed, so this information can be used to make trace maps more informative for flow parameters. Other geologic elements could be in play. For example, the 'network' might be a mixture of opening-mode fractures and faults.

943 On the other hand, Forstner et al. 2025 make the case that outcrop trace maps, however collected, always overestimate length.

962 Awkward. 'allowing to define' > 'defining'

966 The text starting "For these reasons a step behind…" is confusing. Clarify what you mean.

981 'every relevant parameter' is overstating this case, as you have not characterized the distributed aperture distribution or where the fractures are open versus not open.

984-988 Are these major conclusions? The first bullet item is no news to anyone who has measured fractures in outcrop and if this is a conclusion, the Introduction does not do a good job of setting up or prefiguring this point. Also, if this is an issue why not discuss the solution for describing and dealing with incomplete outcrop patches suggested by Forstner et al. 2025?

986-988 This statement does not seem to be posed as a Conclusion.

991 Where was 'tectonic related fracture' sets introduced? Why not stick to descriptive terms? How do you know that any of the sets are related to tectonics? I don't see anything in the MS that demonstrates when and why these fracture sets formed (and is it even relevant to a characterization workflow?).

1255/1260 Why are the article titles formatted differently? Make these uniform.

1265 Check the author's name.

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