The submitted work presents a method for the automated detection of fracture planes from 3D remote sensing data (i.e., terrestrial lidar, digital photogrammetry) based upon region growing using mesh topology to define the search kernel and angular deviation of neighboring mesh elements to define the stop criteria. The authors also present a semi-automated workflow to process and analyze the extracted planar facet dataset to computed key fracture network properties, such as orientation and set distribution, fracture size and fracture intensity measures (i.e., linear, areal and volumetric intensity: P10, P21, P32). The authors are to be commended on undertaking an ambitious objective in the development of automated fracture feature extraction and analysis tools, which can be of major benefit to the community, potentially negating the need for time consuming manual surveys. However, the presented manuscript contains several major flaws which should be addressed in order for the work to be suitable for publication. I therefore recommend that the authors undertake major revisions to their manuscript addressing the following concerns:

Thank you for your useful comments on the paper. We appreciate your interest in our work. We will rework the document following your recommendations. A specific modification plan is detailed below.

- The way the work is presented in terms of prior arts is disingenuous to the existing literature. The earliest automated fracture characterization studies typically focused upon planar extraction of fracture facets (i.e., the current work) yet the authors present the study as though it is groundbreaking in this regard. Some of the earliest approaches are ostensibly similar to the one presented here (i.e., using mesh region growing based upon angular deviation): see Turner, A.K., Kemeny, J.O.H.N., Slob, S.E.I.F.K.O. and Hack, R.O.B.E.R.T., 2006. Evaluation, and management of unstable rock slopes by 3-D laser scanning. IAEG, 404, pp.1-11. The authors need to give the literature its due, framing the novelty of their work in this context. Omitting pertinent literature, some of which has been published for nearly two decades and is very well known within the community is completely unacceptable (I have placed some suggested references in the edited .pdf uploaded with this review).

We agree with your comment on the literature review for 3D fracture recognition. We will completely reword the introduction to include our work better in the scientific context. In addition to the requested changes, we will endeavour to incorporate a discussion that compares our workflow with existing tools available for LidAR data treatment in the context of fracture network investigation and quantification, for reservoir characterisation purposes.

- The description of the authors' methodology in their automated fracture analysis is confusing and difficult to justify or replicate in its current state. I find some of the calculations to be overly complex (for example the authors' calculation of mesh area using vectors in convoluted without justification).

We will rewrite the methodology and explain the workflow, particularly regarding the literature as commented above.

P21 is described in the text as fractures per unit area which is incorrect.

This was a mistake during the paper redaction, it will be fixed in the next version.

I find the computation of P32 to be particularly problematic: why do you use smoothed and unsmoothed surfaces to define the investigative volume for P32? Surely this results in a thin and uneven volume of interest? How do you use single integrals in two dimensions to approximate the volume between the two surfaces? Surely for this to be valid you would need to evaluate the separate integrals between the two curves of intersection at regular intervals (this is not clear from the text)?

Even if we play devil's advocate and accept this analysis there is a fundamental flaw in using apparent fracture surfaces to compute absolute volumetric intensity measures such as P32, as the apparent values obtained are curtailed due to censoring related to both the orientation of the outcrop vis-a-vis the fracture system and the preference of smaller fractures to be censored by the finite window of observation provided by the outcrop. These are first order considerations in fracture analysis which have been ignored. See Priest (1993) and Wang (2005) for discussions (references in the attached .pdf). Thus, you are only calculating 'apparent' P32 at best which is inappropriate for fractured rock mass modelling (i.e., via DFNs).

- Leading on from Point 2, in the results section the authors are overly optimistic about the suitability of their fracture properties for DFN model conditioning due to an overly simplistic view of the derivation of volumetric fracture properties for quasi-2D data (i.e., outcrops). For example, it is conceptually broken to believe that their obtained length distributions are appropriate for DFN model conditioning, as the trace distribution does not fully reflect the underlying distribution of fracture SIZE: as (1) Traces may represent the true diameter of a fracture but are more commonly a chordal length of a larger disk, and (2) trace lengths are right skewed due to the low P of intersection with smaller fractures. This issue can be dealt with using analytical solutions (see Priest 1993) or forward modelling (for example Golder Associates FracMan software has tools for this). In short, you have not fully appreciated the scale of the problem of estimating 3D fracture size from outcrop: a problem that has been dealt with for four decades in the rock engineering literature. This issue is mirrored in the authors' belief that their P32 estimates constitute appropriate input for DFN modelling. As alluded to in Point 2, this is not conceptually valid. The reason why Wang's (2005) work which the authors dismiss on P32 correction factors exists is because you cannot estimate P32 from the exposed network without considering the occluded component of the network related to stereological effects.

About the integral, it is clearly stated that the integration is approximated using the trapezoidal rule twice in both x and y direction. We must admit that presenting the general trapezoidal rule with x is misleading, we will change eq 3 and fig 4 to be more clear. We will also state more clearly that this is a broad approximation as we assume change in z axis to be negligible (which is in reality not the case).

We totaly agree about the fact that our test on P32 calculation is an « apparent » one and not the true P32. We are aware of the corrections from P10 to P32 that exist for the transposition between scan lines data and volumic quantification for DFN modelling. We also agree with your remarks about the trace lengths and fracture intersections that affect the estimation of fracture size distribution.

However, with our P32 calculation, we wanted to try to estimate the fracture density without using a digital scan line for P10 calculation, a technique that as you know also has its limitations and statistical biaises. Although there are also orientation-linked sampling biases in comparison with the 3D rock mass, the calculation we proposed based on this slice of the rock media encompasses a larger representative volume than scan lines, thus reducing the sampling bias. Indeed, if the outcrop is curvated, orientation biases will be less effective for automatic fracture detection than a 1D scan line. This sampling bias is inherent to the outcrop nature, and for statistically robust fracture network investigation, this approach has to be performed on numerous outcrops in which structural framework is quantified. We will precise and remind these guidelines on properly characterising quantified fracture network properties, as it was not clear enough in the previous version of the manuscript.

We admit that the direct use of this P32 calculation for DFN modelling might be perceived as too optimistic at this stage of our workflow development. We will completely reword this part of the paper and include the discussion about these limitations and warnings you mention with our approach. We will specify how to interpret this density calculation, and how it can be compared to P10 computed on the digital outcrop and P10 from the field, and how to use these values in DFN modelling in the framework of permeability range estimation for fractured porous media.

For fracture length characterization, specifically, we will rephrase unclear sentences. We will clarify that this obtained dataset needs to be calibrated to get real fracture length, if necessary, for users' further use of such extracted datasets. Our innovative approach provides a more realistic and time-sparing approach for fracture network properties characterization, either from LiDAR data preprocessing or in comparison with manual digitalisation tools.

- There are numerous issues with the organisation and presentation of the study, including informal or incomprehensible English, missing references or inaccurate technical language / descriptions. I have detailed these issues specifically within the attached .pdf file.

Thank you for your time and constructive remarks, for the detailed suggestions in the attached PDF file outlining specific concerns. In the revised version, we will carefully address and rectify the issues related to organization, presentation, language, references, and technical descriptions. As recommended by the other reviewer, we will deeply reorganise the introduction to better integrate the literature about automatic fracture recognition, simplify the geological context and better describe the workflow. The results will be simplified and shortened to allow more place for comparison with the field data. Finally, the discussion will be enhanced by replacing our workflow in the scientific context.

Regards.