In response to reviewer 2's numbered points and general comments.

1) There is a general lack of guidance on specific use of the observations and conclusions of the scaling laws for sub-surface reservoir models: results in fig 8 span multiple orders of magnitude (8), but with a degree of uncertainty. Fig 13 attempts to address these points, but the text is lacking a discussion on utilization of these findings for subsurface reservoir scale fracture modeling. The authors did state that this is one of their main drivers for this study, thus a more elaborate discussion is warranted.	1)It was not our aim to provide guidance on the specific use of the observations. We aim to show here one way in which the observations might be applied to an example reservoir (we used Clair). We have revised the introduction and discussion and hope this is now clearer.
2) Although specified within the MS, confusion remains on how faults and large lineaments (regional scale) can be used to infer length / aperture in subsurface or micro scale. More discussion is needed. I would have liked to see the analysis of shear fractures separate from opening mode fractures (joints). It gives the impression that, if we can map faults from seismic, we can infer the attributes of joints system. Often the two are not coupled as faults do form associated fracture systems within their damage zones, but this happens independently from pervasively distributed joint systems throughout the reservoir. More discussion needed on why the shear and opening mode fractures are treated similarly over multiple scale of observations.	2)We have explicitly addressed this issue in the revised paper in the following ways A)We make it clear that the attribute analysis is focused on the Group 3 structures. These include both faults and opening mode strcutures where we observe them in outcrop. At our Dounreay location, faults with metre-scale contained the same mineralisation as opening mode fractures. They clearly contributed to the flow in the subsurface. B) In the discussion we make it clear that the assumption that the extent to which the scaling of fracture aperture attribute to the

	regional scale structures needs to be tested.
3) Each fracture system is unique. Even fracture systems in similar host rocks andtectonic regime could vastly differ in their attribute distribution, based on local variations of geological factors. The fracture systems reflect details in the geological hysteresis, and are sensitive to local variations of many geological aspects: local stress field, pore pressure evolution, chemistry, strain rates, diagenesis, geochemistry, etc. The claim the Caithness outcrop is a valid analogue for sub-surface fractured reservoirs like Clair Field is a fair statement, and I don't disagree. But it needs a bit more attention to understand the differences and similarities. The MS tries to convince the reader that the analogue is appropriate, but minimal evidence is provided on why that claim has been made.	3) We have addressed this directly in a new section (2.2) that has been added to the paper during the reorganisation which explains clearly the basis for the choice of analogue.
In addition, the MS would benefit from a discussion on outcrop vs sub-surface fracturing processes in general. Outcrops often are saturated with fractures, as existing fracture systems get enhanced (saturated) during exhumation processes. In this example, I expect the effect of enhancement due to exhumation to be significant at the studied coastal sections. It deserves a discussion on what assumptions have to be made to assume the outcrop dataset and its scaling relationships over multiple scales, are valid to use in sub-surface modelling efforts.	We only measure the Group 3 faults and fractures that we know formed in the subsurface before exhumation
To apply this technique for a different reservoir, one would need to find compatible outcrops that allow a similar sampling of micro /meso / regional scale fractures as analogue for the reservoir. That might be a difficult task. The MS would benefit from a more elaborate discussion on the use of outcrop analogues for sub-surface fracture systems, and guidance on how the resulting scaling relationships can be used.	Finding compatible outcrops is a good point in applying this method elsewhere. It is out of scope for this manuscript to review the use of outcrop analogues for sub-surface fracture systems. That is a very large topic.
4) Paper is missing a discussion on what is assessed as the "length" of the fractures measured at different scales. The length of a fracture as it grows, is different than thelengths that defines fluid flow pathways through connectivity. There is a scale dependency of observation here, that drives the measured length. For example, a single trace on the bathymetric data (Meso scale) might be mapped as several segments on the Macro scale. For example, quality control of fault maps often utilizes fault length vs displacement profiles to identify faults that are mapped with lengths too long compared to their offset, suggesting the fault likely consists of multiple shorter interconnected segments. A discussion on this during multi-scale analysis of datasets is warranted and currently missing. This could be addressed in the discussion near line 123-128. Suggestion: if available, please show the regional scale fault lengths vs displacement to ensure consistent relationships exist, and thus the proper/meaningful fault lengths are recorded.	We do not have displacement data for the regional datasets as these are mapped on imagery. We have added some discussion of this limitation.

5) I am skeptical of the micro scale dataset. It seems like the data was collected from a single sample located within or very close to a major fault (Group 3). This very specific location is likely not representative for the background micro scale fracture set as it was specifically chosen based on its micro-fractured appearance. However, this sample ties the scaling relations in Fig 8 to 8 orders of magnitude. I would have liked to see several samples and thin sections at random locations. The micro-scale data is questionable in terms of expansion of the scaling relations. A discussion around this uncertainty needs to be included.	We acknowledge that this is a limited dataset but we include it as it provides an upper limit on intensity values at this scale. We have added discussion of this in the paper.
The MS would benefit from an expansion through additional plots / discussions tocompare observations to other multiscale studies. (for example, Hooker et al, 2014, and additional references listed in line 62)	We have added discussion of Hooker et al (2014)

Reviewer 2 - Line by Line comments

19) We have revised this sentence and removed the need for the distinction
24) We have revised the abstract to make the terminology consistent with how we use it throughout the paper.
37) We have completely rewritten the introduction with many more references and a more nuanced discussion of the causes of fracture heterogeneity.
62) We have added more discussion of previous multi-scale studies
78) Section has been rewritten
80) Section has been rewritten
82) We have added this point to the revised introduction.
101) We have simplified this sentence
105) We have added this reference and cited it at this position in the text.
131) No – but we have clarified this sentence in our revisions.
177) We accept the reviewers point and have reordered the sections – placing Geological Setting before the methodology. We have reordered the figures appropriately.
189) A new Section 2.2 has been added to cover this point.
258) No. the fault lengths cannot be verified in the field due to the limitations of the exposures. We have clarified that we mean here the lineaments were verified to be natural features and not anthropogenic
279) See Reviewer 1 reply. We have clarified the evidence for this.
290) See reply to Reviewer 1 – we have added 3 sentences to acknowledge this limitation
389) Because we are measuring kinematic aperture these fractures are not ‘open’. We are quite convinced the apertures have not been affected much by the exhumation process as they formed in the subsurface and were partially to wholly mineralised. We purposely avoided joint systems as these may well be exhumation related

467) We have added some text to the revised version to explain the significance of the box counting dimension and say that the values obtained are in line with previous studies. The box counting dimension is generally not sensitive enough to distinguish between different fracture populations unless the are radically different.
568) As stated in the text this was observed and recorded by Franklin (2013)
602-612) We have replaced ‘fault’ with ‘structure’ in this paragraph
634) Done
664-665) corrected
672) We accept this point and have deleted this section
Figure 3) corrected missing labels
Figure 8) We have made this figure bigger