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Interactive comment on "A new methodology to train fracture network simulation using Multiple Point Statistic" *by* Pierre-Olivier Bruna et al.

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Dear Stephen Laubach

First let us thank you very much for the very detailed and highly valuable comments you left on our paper. In this letter we tried to reply to all of your comments as precisely as possible. We hope that our answer will satisfy you.

Overall quality: This is potentially a valuable contribution on the topic of understanding fracture networks. Outcrop fracture studies are being revolutionized by the rapid acquisition of fracture patterns from drones and photogrammetry. Developments in statistical approaches to process these observations are needed. This paper makes a credible contribution on the statistical front. And the written presentation and illustrations are

fairly clear and compelling. I do think that there is room for improvement to increase the impact of the paper.

Specific comments C1:In the presentation encompassing figures 3 through 5, I didn't completely follow how you defined 'fracture facies' and 'elementary zones'. Is there some sort of statistical measure of deviation from random you used (as in, for example, Marrett et al. 2018). Or are the 'facies' just qualitatively identified as 'looking similar'? My apologies if I just missed the explanation.

We decided arbitrarily that the facies would be defined in regard of fracture sets (based on orientation). The facies can eventually be made more complex if the user wants for instance to separate different length of fracture within the same set of fracture. The elementary zones are based on the variability of fracturing intensity per set within the outcrop. This analysis is possible because we have access to the final network that we consider as the "reality". In this case, when we observed a drastic change in the network geometry we placed a boundary around this area and this boundary defines an elementary zone. For instance, EZ1 contains mainly the NS (blue) and the NW-SE (red) fracture sets. On the contrary EZ2 contains mainly EW (green) fractures and EZ3 mainly NS fractures. EZ4 and 5 represent patches where the fracture density is higher. You can see the new density maps in the revised manuscript.

In the manuscript this is explained first in part II.3, probability map. We there the following paragraph The PM comes from a simple sketch (i.e. a pixelated image) given by the MPS user. It is based on the geological concepts or interpretations that define the geometry variability over the simulated area and that allow a partition of the outcrop. In each of the zones defined into the area of interest, the simulated property will follow the intrinsic stationary hypothesis (citation) but the entire domain will be non-stationary. While working on outcrops, the partition of the area of interest can be determined based on observations. For instance, when the fracture network interpreted from outcrop images is available, the geologist can visually define where the characteristics of the network are changing (fracture orientation, intensity, length, topology) and draw limits around zones where the network remains the same. This technique was used in the present paper. However, outcrops or subsurface may lack of continuity between observation sites. If the data are sparse and come mainly from fieldwork ground observation or boreholes, the use of alternative statistical approaches can help to provide a robust and accurate partition of the area of interest. The work of Marett et al., (2018) interprets the spatial organisation of fractures using advanced statistic techniques such as normalized correlation count and weighted correlations count, on scanlines collected in the Pennsylvanian Marble Falls Limestone in the United States. In their approach, the periodicity of fracture spacing (clustering) calculated from the mentioned techniques is evaluated using Monte Carlo quantifying how different from a random organisation are arranged the fractures in the investigated network. These approaches can be highly valuable during the process of building a probability maps when less data are available. The probability maps provide a large-scale. . ..

C2:The abstract reads too much like an Introduction. This part of the text needs to be more information rich. Instead of saying the paper proposes a multiple point statistics method, the Abstract should try to explain the specifics in a highly succinct way. Likewise, how was the method tested; don't just use a passive construction to tell the reader that the method 'was tested'. Bring forward some of the specifics from the Conclusions.

We agree on that point.

In the manuscript, the abstract was modified as following: Natural fracture network characteristics can be known from high-resolution outcrop images acquired from drone and photogrammetry. These outcrops might also be good analogues of subsurface naturally fractured reservoirs and can be used to make predictions of the fracture geometry and efficiency at depth. However, even when supplementing fractured reservoir models with outcrop data, gaps in that model will remain and fracture network extrapolation methods are required. In this paper we used fracture networks interpreted in two outcrops from the Apodi area in Brazil to present a revised and innovative method of frac-

ture network geometry prediction using the Multiple Point Statistics (MPS) method. The MPS method presented in this article uses a series of small synthetic training images (TI's) representing the geological variability of fracture parameters observed locally in the field. The TI's contain the statistical characteristics of the network (i.e. orientation, spacing, length/height and topology) and allow representing complex arrangement of fracture networks. These images are flexible as they can be simply sketched by the user. We proposed to use simultaneously a set of training images in specific elementary zones of the Apodi outcrops defined in a probability map in order to best replicate the non-stationarity of the reference network. A sensitivity analysis was conducted to emphasize the influence of the conditioning data, the simulation parameters and the used training images. Fracture density computations were performed on the best realisations and compared to the reference outcrop fracture interpretation to qualitatively evaluate the accuracy of our simulations. The method proposed here is adaptable in terms of training images and probability map and ensure the geological complexity is accounted for in the simulation process. It can be used on any type of rock containing natural fractures in any kind of tectonic context. This workflow can also be applied to the subsurface to predict the fracture arrangement and fluid flow efficiency in water, heat or hydrocarbon fractured reservoirs.

C3: The introduction could also use improvement. For one thing, the Introduction does not make a very coherent case for why outcrop studies of fractures are so essential. The reason isn't necessarily because fracture networks have 'intrinsic complexity' (line 65) "some networks are quite simple" but because the elements of fracture patterns that govern fluid flow, like connectivity and height and length distribution and the apparent clustered distributions evident in figs 3-5 cannot be adequately sampled in the subsurface. Some attributes like length distribution cannot be sampled at all in the subsurface. Outcrops are where these features can be measured. The Introduction would be stronger if it spelled out this challenge in clear, simple terms. It would also help if the cited literature included some more explicit examples of how these hard- or impossible-to-measure attributes affect fluid flow (for example, Long & Witherspoon

1985 on connectivity; Olson et al. 2009 on length distribution in unconnected networks in porous rocks). Right now the Introduction 'lacks motivation'. Many of the parts are there but the case needs to be made stronger. See some of the specific comments below.

We thank you very much for this comment. We will modify the manuscript accordingly.

In the manuscript Line 65: Despite the existence of these concepts, a range of parameters including fracture abutment relationships as well as height/length distributions cannot be adequately sampled along a 1D borehole and are mainly invisible on seismic images. In addition, fracture networks may present a spatial complexity (variability of orientation or clustering effect) that is also largely unknown in the subsurface. Long and Witherspoon (1985) and Olson et al., (2009) showed how those parameters impact the connectivity of the network and consequently affect fluid flow in the subsurface. In outcrops the fracture network characteristics can be observed and understood directly. Consequently outcrops are essential to characterize fracture network attributes that cannot be sampled in the subsurface, such as length or spatial connectivity.

C4:Ok; the following might seem like a tangential issue. But generalist readers need to have a clear explanation of what problems there might be in using outcrop fracture patterns as analogs for those in the subsurface. In section I.2 about surface rocks as reservoir analogs, an incautious reader would never suspect from the text here that there might be problems with using outcrops fractures for this purpose. This omission needs to be fixed. Some outcrop fractures provide close matches to those in subsurface areas of interest (e.g., Gomez-Rivas et al., 2014) but others do not (e.g., Laubach et al., 2009). In many cases, outcrop fractures provide demonstrably misleading guidance for the subsurface (Corbett et al., 1987 and subsequent work on the Austin Chalk cited in Laubach et al. 2009; Li et al., 2018). Studies typically seek to omit fractures that result from near-surface processes unrelated to fractures at depth (Stearns & Friedman, 1972). But subsurface sampling over the past two decades shows that in the moderate- to deep subsurface (1 km+) in sedimentary basins, many fracture pattern

elements differ from those found in more readily sampled outcrops even if the fractures in those outcrops formed in the subsurface, and for unsurprising reasons. Comparative studies in the same rock type and structural setting of fracture spacing observed in outcrop and sampled in long fracture-perpendicular cores shows that patterns in exposures can differ markedly from those in the nearby subsurface (Li et al., 2018, J. Struct. Geol.). The differing temperature-pressure paths of outcrops and rocks at depth and associated differences in rock properties are key reasons that the evidence outcrop patterns provide on fracture patterns in the deeper subsurface needs to be used with caution. The need for caution should be mentioned even if this particular outcrop is a good subsurface analog. Part of the process of using outcrop fractures is figuring out to what extent the outcrops are guides, and to what circumstances, of the subsurface. This part of the Introduction should acknowledge this issue and mention that the authors addressed it (I notice that later in the MS the outcrops are said to be good analogs; can the authors mention why?). I'm sure the authors recognize this issue and despite the length of my comments a brief but complete acknowledgment of the issue is all that is needed in my opinion.

We will also follow this advice as we are sharing the same opinion on analogues. The issue pointed in the second paragraph of this comment is also approached in the revised text proposed below.

In the manuscript after the line 86 the following text was added However, not every outcrops can be considered as good analogues for the subsurface. Li et al., (2018), in their work on the Upper Cretaceous Frontier Formation reservoir, USA observed significant differences in the fracture network arrangement in subsurface cores compared to an apparent good surface analogue of the studied reservoir. In the subsurface, fractures appear more clustered than in the outcrop where the arrangement is undistinguishable from random. The origin of these difference is still debated but these authors suggest that alteration (diagenesis) or local change in pressure-temperature conditions, may have conducted to the observed variability. The near-surface alteration processes (exhumation, weathering) may also conduct to misinterpretations of the characteristics of the network. In this case, one should be particularly carful while using observed networks to make geometry or efficiency (porosity, permeability) predictions in the subsurface. Therefore, the application of the characteristics observed in the outcrop to the subsurface is not always straightforward or even possible, and may lead to erroneous interpretations. Relatively unbiased signals such as stylolites or veins and particular geometric patterns might be a trustful basis to show that the studied surface fracture can be, to some extends, compared to the subsurface.

C5:The statistical approach seems like a reasonable one. But I think the paper would benefit from a clearer explanation perhaps aimed at a generalist audience, as well as featuring a compare-and-contrast with other similar approaches. I'd be interested in seeing a comparison with the Hanke et al 2018 directional semivariogram (J. Struct. Geol. 108 [March]). I noticed that the Liu et al. 2002 citation in your reference list is incomplete. [Liu, X., Srinivasan, S., & Wong, D. (2002, January). Geological characterization of naturally fractured reservoirs using multiple point geostatistics. In SPE/DOE Improved Oil Recovery Symposium. Society of Petroleum Engineers.] If you go to One Petro you can get the doi for papers like this one.

It is true that a lot of work has been done in geostatistics concerning the simulation of fracture network. For instance the work of Bruna et al., (2015 JOH) using two points statistic to evaluate the connectivity of fractured geobodies, the extensive literature using simple or sophisticated DFN (Fracman-type approach) or the approach you mentioned from Hanke et al., 2018 (appearing very interesting). However we believe that MPS is a bit apart in terms of algorithm but also in term of implementation. As we already stated in the new abstract, the goal here is to integrate more geology from scratch (even before starting the simulation) in a series of simple images which are in that respect much more flexible than an average value of density along a well for instance. This is why we did not extend too much on the comparison with other existing methods.

In the manuscript we believe that the new abstract and the part I.3 are sufficient to give to the reader a hint of what are the approaches classically used or used in the past and to present how different and flexible is the MPS approach. However, we added the following paragraph after line 106: Work of Hanke et al., (2018) uses a directional semivariogram to quantify fracture intensity variability and intersection density. This contribution provides an interesting way to evaluate the outputs of classical DFN approaches but require a large quantity of input data that are not always available in the subsurface. An alternative geologically-constrained method which i) explicitly predict the organisation and the characteristics of multiscale fracture objects, ii) takes into consideration the spatial variability of the network and iii) requires a limited amount of data to be realised would be an interesting and innovative way to represent fracture network geometry in various contexts. The second paragraph of the comment was modified in the reference list.

C6:I didn't find the analysis of aperture variation to really be much of a test and the whole exercise seems a bit extraneous to the statistical analysis of the pattern. The text needs to explain more clearly in what sense this is a test (even if that turns out to underline that it is a limited test). As noted below, it would also be appropriate to present the 'stress sensitivity' (or not) of fractures in a more nuanced way. Why no direct measurements of aperture size distributions?

We had similar comments from one of the other reviewer of our paper and we agreed to remove the part talking about fracture aperture IV.2. We admit that the position of this part is not adequately positioned and that the test has a limited impact on the validation of the method already given by a detailed sensitivity analysis. Concerning your last comment we did not took into consideration direct measurements of aperture size distribution for the same reason you mentioned earlier in the review (C4 alteration process). In fact veins are not available everywhere and in each considered sets. This is why the modelling approach appeared to us more robust.

In the manuscript, as already proposed in the answer to one of our Anonymous Reviewer we will add a small visual comparison of the P21 between the 5 models proposed in the figure 14. This part will be added as a III.5 before the discussion.

Technical questions & comments C7:30 Abstracts do not normally contain citations.

The citation will be removed

In the manuscript the new abstract will not contain citations.

C8:53 'Ubiquitous' means that fractures are everywhere but excavations and horizontal core studies show that some rocks in the subsurface lack fractures, or if fractures are present they are so widely spaced (hundreds of meters or more) that 'everywhere' is not an apt description. An outcrop example showing how resistant to fracture some rocks are is Ellis et al. 2012, J. Geol. Soc. London. A better word might be 'widespread'. Moreover, areas of completely sealed fractures are also common in the subsurface, and such fractures are rarely fluid conduits. Although I don't agree with people who don't count such rocks as fractured, it's certainly the case that some rocks lack fracture flow conduits.

We understand this comment and we will modify the phasing in the manuscript.

In the manuscript the sentence will be modified as following: Fracture are widespread in Nature and depending on their density and their aperture, they might have a strong impact on fluid flow and fluid storage in. . ..

C9:55 I think more caution is called for in citing for this point (effects of fractures on fluid flow). There are relatively few papers that document the effects of fractures on fluid flow in hydrocarbon reservoirs but many papers that repeat the contention that fractures are important for fluid flow. One of the papers that does quantify production data with respect to natural fractures is Solano et al, 2011 SPE Reservoir Evaluation & Engineering. However, although both of the papers cited here in the MS are interesting contributions, I don't think they are the right papers to cite in support of the point the

authors make. All of the references mentioned in his section of the text should be reviewed with this point in mind.

We think that the authors cited in this section are all dealing with fractures affecting subsurface reservoirs or surface reservoir-analogues in different contexts and they all seem to converge on the conclusion that fracture play a role (positive or negative) in fluid flow. We agree however that the paper from Solano deserves to be cited there in addition to the Agar and Geiger and Lamarche et al.

In the Manuscript the reference was added in line 55

C10:58-ca. 62 Ok, so maybe a quibble, but 'well known'? really? Maybe I'm not following what the authors are trying to say here, but connecting the specific strain and stress conditions to the formation of a given fracture or fracture pattern is full of uncertainty: the timing of fracture formation is commonly very challenging to estimate unambiguously and because fracture arrays are generally low strain phenomena and through geologic time a wide range of loading paths might lead to fracture (e.g., Engelder 1985, J. Struct. Geol.) the connection between pattern and cause is frequently ambiguous. A good example relevant to this paper is fractures in outcrop. Did they form due to some process at depth (for example, elevated pore fluid pressure) or during uplift or exposure? This issue gets to the reliability of outcrop-derived fracture pattern information (which I'm all in favor of obtaining) but the challenge of determining the causes of fractures I think needs a bit more thoughtful or nuanced treatment.

We admit that a strong shortcut has been made there and that the phrasing has to be revised. In our case fractures form in the response of loading. The differences in patterns between two outcrops AP3 and AP4 distant of about 2.5 kilometres are still debated. However Bertotti et al., 2017 bring some new answer to those questions.

In the manuscript the lines 58-62 have been removed and replaced by: These conditions have been used to derive concepts of fracture arrangements in various tectonic contexts and introduced the notion of geological fracture-drivers (fault, fold, burial, facies). Based on these drivers it is possible to some extents to predict reservoir heterogeneity. . .

C12:67 Do you mean stresses in the past when fracture patterns formed (paleo stresses)? You seem to be claiming that fractures are highly sensitive to current stress state. I know this is a widely accepted premise, but you should at least note that many reservoirs are known to have fractures that are stiff and insensitive to current stress state (e.g., Laubach et al., 2004, Earth & Planetary Science letters).

We were there talking about paleostress. However, this paragraph was modified according to the comment you made earlier.

In the manuscript: see the response provided in C3

C13: 71-86 This section needs to contain some caveats about the limitations of outcrop fracture research.

This issue was addressed in the general comments mentioned before and have been modified in the manuscript.

In the manuscript: see the response provided in C3

C14: 73 The use of outcrop fracture patterns to constrain the subsurface goes much deeper into the past than the recent references cited here: National Research Council 1996. Rock fractures and fluid flow: Contemporary understanding and applications. National Academy Press, Washington D.C., 551 p.

We agree that this reference is important and we will follow the advice of the reviewer without removing most recent citations.

In the manuscript: the reference was added

C15:81-82 The 'how, when, and where' is rarely obvious from the pattern alone. Flagging this comment is not off topic since it relates to how outcrop data can or should be used.

This was the goal of this sentence. A lot of interpretations is possible from collected outcrop data. This is why we put the word eventually in this sentence. We do not see specially what the reviewer means there.

In the manuscript we did not changed this sentence.

C16:89 'provide'

Changed

In the manuscript we modified "are providing" with "provide"

C17:93-94 This sounds like jargon; provide a clearer explanation of what you mean for a general audience.

We modified the text

In the manuscript the sentence was separated in two parts: The generated models follow a local stationarity hypothesis. This implies that the statistics used during the simulation are constant in the defined area of interest. . ..

C18:113-119 This is too late in the MS to introduce this material. Some of this could be in the Abstract.

We agreed on that and we changed the abstract as per the answer to comment C2

In the manuscript the abstract was changed

C19:125 What do you mean by 'full outcrops'. This seems vague. If you have a size range in mind, why not state it?

We were working on outcrops which sizes are in the order of 100m. The exact dimension of these outcrops is presented in table1.

In the manuscript the sentence was modified as following: ". . .geometry variability over outcrops (size order of 102m) and a methodology."

C20:135 I'm not sure I follow you here. You didn't measure any apertures in outcrop, did you? So is this just a process of a computation applied to both the outcrop imaged fractures and the statistical realizations? Why no measured outcrop apertures?

Some apertures were measured in outcrops but we did not used them in this work as we favoured the modelling part. We did not took into consideration surface fracture apertures because they were not representative of the subsurface conditions (weathering issues and exhumations). In any case the part on fracture aperture will be removed from the manuscript.

In the manuscript the sentence: "we computed mechanical and hydraulic apertures in outcrop fracture interpretation and on the obtained stochastic models." Was removed and replaced by "we computed density maps in outcrop fracture interpretation and on selected stochastic models."

C21:205-206 Some of the text here sounds like it is carry over from a proposal, since you've done the work.

We will remove "propose to" from the manuscript

In the manuscript we replaced it by "we used multiple training image"

C22:271 Does the karst figure into your aperture calculations?

Unfortunately not. But we believe that the karstification is due to "recent" surface alteration and will not be present in the subsurface as we can see them today in the outcrop. This topic is close to the one discussed in C4.

In the manuscript: no change applied except that the part on aperture was removed.

C23:322 This seems late in the text to have this kind of preview of goals?

We agree that this sentence is out of place

In the manuscript we removed line 332 to 324.

C24:365 Interesting. Are some of the >40-m-long fractures still censored by outcrop size?

Yes few of these fractures are censored by the boundaries of the outcrop. However a large majority of the fracture are included inside the pavement and we assume that they are representative of the maximal length of the fractures there.

No changes were requested in the manuscript

C25:572-575 There are some jumps in logic here. Yes, flow depends on open fractures. But whether or not fractures are open or not does not simply depend on in situ stress conditions. Some (many) fractures are insensitive to stress state (they are very stiff) and some are closed because they are mineral filled. It therefore does not necessarily follow that 'contribution of fractures to fluid flow: : :can be defined by the Mohr-Coulomb: : :' etc. The development here needs to be more nuanced and include a few caveats. It is also worth noting I think that the predominant role of aperture in fluid flow presumes a completely impermeable host rock, which is generally not a good assumption even for low porosity unconventional reservoirs (TGS; shale). If there is flow in the host rock and the fractures are not interconnected, open length distribution is what matters (Philip et al. 2005). Philip et al. varied the apertures in their simulations by a lot and got no significant difference in flow. Philip, Z. G., et al., 2005, Modelling coupled fracture-matrix fluid flow in geomechanically simulated fracture networks: SPE Reservoir Evaluation & Engineering, 8/4, 300-309.

Thank you for this comment and for the interesting reference you provided. However as this part was removed from the paper this matter will (hopefully) be addressed in a separate paper.

In the manuscript part IV.2 was removed

C26:576 'a key parameter'; if it's a key parameter, why were apertures not measured in the field?

As discussed previously we wanted to apply the aperture calculation into subsurface conditions so we did not considered fracture aperture measured in the field.

In the manuscript part IV.2 was removed

C27:622 'statistic'; is this the word you mean? Obscure usage.

We agree on this comment

In the manuscript the sentence was replaced by: "To Tackle these problems we choose to use multiple 2D MPS-generated fracture networks".

C28:625 What do you mean by 'aborted' fractures? Non-standard usage; suggest you pick another word.

We agree on this comment

In the manuscript the word aborted was removed

C29:632 Mechanical stratigraphy is readily measured in the subsurface; 'fracture stratigraphy' is more challenging. Did you rigorously describe your fracture height patterns for the outcrops (maybe it is in one of the cited references). Height patterns and fracture stratigraphies have different patterns. There is a useful classification in Hooker et al. 2013, J. Struct. Geol.

In fact the aim of this part was to provide a way to use 2D MPS in 3D. We showed our idea and we built a very simple 3D DFN based on our outcrop. The method is now much more elaborated and takes into consideration the issue you mention.

In the manuscript we had the following sentence after the Laubach et al., 2009 citation: The fracture height distribution, refered as fracture stratigraphy (Hooker et al., 2013) requires here a particular attention and is difficult to extract from borehole data. In outcrops, the use of vertical cliffs adjacent to 2D horizontal pavement should be a way to evaluate these height and to constrain the 3D model.

C30:637 'fracture family' is non-standard usage. Is there a reason not to call these groupings 'fracture sets' (Hancock, 1985)?

We agree on this comment

In the manuscript fracture family was replaced by fracture sets

C31:641 'provides'; ('The method provides a realistic: : :')

We agree on this comment

In the manuscript the suggested sentence was inserted

C32:Fig. 8, caption 'Fracture: : :'

We agree on this comment

In the manuscript fracture will appears with a capital "F" there

C33:Fig. 10. Some of the colours on this figure make it hard to read.

We agree on this comment

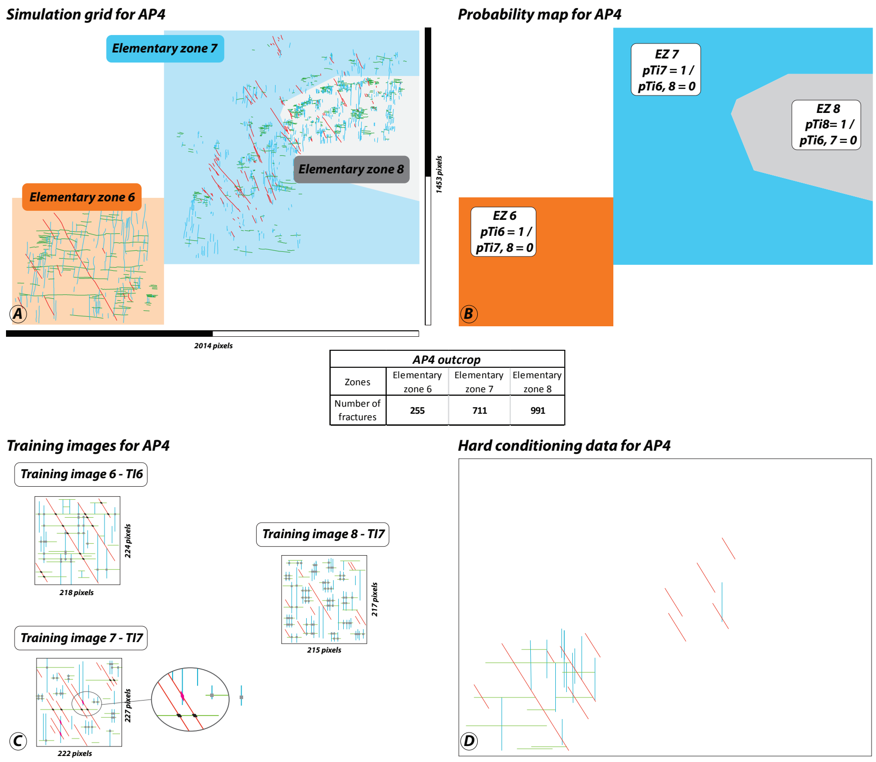
The figure was modified for the article

C34:Fig. 12 would be more informative with more labelling and explanation on the face of the figure. Add a graphic explanation/key.

We agree on this comment

The figure was modified for the article

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2018-106, 2018.



AP4 outcrop			
Zones	Elementary zone 6	Elementary zone 7	Elementary zone 8
Number of fractures	255	711	991

Fig. 1.

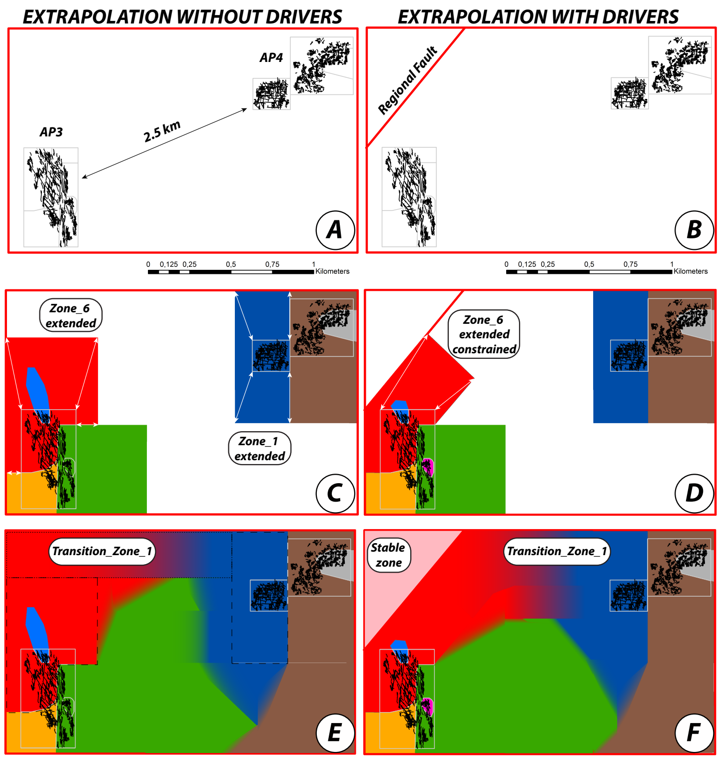


Fig. 2.