The manuscript by Hupkes et al. proposes a new approach of using kinematically meaningful fracture and stylolite associations in various analog outcrops to identify regionally persistent patterns of fractures that can be used to guide image log interpretation in wells and are relevant to geothermal exploitation in the subsurface. This is in principle an interesting topic that deserves publication but the manuscript requires to me significant revision before being possibly further considered for publication.

My comments below :

*Title : the first sentence of the title should be removed. They are plenty of examples of occurrence of fracture populations without any stylolites owing to lithology for instance. I do not mean at all I disagree with the idea of working on stylolite-fracture associations, just that the first sentence clearly designed to appealing purpose is wrong in many instances.

The title has been changed into: "A Fracture Rarely Comes Alone: Associations of Fractures and Stylolites in Analogue Outcrops Improve Borehole Image Interpretations of Fractured Carbonate Geothermal Reservoirs"

*The Introduction mixes different aspects in a sometimes non logical order, each of which requiring more attention as well as support by references beyond very general considerations. Overall, some reorganization is needed to clearly state the scientific question at hand and to clarify the aims of the study.

Introduction has undergone major revision. It has the following structure, to make the statements of this study clear in the Introduction:

- *Understanding discontinuity networks is important for geothermal exploitation of fractured reservoirs.*
- *Borehole images are an important tool for subsurface characterization of discontinuities but come with limitations. Note that we present this already in the second paragraph to underline that in the end, it are the discontinuities in the subsurface that matter.*
- *Analogue outcrops can be used to overcome certain limitations of BHI. To be a good analogue, the outcrop must meet several criteria, among which the shared tectonic/stress history.*
- *To understand the genetic origin of a discontinuity is essential for extrapolation in the subsurface to reservoir scale.*

- *There are several methods to determine paleostress. One is to group discontinuities into associations, following Hancock (1985), and despite its simplicity, it is barely used to link outcrop with subsurface.*
- *Statement of claims. ~45% of the features identified on BHI of the target reservoir in the Geneva Basin can be placed in the predicted framework of the background network.*

Also, the position of the Geological Background is switched with the Methodology, to improve the readability in the first part of the manuscript.

*L24 : Addressing the role of discontinuities in forming preferred drains or barriers should also rely on papers from outside the academic circle from the authors (e.g., from host rocks different from carbonate rocks, and mineralizations of other types than calcite; see for instance Grare et al., 2018, Minerals, among others). Also, the respective importance of diffuse fracture networks on rock permeability and fluid flow compared to seismic-scale faults has already been addressed (e.g., Beaudoin et al., 2013; Smith et al., 2022).

The references are added (line 20 and line 40-41). Indeed, there are many papers who describe potential impact of fractures on flow (either barriers or conducts), and we aim to refer to papers who demonstrate this in the subsurface, either in carbonate or siliciclastic reservoirs.

* L29-30 : The authors use orientation and relative timing information derived from observations of geometric relationships to gather faults, opening-mode fractures and stylolites into kinematically or mechanically consistent patterns over a wide area in a fold-and-thrust belt and its foreland basin owing to their development in flat-lying strata before folding (e.g., during the layer-parallel shortening stage, see Tavani et al., 2015; Lacombe et al., 2021). In contrast, the authors seemingly consider that fractures related to fold development are only of local significance. This may be true for fractures formed in response to strata bending at fold hinges, but some mesostructures observed in folded strata may be unrelated to the contractional event that caused folding, instead having originated in response to strata burial (Lamarche et al., 2012; Lavenu et al., 2013), foreland flexure (Mercuri et al., 2022), strata exhumation (Bellahsen et al., 2006a), extensional collapse of fold-thrust systems (Tavani et al., 2012) or to other pre-or post-folding tectonic events (e.g. Bergbauer and Pollard, 2004), and some may be of regional significance. The appraisal of the local vs regional significance of fractures therefore may be scale-dependent and should be dealt with more caution eventhough I understand the wish of the authors to simplify the topic.

We use orientation and discontinuity type to define discontinuity sets, and group these into associations if they are mechanically consistent (i.e. they might have formed in the same stress field). We prefer to refrain from using relative timing to define discontinuity sets, as we believe cross-cutting and abutment relations are often ambiguous, and contain little meaning if the two discontinuities formed in the same stress field that might have prevailed for several millions of years.

Regarding the timing of the formation of discontinuity associations with respect to the tilting/bending of the strata, we do not limit ourselves to only the layer-parallel-shortening phase, but to all deformation phases that occurred prior to this tilting and/or bending of the strata. With the methodology we propose, all pre-folding deformation events that fulfill two conditions will be captured: 1) at least two sets of discontinuities are formed that together fit in a discontinuity associations, and 2) the paleostress orientation must be consistent on a regional scale, i.e. on all sides of the basin which contains the target reservoir (see Methodology, line 144-146). The latter criterion defines for us what we mean with a 'regional' scale – i.e. on all sides of the basin that contains the target reservoir.

*Among the so-called local drivers of fracture development, faults deserve particular attention owing to the perturbations of the regional stress field they cause. The authors should have a close look to the works by e.g., Rispoli, 1981; Rawnsley et al., 1992; Homberg et al., 1997, 2004; Reiter et al., 2024. In particular, the works by Homberg et al. 1997 and 2004 are of prime interest to the study and deserve attention since they document stress perturbations and associated fracture patterns in the vicinity of major left-lateral strike-slip faults in the Jura (Pontarlier and Morez faults), keeping in mind that the area investigated is close to a very similar structure, the Vuache fault, which, on top of that is still seismically active. Again, this questions the scale at which one should consider the fracture patterns to be of local or regional significance in the kind of study carried out by the authors.

In this study, we focus on predicting the background network (i.e. diffuse discontinuities) in the subsurface, rather fault-related discontinuities. We by no means claim that the background network constitutes the complete network in the subsurface - moreover, we do not know if the contribution of the background network to flow in the target reservoirs is significant. It is more than likely that faults and discontinuities related to the stress perturbation around the faults influence locally the flow characteristics of the reservoir, but that is outside the scope of the current study. Interestingly enough, we do find discontinuity associations that are similar in orientation to those observed in the other outcrops (Parmelan, Jura) network close to the seismically active Vuache fault (see stations 32, 35 and 39 on figure 10). Therefore, we define them as part of the background network, that *formed prior to tilting of the strata. So even though the complex history of this fault, with several reactivation phases, the regionally consistent background network is still present within several 10s of meters away from the Vuache fault.*

*L28 and 83-84 : The definition of a set of fractures by the authors is a bit simplistic and should be based instead on a more complete set of criteria, including a common orientation in either raw or unfolded attitude, common relative chronology with respect to other mesostructures and shared mode of deformation (e.g., Pollard and Aydin; Bellahsen et al., 2006a; Ahmadhadi et al., 2008; Lacombe et al., 2011; Sanderson et al., 2024). Due credit should be better given to such existing literature, including the statistics behind defining distinctive fracture sets.

We used common orientation and mode of deformation to define the discontinuity sets that make up the associations. In particular the discontinuity mode is essential, as this is used for defining the discontinuity associations. It should be noted that in the end we are not interested in single discontinuity sets, but in the stress field in which they formed, because that is what we need to extrapolate discontinuities to the subsurface. To be more consistent on this point, in the Introduction, the definition of sets is replaced by a focus on the tectonic driver of discontinuities (line 40-50). In the Methodology, references for the definition of sets are added (Lacombe et al. 2011, Sanderson et al. 2024, see line 113). As mentioned above, relative timing is of subordinate importance for defining discontinuity sets in our study, as we focus on associations of discontinuities and the stress field in which they are formed, rather than individual sets.

*L29-31 : again, references should be provided from out of the academic circle of the authors : e.g., Bergbauer and Pollard, 2004; Casini et al., 2011; among others

These are indeed good references and added to the manuscript (line 42-43).

*L55 : That multiple discontinuity sets can form coeval in a single stress field is now nicely supported by absolute dating of syn-tectonic calcite mineralizations, which allows for a better appraisal of the local vs regional tectonic significance of fracture networks (e.g, Beaudoin et al., 2018; Parrish et al., 2018; Zeboudj et al., 2025). However, in some instances, the use of geochronology may either simplify or sometimes increase the apparent complexity of the fracture network analyzed on the sole basis of orientation and kinematic criteria. Geochronology may reveal very similar ages for veins of significantly contrasting orientations, which questions the perfect stability of the direction of the maximum principal stress during a fracturing event and the range of orientations allowed to define a fracture set considering possible stress variation at the scale of the strata or close to faults, in a time span lower than the few My of uncertainties of the geochronology technique. Conversely, absolute dating can, in some cases, complicate the interpretation of deformation history particularly when multiple distinct ages are obtained from fractures belonging to the same set defined on the qualitative criteria above. I would suggest much more caution with the hypothesis of constant principal stresses at the regional scale (L119).

We do not assume there is a regional consistent regional stress field, but the results show that in fact there is consistency with respect to orientation of σ1 that formed the discontinuity associations of the background network (see figure 10).

Geochronology in the form of dating calcite veins is indeed a very interesting way to constrain timing of the formation of the background network. It will give a minimum age constraint on the forming of the discontinuities, as the timing of calcite precipitation does not necessarily coincide with the propagation of a fracture. On top of this, we don't know how much time is involved in the creation of the background network. If we consider that they are formed under sub-critical conditions (e.g. numerical work on sub-critical crack growth of Welch et al. 2019, Olson 1993, 2004, 2007), it might have taken 10s Ma. Therefore, it is expected that different ages will come out for the discontinuity association. But in the current study, we don't have any absolute time constraints on the formation of DAs. That is why we focus on relative timing with respect to tilting/bending of the strata.

*L107-110 : the authors should be more careful when using the Andersonian stress hypothesis according to which one principal stress is generally vertical, especially in fold-and-thrust belts. As stated in the discussion paper by Lacombe, 2012 (section 4), mesostructures that yield one stress axis perpendicular to bedding while the other two lie within the bedding plane (e.g., bed-perpendicular joints/veins or stylolites, or Layer-Parallel Shortening (LPS) – related microfaults) yield a sub-vertical paleostress axis only after backtilting to their prefolding attitude (unfolding), which in turn implicitely leads to consider them as pre- or early-folding. However, it has been argued that such structures could have developed within tilted layers, hence possibly under a non vertical principal stress, if bedding anisotropy was able to significantly reorient stresses or if flexural slip occurred at very low friction so that the principal stresses rotated but remained either parallel or perpendicular to bedding (e.g., Tavani et al., 2015). A kind of circular reasoning may thus be involved when chronology is based on an Andersonian assumption only. As a result, the classical fold test must be preferred.

It is indeed an assumption that DAs are formed in Andersonian stress field, as mentioned in the Methodology section (line 136). It is outside of the scope of the current manuscript to quantify the orientation of the bedding at the timing of fracturing. We consider the simplest *solution the most plausible, i.e. they formed in Andersonian stress fields. In the Discussion, we discuss the possibility that the DAs are related to LPS (line 286-300). LPS will likely produce associations of discontinuities of reverse regime with a principal stress close to vertical to the bedding. However, for making the link with the subsurface, it is crucial to identify that these discontinuities formed prior to tilting of strata and related strain localization. That allows us to predict the presence of these structures in the subsurface of the Geneva Basin.*

*L292 : I suggest to change into : … predates the onset of localized deformations occurring during fold growth and late stage fold tightening.

Accepted. See line 295-297.

*Stylolites are expected to be important players according to the title of the manuscript and the approach is supposed to use fracture and stylolite relations, but the 'discontinuity' terminology used throughout the manuscript leads to overlook the role of stylolites. Poor use is made of stylolites and the most recent literature on stylolites is properly ignored (eg. Toussaint et al., 2018). Especially, it has been shown that the roughness of a stylolite brings some signal that can be treated and used to derive the state of stress prevailing at the time the stylolite ended its development. This applies to compaction-related sedimentary stylolites as well as to tectonic stylolites. In contrast to sedimentary bedding-parallel stylolites used to derive paleodepth constraints (eg., Beaudoin et al., 2019; 2020; see also the work by a senior author of the manuscript), tectonic stylolites the manuscript seemingly focuses on can bring very useful kinematic information for fracture analysis, beyond the classical papers (e.g., Marshak and Engelder). Tectonic stylolites can be either compressional or strike-slip in type (e.g., Beaudoin et al., 2016, 2020). In this case, the accurate interpretation of the stylolites in terms of stress is highly relevant and potentially predictive in fracture network studies. Adding one or two sentences on this topic may strengthen the 'stylolite' aspects of the manuscript which otherwise remains weak and nearly useless as is.

We added the references for stylolite as paleostress marker in the Introduction (Beaudoin et al. 2016, Toussaint et al. 2018, see line 53). Although it is a highly interesting topic, using stylolites to reconstruct magnitudes of paleostress is outside of the scope of the current manuscript, and therefore references are not included. It would indeed be an interesting next step to complement the current work with paleostress magnitudes, and compare if all outcrops experienced similar stress magnitude. However, for tectonic stylolites, this may *be challenging, as the assumption that σ2 and σ3 are of same magnitude is no longer valid, as for bed-parallel stylolites (see Ebner et al. 2010, JGR Solid Earth).*

In the Results section, when defining the regional events, we appreciate the fact that bed-perpendicular stylolites are indicative of a strike-slip or reverse regime (line 210-220).

More in general, the term discontinuity in this manuscript is chosen to include both fractures and stylolites. The point of this study is that discontinuities, being it fractures or stylolites, should be grouped based on the stress field in which they formed, instead of interpreting isolated features or sets, if to have added value for subsurface characterizing and extrapolation. Grouping of discontinuities is also important in the absence of stylolites: it makes no sense to treat a conjugated pair of shear fractures as two independent fracture sets belonging to different events when populating a fracture model. It can be seen in figure 8 that in some stations, tectonic stylolites are not used for defining an association.

*The role of fractures in fluid flow in unclear, especially the role of diffuse fracture networks compared to major drains (eg, seismic scale faults), see for instance Beaudoin et al., 2013. Also, the authors discuss in which situations fractures may enhance fluid flow, but all the field examples document sealed mode I fractures. In the vast majority of studies as well as in modeling works, the features conducting fluid are commonly assumed to be open fractures (e.g., Wennberg et al., 2016). Would the authors expect that in the subsurface, the fractures of interest (i.e., conductive) be open or only partly sealed while those in the outcrops are filled with calcite ? Or do they consider that already sealed fractures may be re-opened and made conductive under the current stress field ? It is unclear since authors speak about either mode I fractures or open fractures. It should be kept in mind that mineralization of mode I fractures and faults requires some specific chemo-physical conditions (see the synthesis by Laubach et al, 2019) unlikely to be met very close to the surface. Open fractures are more likely to be found close to the surface than under a certain overburden. In other words, the structural, burial and fluid flow history of exposed rocks and rocks in the subsurface may differ. This point requires an even brief discussion.

Accepted. A section in the Discussion is dedicated to this point (line 310-331):'

'6.2 Infill and aperture of discontinuities

The DA-method can be used to predict the geometry of the background network in the target reservoir, but is limited in extrapolating the aperture and mineral infill of fractures. The geometry is useful when considering stimulating the reservoir, as even the sealed discontinuities may create a strength anisotropy that will control the orientation and propagation of hydraulic fractures (Cao and Sharma, 2022; Rysak et al., 2022). However, for *predicting flow behaviour in the reservoir caused by natural discontinuities, modeling the aperture and mineral infill of discontinuities is crucial, as only (partially) open discontinuities might contribute to the flow. At the same time, outcrops should be treated with care when extrapolating these properties to the subsurface (e.g. Bauer et al., 2017; Peacock et al., 2022), also when the link between outcrop and subsurface is established with the DA-method. The timing of fracturing, emplacement of the infill and potential dissolution are important factors to consider when extrapolating these characteristics to the subsurface. On the Parmelan, for example, many small-scale (<10 meter) fractures of E1 and E2 are calcite filled (e.g. see figure 3). The diagenetic evolution can be used to constrain the timing of calcite cement formation in the outcrop (e.g. Lavenu and Lamarche, 2018; La Bruna et al., 2020), and subsequently provide insights how the aperture of these discontinuities can be modeled in the subsurface (Elliott et al., 2025). On the other hand, the large-scale fractures (> 100 m) of E1 on the plateau are currently conductive due to dissolution and karstification (see figure 4). It depends on the timing of fracturing and subsequent dissolution if the conductivity of these fractures can be used as an analogue for the paleokarst network that is observed on top of the Lower Cretaceous in the subsurface of the Geneva Basin (Eruteya et al., 2024). If E1 was formed prior to sub-aerial exposure of the Lower Cretaceous during the Paleogene, it is likely that they partially controlled the orientation of karst development. On the contrary, if the karstification on the Parmelan only occurred after the exhumation in the Pliocene, similarly dissolved fractures cannot be expected in the subsurface. So, in order to predict the aperture and if discontinuities are sealed in the reservoir, solely based on outcrops, the timing of fracturing and the diagenetic evolution of the formation are both essential to predict which discontinuity sets in the subsurface are likely to be conductive. Another possibility is to use borehole data to assess which discontinuities are conductive, and the DA-method can be part of the workflow to improve the interpretation.'*

The reference of Beaudoin et al. 2013 is added in the Introduction (line 40) as well as Laubach et al. 2019 (line 32).

*Like the physical properties (e.g., permeability) of fault rocks from exhumed ancient faults may hardly be extrapolated to the in situ hydraulic behavior of deep active faults because during uplift these rocks underwent unloading, decrease in temperature and changing fluid-rock interactions (including weathering), one can wonder whether fracture networks identified in exposed rocks are reliable analogs for the mechanical and hydraulic properties of subsurface fracture networks despite similar regional tectonic history and host rocks.

We added a section in the Discussion to touch upon this (line 310-331, also see above). In general, the 'goodness' of the analogy of the outcrop for the subsurface depends on what it *is used for. The DA-method is aimed to predict the geometry of the background network, and in this study we show why the outcrops we are used are a good analogue for the subsurface of the Geneva Basin (pre-tilting discontinuity associations that are regionally consistent in orientation). Extrapolating discontinuity aperture and infill from the outcrop to subsurface to assess hydraulic properties of the reservoir remains challenging (see e.g. Bauer et al. 2017, Peacock et al. 2022), but in the discussion we illustrate how the DA-method might contribute to this (line 361-385).*

*Since veins contain nearly pure calcite material compared to the host rock, one could expect some kind of strength change in the rock due the structural diagenesis, with the vein network causing possibly some strength anisotropy at the scale of the reservoir. Would it be an alternate parameter to usefully consider during exploitation ?

Yes, this is true, in particular when stimulating the reservoir with hydraulic fracturing is considered. It is added in the Introduction (line 21-22):

'Also, natural discontinuities impact the rock strength, which is essential to consider in the case of hydraulic stimulation of a reservoir (Cao and Sharma, 2022; Rysak et al., 2022).

and the Discussion (line 310-313):

'The DA-method can be used to predict the geometry of the background network in the target reservoir, but is limited in extrapolating the aperture and mineral infill of fractures. The geometry is useful when considering stimulating the reservoir, as even the sealed discontinuities may create a strength anisotropy that will control the orientation and propagation of hydraulic fractures (Cao and Sharma, 2022; Rysak et al., 2022).'

*What about stylolites in fluid flow studies ? The authors report that stylolites may behave as drains or barriers. First, again, citing only Bruna et al. is not a fair acknowledgement of previous work on the topic (see for instance, Koehn et al., 2016; Heap et al., 2014; Braithwaite, 1989). In addition, when speaking about compartmentalization of the reservoir in term of fluid flow, I guess the authors also consider sedimentary, i.e., non-tectonic bedding-parallel stylolites, the development of which has nothing to do with the topic of the manuscript. It may be misleading to mix features as different as fractures and stylolites, or tectonic and non-tectonic features.

The useful references are added for the stylolites are added (line 345). In general, flow modeling is outside of the scope of the current manuscript. We only predict geometry of the background network in the subsurface and validate this with BHI-data. In the *discussion, we do mention how our proposed methodology might improve fracture modeling and flow simulation workflows.*

Bed parallel stylolites are indeed an interesting feature (we prefer this over 'non-tectonic' stylolites: they are as indicative for paleostress orientation and thus the tectonic regime as bed perpendicular stylolites). They are not included in the presented DAs, as there were no other discontinuity sets observed that are in association with these stylolites, and therefore they did not pass the criterion that we need a minimum of two discontinuity sets before defining an association. Nonetheless, we do think they will have an impact on flow in the reservoir. However, the outcrop is of limited help to predict the impact on the flow of these stylolites in the subsurface: in the outcrop, most bed-parallel-stylolites are open and would act as flow conduits. However, this might well be related to the fact that there is no overburden in combination with recent dissolution, which might not be expected in the subsurface in the target reservoir.

*If I understand well, the authors show that in the area investigated, ~40-50% of the fractures identified by borehole imaging correspond to natural fracture patterns, and discard the uncorrelated fractures as being more recent structural objects (L 321). The authors do not explain which process the formation of these uncorrelated fractures can be ascribed to, or if (why?) they necessarily postdate the development of the regionally consistent fracture sets. In addition, should they predate or postdate the formation of the natural fracture sets, I would expect some of the uncorrelated fractures to be of possible interest by enhancing connectivity of the regional natural fracture sets despite being of local significance, hence permeability of the reservoir . This would deserve a bit more attention for the interested reader.

Indeed, we focus only on the discontinuities that we can place into regional consistent DAs, and refrain from making claims about the other discontinuities that do not fit in this concept, as this would be speculation. The fractures observed in BHI that do not fit within the predicted DAs could either be formed before or after the 45-50% we define as background network, we do not know. Of course, for fluid flow, they are important, because they are present. But they explain these fractures and assess their importance for the reservoir in the Geneva Basin is outside of the scope of the current paper, but clearly should be addressed when executing flow simulations of the reservoir.

* I think that accepting a maximum deviation of ~30° for azimuth and dip of fractures recognized on the BHI is reasonable, but there is no justification for this acceptability 'threshold'. Is it purely arbitrary and does it rely on observations or established statistics ? Please justify.

There is uncertainty related to the orientation of the picked fractures. On top of that, in the outcrop, we also observe variability in the orientation of discontinuities that we grouped into DAs.

Therefore, we consider a 30 degree deviation (azimuth + dip) as acceptable. A higher deviation might even be reasonable, but we do not exceed the 30 degrees, as the individual sets that make a single DA would start to overlap. This justification is added in line (268-271).

* What about the interpretation of the clockwise rotation with depth of the dip direction of bedding planes in GEO-01 ? The authors relates it to the probable presence of a fold, itself potentially related to a fault, at ~480m depth. First, the authors should substantiate this interpretation and possibly discuss the significance of this fold (geometry, consistency with regional features). Second, following the authors' logic, this fold and associated fault have expectedly caused localized brittle deformation, for instance in the form of extensional fractures at fold hinge. Did the authors try to incorporate this aspect in their interpretation / scenario? How do they explain that the % of uncorrelated fractures is similar with Geo-02 ? Third, several studies have shown that stress may be perturbed close to the tip of a reactivated / propagating fault even in still flat-lying strata, causing local directional perturbation of both stress orientation and magnitudes during LPS which may strongly influence the distribution of fractures in advance of fold development (e.g., Bellahsen et al., 2006b; Amrouch et al., 2010). I would expect the authors not to simply brush aside this point. This comment is also relevant to fractures formed in the vicinity of the Vuache fault which formed prior to the Alpine LPS (Oligocene extension, Homberg et al., 2002) and hence was prone to reactivation during the subsequent Alpine compression.

We did not focus on the fold and/or fault that might be present in the well, because that is not the scope of the current manuscript. We focus on characterizing the background network based on outcrop observations. It is by no means the aim to present a 'complete' interpretation of the BHI that explains all features observed in the well.

It is an interesting observation that the % of uncorrelated fractures is similar in Geo-01 and Geo-02. However, as in this study we focus on the background network, we prefer to turn the observation around: the same percentage of features in both wells can be related to the predicted background network. This underlines the regional character of the background network, or in other words, that this portion of the fractures may be extrapolated to the entire reservoir. As mentioned above, it was outside of the scope of the current study to *investigate how the features that are <u>not</u> related to the background network compare between the two wells.*

In this study, we document DAs in the field. It turns out there is a regional consistency in the orientation of DAs, regardless of the distance to regional faults such as the Vuache. We acknowledge that local stress perturbation in flat-lying strata may occur, but with the methodology presented in the current MS, they would not be documented, as they don't reveal a regional pattern. Yet we manage to explain 40-50% of the subsurface features. As mentioned above, we are aware that local stress perturbations may have created fractures - but they are outside of the targeted discontinuities in this study, as it will require a different approach to use outcrops to predict the stress perturbation-related discontinuities in the subsurface.

* The authors make a big deal about the use of directional and kinematic characteristics of fracture –stylolite associations in outcrops, but I am also wondering whether additional information such as fracture dimensions (length, vertical persistence across strata) or fracture connectivity owing to the type of fracture intersections, cross-cutting relationships with stylolites could not be also useful to built a reliable fracture and permeability model of the reservoir since these attributes can hardly be assessed using well bores.

Yes - length, vertical/lateral variability, clustering, aperture, connectivity are all important factors to consider when building the reservoir model, and these factors are hard to assess with borehole data only. It remains challenging to proof that outcrops can serve as an analogue for these characteristics (see e.g. Bauer et al. 2017, Peacock et al. 2019). As this is an important point, it is added in line 314-316 of the Discussion. We believe that the Parmelan might be a good outcrop to study these characteristics, as it is such a large exposure. The best approach to validate its analogy then would be to conduct flow simulation through networks as observed on the Parmelan, and compare this with the dynamic data of the reservoir. However, such a study is beyond the scope of the current MS.

*The authors conclude about the success of their approach in the particular case of the Geneva basin, and encourage people to apply it to other case studies, but what could be the limitations of this approach if applied blindly to a different setting ? I guess the change in lithology with depth, the stress discrepancy between above and below a mechanically weak decoupling layer or stress perturbations caused by large-scale structures could be part of the answer, but the authors should elaborate a little bit on that point.

A clear limitation of the proposed method, as for most methods that use outcrops as analogue for the subsurface, is the aperture and infill of discontinuities. We added a section in the discussion to elaborate on this (line 310-331). Another limitation is the availability of analogue outcrops in the vicinity of the target reservoir.

We think our methodology is not limited to a single lithology type (in this case limestones), as discontinuity associations might form in any rock type. Stress changes with depth are indeed an interesting point, and in many paleostress studies neglected. By considering stress permutations when grouping DAs into events (Methodology, line 138-143), we aimed to include potential stress regime changes that are caused by change in overburden. Mechanically weak decoupling layers might create differences in the regional stress field, but for the proposed DA-methodology, we think it will have minimal impact. The outcrops are of the same age and same lithology as the target reservoir (as in most analogue outcrops studies), and as we focus on the background-forming-events (i.e. prior to fold-and-thrusting and tilting of the strata), we think it is a safe assumption that the target reservoir and analogue outcrops had a similar position with respect to weak layers at the timing of fracturing.

I hope that these comments will help the authors improve their manuscript.

Yes, thank you kindly!