Reply to RC1

We kindly thank the referee for their comments on the revised manuscript and the revision memo. Below, we give our point-by-point response in italic blue font.

I've highlighted some text from the revision memo that underline the differences of opinion. Point 1 is the main issue; points 2-4 are minor.

1. "*To understand the genetic origin of a discontinuity is essential for extrapolation in the subsurface to reservoir scale.*"

"*...we strongly believe that, to improve subsurface discontinuity modeling from a geological perspective, it is essential to consider the stress fields in which discontinuity associations are formed. Without such a step, we cannot predict if a given discontinuity (association) has to be extrapolated to the entire reservoir (with a spatial variability), or only along a fault/fold...*"

"*It is true that we don't know the timing of formation of the fractures and stylolites described in the MS. However, if discontinuities fit in the framework described by Hancock (1985), the simplest interpretation is that they formed in the same stress field. In this workflow, the relative timing between individual discontinuities is of subordinate importance. As we know, the duration of the stress field in which discontinuities form is much longer than the time needed for a fracture to grow and stop, relations between single features do not say much.*"

The main issue is the assertion (1) that "To understand the genetic origin of a discontinuity is essential for extrapolation in the subsurface to reservoir scale." It is notably challenging to discover the 'genetic origin' of most fractures, so this seems like a high bar to me. For example, did this particular fracture (or array of fractures) form due to elevated pore pressure or to tectonic shortening/extension? If making those kinds of distinctions was 'essential' then extrapolation would indeed be hard to do (as some of the references cited in the revised MS show). But as I think the text currently makes apparent, the aim in the MS is the less ambitious one of assessing whether groups of fractures and stylolites formed in arrays of regional extent in flat lying rocks or if they are just associated with faults. Although I think the revised text is adequate, my preference would be to make it a bit clearer to the reader what level of 'genetic origin' information is needed and to underline that many actual subsurface fracture arrays (as documented in the literature) are not part of arrays like those shown in figure 2. *If* outcrops contain arrays of fractures and stylolites that can be associated with each other by the model in figure 2, and the arrays are widespread around the margins of a basin, then an extrapolation of the patterns into the basin is defensible, especially if the patterns can account for orientation patterns documented from the subsurface with image logs. But, the relative timing between individual fractures remains valuable information to collect (and is unfortunately rarely documentable in subsurface data sets), and the relative durations of stress fields and fracture growth are assumptions. I don't agree with the revision memo's apparent disparagement of the approach of defining fracture sets, but this point doesn't seem central to the point of this MS.

We thank the reviewer for clarifying the issue of using 'genetic origin'. We think the reviewer is correct that the 'level of genetic origin' might be the cause of a misunderstanding.

Indeed, the current manuscript makes the claim that for the purpose of developing better subsurface fracture models, it is important to differentiate background/diffuse discontinuities from the fold/fault related discontinuities, as this has direct implications for the distribution of these discontinuities on the reservoir-scale. This is what we mean with 'genetic origin'. To ensure this is clear for the reader, we added this in line 49-50:

'Understanding the genetic origin of discontinuities (i.e. background vs. fold/fault related) is therefore essential for extrapolation of discontinuity geometry to reservoir scale.'

Regarding the relative timing between individual fractures, we might not have expressed our opinion in the best way. But we believe that within a discontinuity association, the relative timing of individual sets is of secondary importance, as it will not provide additional information on the paleostress field in which the DA has formed. We do acknowledge that cross-cutting relationships may contain relevant information. In particular in the case of veins, where the cement of the younger cross cuts the cement of the older, it is a very clear indication of relative timing. We used this to define the relative timing between E1 and E2, i.e. between different DAs.

To clarify this point regarding the relative timing, we added the line (116-118):

'Relative timing based on cross-cutting and abutment relationships between individual sets within a discontinuity association are not considered in this study, as they do not reveal additional information on the orientation of the paleo principal stresses.'

2. A couple of (mostly) usage issues.
"We chose the term discontinuity, as we want to include both stylolites and fractures in our proposed methodology. In this study, we do not focus on individual sets, but on the stress fields in which associations of discontinuity are formed…" and *"As mentioned above, we prefer to refrain from using relative timing for defining sets, as this is not consistent with the DA-methodology. We do infer time relations between different stress fields. These are based on cross-cutting relationships, but we acknowledge that this can be tricky. In this case we do this however, because we are sure we are looking at features from different DAs."* The revision clarifies one of my concerns with the discontinuity association terminology by mentioning 'stylolites and fractures' early in the text. I think readers may still wonder how crosscutting relations can both be used and not be used to make these distinctions.

"We prefer not to use 'kinematically compatible', as we think this is somewhat misleading: discontinuities are compatible with respect to the information they deliver on the stress field in which they formed, not for their kinematics." I guess this response might reflect a

misunderstanding of my original comment. The faults, opening mode fractures, and stylolites in figure 2 are kinematically compatible in that their movement and configuration are consistent with the stress field indicated there, that's why they can be used to infer a paleostress field.

We thank the reviewer for the clarification; we believe that we agree that multiple discontinuity sets are compatible in the sense that they deliver consistent information regarding the paleostress field.

3. *"…fault has the connotation of large displacement (meters)…"* I don't agree with this assertion, but the term 'shear fracture' is entrenched in the literature despite Pollard and Aydin (1988).

We replaced the term 'shear fracture' by fault throughout the text. To make clear to the reader that we are typically dealing with very small displacements, we added 'small-scale' as a prefix in some occasions.

4. As I noted in the initial review, I don't think the word 'analogy' in the Abstract, line 35, and elsewhere, is being used correctly (or at least some other phrase might be clearer). Maybe this is a translation issue. The term 'outcrop analog' (something comparable to another) is widely used but making the inference that the outcrop matches the subsurface is not usually called an analogy. An analogy is a comparison between one thing and some other thing that helps explain or clarify. Usually this is a comparison of two otherwise *unlike* things based on resemblance of a particular aspect, which is not the case in this instance because the authors are claiming that the discontinuity associations in the outcrops are *the same* as those in the subsurface. Calling this proposed correspondence an analogy may hinder comprehension of what the authors are proposing and furthermore this usage hides a claim of the paper. Why not just say that 'based on the similarity of the outcrop patterns to the elements of those patterns that can be discerned on image logs, we infer that the outcrop patterns are representative of the subsurface.' Or in line 35: "However, [demonstrating?] the analogy [correspondence?] between outcrop and subsurface is far from trivial (e.g. Bauer et al., 2017; Peacock et al., 2022). To establish the analogy [correspondence?] between outcrop and subsurface…" The further inference that if two or more things agree with one another in some respects they will probably agree in others is matter for the Discussion (that is, if the orientation patterns match, perhaps the length and connectivity patterns will match). This word usage is certainly not a technical issue worth holding up the MS and I'm happy with whatever the authors and editor decide.

We thank the reviewer for elaborating on the term 'analogy', and we apologize for not addressing this comment properly after the first review. We indeed claim that the geometry (orientation and type) of a part of the network observed in outcrop (namely, the background) is the same for the outcrop and subsurface. To ensure this is clear, the text is adjusted in the abstract in line 1:

'In this study, we present a method that uses associations of discontinuity sets to demonstrate similarities between the outcrop and subsurface.'

And in line 10-11:

'Given the regional character of these events, we predict that the target reservoir is impacted by

them as well'.

And in line 35-37:

'However, demonstrating that the outcrop and subsurface are similar is far from trivial (e.g. Bauer et al. 2017, Peacock et al. 2022). To justify the usage of analogue exposures for characterizing discontinuities in the subsurface…'

Comments keyed to lines in the text

Since I read through the MS again I've marked a few imperfections or questions.

Line 55: "The concept that multiple discontinuity sets ['discontinuities' instead of 'discontinuity sets'?] can form in a single stress field is largely sensed by structural geologist[s] (e.g. Groshong, 1975)…"

Accepted (line 55).

Line 394-5 Don't you mean 'Associations of genetically related discontinuities that form the background network produced by <u>a</u> far-field <u>paleo</u>stress are defined in the field…' Otherwise it sounds like you mean fractures created by the current state of stress.

Correct, it has been adjusted (line 395).

Reply to RC2

We kindly thank the reviewer for the additional comments on the manuscript. We give our point-by-point response below in blue italic font.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

I have now reviewed the revised version of the manuscript by Hupkes et al. My overall feeling is that the manuscript has improved but still requires minor revision before potential publication.
The authors have addressed most of my comments and responded often convincingly to them. However, I have still some recommendations that should be taken into account before acceptance.

*When talking about fracture associations and more generally about the brittle deformation pattern in fold-and-thrust belts, I think that due credit must be given to the review paper by Tavani et al., 2015, Earth-Science reviews which is highly relevant to the topic of the manuscript, should it be about fracture occurrence and types, associations, chronology with respect to folding and even stress interpretation.

The reference is added in line 42

*L79 : Barbier et al. MPG, 2012 should also be considered here

The reference is added in line 48

*L83 : should be roughness instead of shape'

This is adjusted (line 53).

*L153 Focal mechanisms of earthquakes

This is added in line 107-108

*L 167 remove 'fractures'

Accepted.

*L188: change into principal stress axes

Accepted.

*The authors wrote in the rebuttal : 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.

I disagree with this statement, since I do think that for instance multiple discontinuity sets with similar orientations may have formed at different stages of the tectonic history. This is often observed in the field on the basis of relative chronology and has been confirmed recently by U-Pb geochronology on synkinematic calcite mineralization. I strongly believe that determining the relative chronology between individual fractures (mode I fractures or faults) using cross-cutting/abutting/reactivation criteria on a statistical basis remains an extremely valuable and necessary information for tectonic reconstructions. I therefore do believe that the definition of a fracture set should also include the relative timing.

Maybe we didn't express ourselves well, we apologize for that. But we think that in the context of defining DAs, the relative timing between individual sets within such a DA is of secondary importance. The purpose of defining DAs is to reconstruct paleostress orientations that can be extrapolated to the subsurface, and the relative timing between sets within a DA is not going to reveal any new information regarding the paleostress orientation of that DA. Therefore, we did not include this criterion in the definition of discontinuity sets.

We did not mean to say that cross-cutting relationships have no value at all. In particular for the case of veins, where the cement of the youngest vein cross-cuts the cement of the older, it is a very clear indication for relative timing. This is what we used to determine relative timing between different DAs (but not between individual sets within a single DA).

To clarify this point in the manuscript, we have added a line (116-118):

'Relative timing based on cross-cutting and abutment relationships between individual sets within a discontinuity association are not considered in this study, as they do not reveal additional information on the orientation of the paleo principal stresses.'

* The authors wrote in the rebuttal : 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.

I may partly agree, but this must be clearly stated in the manuscript, i.e., your answer to my earlier comment must be transfered into the text. It is of utmost importance in order to unambiguously show the interested reader that this point was not neglected a priori and that you are aware of stress perturbations in the vicinity of (strike-slip) faults (rotation of principal stress axes, variation of the stress ellipsoid shape ratio) and of the development of associations of secondary fractures formed in the perturbed stress field. I therefore invite you to add a few sentences on this point and to consider citing the papers dealing with directional stress perturbations in the vicinity of the major strike-slip faults in the area investigated (Jura) by Homberg et al., JSG 1997 and Homberg et al EPSL 2004.
I would add that according to these papers, the stress may be deviated over a 4-5 km wide area around the fault itself, and not 10s of meters as stated. It is clear from the map of Fig.10 that in sites 35 and 39, the compressional stress is rotated counterclockwise, which substantiates my comments and the need to add some cautionary sentences. In fact, it is because you accept a deviation of 30° in the orientation of your discontinuity sets that you can consider them to still reflect the regional background network despite obvious stress deviations. Again, this must be clearly stated.

Accepted, we added this to discussion (line 302-308):

'Previous workers have demonstrated that in the Jura belt, northeast of the area of interest of this study, stress perturbations impacted the orientation of deformation structures around major left-lateral strike-slip faults (Pontarlier and Morez faults; Homberg et al., 1997, 2004). These faults are similarly oriented as the Vuache fault that bounds the Geneva Basin on the southwestern side. We document two stations in the vicinity of this fault (32 and 39, see figure 10), where the orientation of E1 is rotated 10◦ counterclockwise with respect to the average orientation of E1. It is possible that this small rotation is also related to a perturbation of the stress field around the Vuache fault. However, the relationship of E1 with the bedding indicates that it was formed prior to tilting of the strata, and therefore we consider it to be part of the background network.'

* The authors wrote in the rebuttal : 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.

This looks like you choose to overlook the fact that similarly oriented discontinuity sets or associations of discontinuities (eg. mode I fractures) may have formed at different times as it is now demonstrated by U-Pb geochronology (eg, Zeboudj et al Geosciences, 2025), which would be documented otherwise if looking carefully and statistically at cross-cutting / abutting relationships or reactivation. May I recall that the chronology between E1 and E2 events is based on relative chronology criteria ? So even though you consider the relative timing between individual discontinuities within a given association to be of subordinate importance, you do use relative chronology between discontinuities belonging to different associations to define the sequence of development of the discontinuity associations, hence of the driving paleostress fields. So I find it weird and somewhat dangerous to depreciate the use of relative chronology in fracture studies as you seemingly do. I guess the reader would expect an even short note on that point.

We thank the reviewer for highlighting this point and agree that a note should be added on this point (line 116-118). See our comment above for our explanation.

*The authors wrote in the rebuttal : 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, it might have taken 10s Ma.Therefore, it is expected that different ages will come out for the discontinuity association.

I cannot grasp the logic behind the last sentence regarding the present study. Calcite mineralization is undoubtedly syn-kinematic when associated from slickenfibers from striated mesoscale faults or when calcite has grown as fibers within opened veins. In case of blocky calcite textures, I agree that calcite mineralization may be delayed with respect to opening at the scale of an individual vein, but one can safely consider that, for a vein set, calcite precipitation is coeval with vein opening over the time span corresponding to the fracturing event forming the vein set, and is thus syn-kinematic in a broad sense. This means that in the time interval required to form a discontinuity association you are interested in, calcite mineralizations can be considered as syn-kinematic for veins and faults (see Zeboudj et al Geosciences 2025) within the uncertainty of U-Pb calcite geochronology, so the range of consistent ages obtained for such given fracture association may give a hint to the duration of the related fracturing event/driving stress field. U-Pb calcite geochronology has also revealed that individual fractures as well as fracture associations may be wrongly interpreted to be coeval, hence formed in the same stress field, if the interpretation is done on the sole basis of their geometry and orientation, so going back and forth between absolute dating and 'classical' field-based approach (a fracture set defined on the basis of similar orientation, deformation mode AND relative chronology) is to date the more efficient way to meaningful tectonic reconstructions. I acknowledge absolute dating of fractures is beyond the scope of the study, but, again, a cautionary note would be very useful.

We agree with the reviewer that absolute ages with U/Pb geochronology of calcite mineralization would be of great added value to the present study. It will indeed constrain the timing of events, and may reveal complex tectonic histories of the discontinuity network. The potential of geochronology was added in the manuscript in line 315-317.

* The authors wrote in the rebuttal : 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.

Yes (see above), but in fact you analyzed fractures with an Andersonian state of stress in mind, which is already an hypothesis in folded and faulted domains (again, see Tavani et al., 2025). As I wrote previously, the classical fold test must be preferred.

All the data presented in figure 8 is backtilted with respect to the bedding, to investigate if the resulting princpial stress orientations are indeed indicative of an Andersonian state of stress.

* The authors wrote in the rebuttal : We added the references for stylolite as paleostress marker in the Introduction. 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. 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.

Yes, I agree, but in the case of tectonic stylolites you should be able to define whether the stylolites truly developed under a reverse or a strike-slip stress field, and therefore if they are mechanically consistent (or not) with the discontinuity association of interest to your tectonic reconstructions. Note that the references I provided pertain to tectonic stylolites only. I acknowledge this point goes beyond the scope of your manuscript.

To use stylolites to distinguish between strike-slip and reverse regimes is an interesting point, and indeed would be a great complementary analysis for the work we present in this study.

* The authors wrote in the rebuttal : 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.

Just to make things clear, you appreciate tectonic stylolites may be indicative of strike-slip or reverse regime because they are seemingly associated in the field with strike-slip or reverse faults, respectively. But this is some kind of circular reasoning. What I mean is that the real stress regime associated with stylolite development could be documented independently from the fracture associations by the approach mentioned above, the only to date to the best of my knowledge, providing a possible test on the validity of relating stylolites to the same association than conjugate faults and veins. However, I acknowledge that this point requires also the estimate of the depth of formation of the tectonic stylolite, and that the approach would need much more analyzing effort than collecting fracture data in poorly deformed sedimentary rocks.

That clarifies the point. Indeed, such a study on stylolites would be a very nice addition to the presented work, and worth investigating in follow-up research.

* The authors wrote in the rebuttal : 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.

Frankly speaking, you cannot simply brush this comment aside by claiming the occurrence of the fold is not the scope of the manuscript. Since you choose to show two wells GeO-01 and GeO-02, you cannot overlook simply the fold occurrence in GeO-01.

The authors wrote in the rebuttal : in this study we focus on the background network and 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 not related to the background network compare between the two wells.

I may understand your point, but first in L 394 you wrote there was no fold ! Second, since you also wrote that a fold create some localized fracturing, the reader expects that crossing a fold will increase the number of fractures encountered in the well. How do you explain that the same percentage of features in both wells are related to the predicted background network, regardless of the folded or flat-lying character of strata ?

The reviewer is correct that in line 394, we suggested that there is no fold/fault in the subsurface of the Geneva Basin, and this is not the case everywhere. The statement only applies for GEo-02, and the line is adjusted accordingly (now line 309-310). But also in Geo-02, almost half of the observed features fit within the background framework.

The question why we see the same percentage of features of background network in both wells is an interesting one. There are different explanations possible for this.

One possible explanation is that the background network may have a spatial variability, and clustering of background-discontinuities may occur. This could be the reason that the total background-related features in Geo-01 is higher than in Geo-02, but then the overprinting of the fold resulted in a similar percentage as in Geo-02.

Another reason could be the different available logs: for Geo-01, there is both OBI and ABI available, for Geo-02 only ABI (see line 238). It is possible that not all features present in well Geo-02 are observed due to the lack of OBI. Besides this, there might be a bias in the interpretation of Geo-01. We are currently re-evaluating multiple interpretations of this well, to assess the impact of bias on the final interpretation.

These considerations are added in the manuscript in line 360-370:

'The DA-methodology provides a prediction of the discontinuity type of up to 50% of the observed discontinuities in the two boreholes in the Geneva Basin, even though the resolution of the BHI is too low to determine discontinuity type, and there is no core

available to correlate the BHI with. The percentage of background-related discontinuities is similar in the two wells, whereas the total number of features is higher in GEo-01. A possible explanation is that there is only ABI-log available for GEo-02, and no OBI. It might be that not all discontinuities present in GEo-02 are visible on the ABI log, due to a lack of contrast in acoustic properties between host rock and discontinuity, resulting in a lower number of features picked. Another possibility is that the difference in number of background-related features is caused by the spatial variability within the background network. GEo-01 might have penetrated a denser part of the background network compared to GEo-02. After the emplacement of the background network, GEo-01 is affected by localized deformation (i.e. a fold, see figure 11), producing more discontinuities in this well. At present-day, the percentage of background-related discontinuities is then the same as in GEo-02.'

*The authors wrote in the rebuttal : 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.

I disagree. Local stress perturbations are likely documented but they are hidden behind the 30° deviation you accept for considering the discontinuity association to belong to the background pattern. Clearly, the stress is rotated counterclockwise on the map of figure 10 (sites 35 and 39). See also my earlier comment above. Again, a cautionary note is needed.

Indeed, for E1, there is a counterclockwise rotation of 10° for S1, which falls within the deviation we consider acceptable for defining background-related discontinuities in the wells. We added a cautionary note on the possibility of rotation of paleo principal stress axes due to perturbation around faults (see line 302-308).

I hope that these new comments will help the authors clarify further their interpretations, introduce some cautionary notes about their choices and possible pitfalls of their approach and provide a more comprehensive description of the hypotheses behind their approach without omitting some specific points which would easily open room for criticism.
I am looking forward to seeing the revised manuscript published soon.