

I thank the editor for asking me to assess the revised manuscript and revision memo based on his impression that the revision and revision memo may not have been fully responsive to my review. In general, I think the authors have done a thorough revision and that the concerns that I raised have been attended to. There are several technical and usage issues where we do not agree, but the revised text (and the published review comments) should allow readers to make up their own minds about it.

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.

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.

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).

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.

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)..."

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