

## ***Interactive comment on “Transverse jointing in foreland fold-and-thrust belts: a remote sensing analysis in the eastern Pyrenees” by Stefano Tavani et al.***

**Roger Soliva (Editor)**

roger.soliva@gm.univ-montp2.fr

Received and published: 26 June 2020

Dear Authors,

Thank you for the revisions made which improve the quality of the paper. I understand the wish to keep hypothesis first in the paper (model based writing style mentioned by both reviewers and I) and then I have recommendations about strengthening both your hypothesis and interpretation. This is needed anyway in the paper whatever the style is chosen, but actually more relevant if you chose this style. Especially, I recommend considering some significant references on the topic that have been ignored to reinforce both your interpretations and hypotheses.

C1

1) In the revised version, you mention in lines 30-31 of p7 "we conclude that foredeep-parallel extension has occurred in the foredeep of the Pyrenean belt since the Paleocene and until the end of convergence" Do you consider here that  $\Sigma_3$  is negative as proposed in Figure 1b and introduction? Extension is an unclear deformation term not synonym to tension or extensional stresses (i.e. negative stresses). Clarify this in the text please.

2) On this negative stresses as shown in Figure 1b, although we can agree on your interpretation, the paper suffers considering the significant contributions from experimental tests which have been compared to natural joints from the past decade. You mention extensional stresses (negative) but what about splitting without negative stresses (and even with a slightly compressive  $\Sigma_3$ ) such as demonstrated in dry axis-symmetric, oedometric, plane strain and poly-axial experiments by Chemenda et al. (JGR, 2011) and Jorand et al (Tectonophysics 2012)? These studies show joints formed under dry contraction without negative  $\Sigma_3$ , which are not so far than uniaxial splitting fractures observed in triaxial cells (e.g. Holzhausen and Johnson, 1979), but here clearly without the triaxial boundary effect mentioned by Fakhimi and Hemami (2015).

3) A common species of joints show very low displacement gradients compared to other fractures (veins, faults) (Pollard and Aydin, 1998; Schultz et al., 2008), which also support the general fact that joint sets do not require significant amount of negative stresses perpendicular to them. Have you measured the mean opening of the observed fractures? Can this help to discuss this point?

4) I recommend you to better support the hypothesis mentioned in lines 31-32 p2 and 1-3 p3, which only relies on one reference, while others works previously described stress permutation during LPS. For example, stress permutation in foreland basin has been proposed from field observations and stress path calculations by Soliva et al. (2013), and reused with nearly the same concept in Fossen's book 2015 version. Addition of such references is just a fair strengthening of the hypothesis on which the work relies.

C2

Please, consider discussing/including these elements in your paper, you have plenty of space since the paper is quite short.

Best regards, Roger Soliva

---

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2020-70>, 2020.