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

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Comment 1

In the introduction, you outline a mechanism to explain transverse jointing in the foreland. Then you continue with the outlook that the acquired remote sensing data allows to investigate the primary mechanism for this joint formation. Hence I wonder: Haven't you proposed already before the data collection and discussion of what the primary mechanism is? Does this lead to a bias in the data interpretation? Maybe it would be better to present the model after the data presentation in the discussion to avoid that such an impression might arise.

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Response
Not agreed. This is a matter of writing style and not a source of bias. In our view the rationale of the work and the state of the art must include a brief introduction to the causative geological processes allowing to understand data.

Comment 2
Also on that regard, a discussion on the potential driving force behind the suggested orogen-parallel stretching of the foreland basin is largely missing (or well hidden, in case I missed it), which would be very interesting though.
Response
We will add this in the discussion: The basic concept behind this mechanism is the following: when a straight line joining two fixed points - the tips of a fault in the case of Destro (1995) or the edges of the fordeep in the case of Quintà and Tavani (2012) – becomes an arch, there is stretching (Fig. 1b), which causes extensional stress parallel to the direction of elongation. In essence, this mechanism is expected to operate in any doubly plunging foredeep, particularly at its lateral edges, such as the study area (Fig. 3a).

Comment 3
In the introduction, it is briefly referred to two publications invoking the possibility of lithospheric bending to account for such kind of stretching (Doglioni, 1995; Quintà and Tavani, 2012; although the process described by Doglioni is regarded as not applicable for the study area, which is comprehensible). In the discussion, this subject is covered with only two sentences (page 7 line 31 to page 8 line 3; half of it being a repetition of the introduction statement). Here, foreland-parallel stretching is suggested to form the N-S joints and an analogue reference is made to the process of release faulting (Destro, 1995). I think this requires much more attention: Destro (1995) describes a purely extensional setting and it is therefore not straightforward to

understand how this applies to the Pyrenean foreland, especially in the light that you propose foreland-parallel extension from the Paleocene until the end of convergence (page 7, line 22). Hence, I believe a more elaborate discussion for the use of this model is necessary, in particular, and potential driving mechanism for such stretching, in general.

Response

See response to the previous point.

Comment 4

Adding to this, you mention the westward plunge of the foredeep basin and refer to figure 3a. I am not sure if this is it actually visible in the figure or if it requires previous knowledge of the region to identify it!? I think an E-W cross-section would be very helpful.

Response

We will add the trace of the axis of the foredeep basin in figure 3 to show its W-ward plunge.

Comment 5

A second issue revolves around the timing of joint formation. You state that the dominant N-S trending joint system formed prior to folding and refer to figure 2b, where joints are supposedly tilted. Unfortunately, from the picture alone, it is very hard to see this. How did you determine that these joints are tilted? How can you exclude the possibility of joint formation after folding? Such a determination appears to me as a very difficult asset, since you would have to know their original orientation and at the same time line out why its present orientation is not the original one. I think this is a very important issue that needs to be clarified.

Response

Timing of deformation is rather evident from figure 2. We will improve the description of this figure: In the field, joints are constantly bedding-perpendicular, regardless of

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the bedding dip (Fig. 2a,b), and they are characterized by the occurrence of a single set (Fig. 2c) or by a ladder pattern (Fig. 2d,e). In the latter case, the few E-W striking joints are almost everywhere perpendicular to the N-S striking set and abut on it (Fig. 2e). This indicates that E-W striking joints are cross-joints formed perpendicular to, and about synchronously with, the N-S striking joint set.

Comment 6
A second argument for the age of joints is their absence in Quaternary sediments. First, there is still a large age span from the Quaternary to the Eocene (using the word "evidence" (page 7, line 9) for an Eocene formation age is therefore maybe a stretch), and second: what is the character of these sediments? Are they solidified to a degree where fractures would be able to form in case the joints in the Eocene rock were of Quaternary age?
Response
See response to previous point.

Comment 7
Another thing: As you have been in the field, it would be great to see a comparison of field data with the remote lineament data. E.g. do the joints have a preferential dip direction, are they all just vertical?
Response
We will add stereoplots of joint data collected in the northern portion of the study area.

Comment 8
Figure 1: This is a very nice figure, but some features can only be identified when zooming in a lot, i.e. the text "peripheral bulge", veins, and stylolites. Please improve this. Also, I recommend to place the names forebulge, foredeep, foreland fold-andthrust belt into/above the block figure and not just mention them in the figure

caption.
Response
We will do that. Thanks for the suggestion.

Comment 9
Figure 2:
1) please show the locations of these outcrops in figure 3.
2) Also, I would prefer to show field photos after showing a map of the study area.
3) In Figure 2b, please point at the joints as it is not super clear that the big surface is, I assume, the bedding surface.
Response
1) This cannot be done due to the size of the figure: the labels of the five sites would cover much of the figure.
2) In the text the figure 2 is called before figure 3, so it cannot be shown before.
3) Yes, the south-dipping surface is the bedding, we will mention it.

Comment 10
Figure 3: add some placemarks (e.g. towns) to the map, so that it's a bit easier for the reader to capture the location of the study area. (took me a little bit to find the exact area on google earth).
Response
We will do it.

Comment 11
Figure 5: I think it would be really nice, if you exemplarily show a few rose plots (joint length-weighted) for different colored regions in figure 5. I believe this would make it much easier for the reader to understand how to read the color code of the figure.
Response
This is probably a misunderstanding. The colour code refers to the dispersion of

azimuthal data, which is not well appreciable in rose diagrams as the dominant set is much developed than the other sets. We will improve the caption of the figure.

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