Review of Kettermann et al manuscript

This manuscript deals with the laboratory study of the structural features associated with rifting of jointed rock formations. The first section nicely introduces the problem, highlighting that most former research focused on rifting of intact rocks, whereas it is known that natural rocks are intensely jointed/fractured. Therefore this study is innovative and relevant.
The method is also well described, and clear. It explains in a very didactic manner how the experiments are prepared, describing in details the technical tests the authors performed. In particular, they discuss the methods used to prepare pre-joints in their experiments, and justify the method they chose.
The results are also convincing. They show how the angle between extension direction and the orientation of pre-existing joints control the deformation patterns. They also highlight very well that the deformation pattern strongly differs, whether the models are pre-jointed or intact. This fact demonstrates that former intact models are not relevant for studying the natural complexity of rifted rock formations.

I notice, however, some critical potential problems in the concepts of the presented experiments. Indeed, it is not clear how reproducible the experimental results are, especially with respect to the initial conditions (see details below). In addition, the authors arbitrarily selected a joint depth, without justifying it and discussing its relevance/effect.

Therefore, I would recommend the manuscript to be accepted for publications, after some critical corrections.

Main comments

Reproducibility of the results with respect to initial conditions. Figure 10 shows complex trends of the results. In particular, the authors argue that the lack of trend for the JF-Angle between 0 and 8 is possibly an "effect of the limited width of the deformation box, as in experiments with small joint-fault angles joints do not necessarily intersect the basement fault trace". This is critical, because the authors implicitly imply that according to the initial conditions, i.e. the position of the joints with respect to the basement fault trace, the results are not reproducible. This means that the initial positions of the joints with respect to the basement fault trace is a very critical factor. And I suspect that it might be also the case for JF-Angle larger than 8 degrees. Indeed, depending on the positions of the joints, a different number of joints can intersect the basement fault trace. This fact strongly questions the relevance of the "trends" of Figure 10. At least, this critical initial condition likely explains the very scattered and chaotic results displayed in Figure 10. The authors should really discuss, and even quantify, the effect of the initial position of the joints with respect to the basement fault trace. I suspect that this would require other laboratory tests to be performed.

Initial conditions. The heights of the joints are of 5 cm, with respect to the 19 cm thick box. What is the effect of the ratio between the heights of the joints and the total thickness of the box? What would happen if, for example, the joints are as deep as half of the box? This has major consequences, as joints often fully cross cut rock layers entirely. I suspect that the results would be very different. The authors should also discuss this very important initial condition and it potential effect on their results. I recommend the authors to perform a few experiments with varying heights of the pre-existing joints.

Section 5.3, Field data. This section is difficult to read, as there is no graphic support, i.e. no figure. The only figure is a field photograph, where none of the conclusions of the laboratory study is visible. This is incompatible with the conclusion of the authors that states "Robust structural features that occur in the models as well as in field prototypes…". In this paper, it is impossible to assess the relevance of the laboratory results with respect to field data. I thus would recommend the authors to include a figure (at least) displaying field data, such that the comparison between laboratory and field results is more obvious.

Minor comments

Page 3, line 6. The authors could also include references to Galland et al. (2006), Galland et al. (2007) and Le Corvec et al. (2013) at the end of the last sentence of the paragraph.

Page 5, lines 7-18. The authors should specify whether the optical distortion is corrected or not. This has important implications on angle measurements presented later. Also they should indicate the lens characteristics, especially focal length, such that the reader has a good idea of the amount of optical distortion to be corrected.

Section 3. In general, it lacks lots of references to figures. It is difficult to follow the text. Therefore, almost for each statement of the text, the authors should refer to the corresponding figure.

Page 7, last sentence. I don't understand the sentence. Maybe I am not awake, but the structure seems quite complicated. It would be good to split the sentence into several.

Page 7, lines 26-27. The authors mention that the y-displacement components highlight reverse faults. This is the case because the y-axis is coincidently properly oriented with respect to the local shortening. In addition, without the drawings of the authors, the reader would likely not see the reverse faults. In general, it is better to compute the divergence of the displacement field: positive divergence means extension, negative divergence means compression (Byrne et al. 2015). I recommend the authors to plot the divergence field to highlight the local compression.

Figure 12. What is actually plotted in this figure? Shear strain, divergence, rotation angle? This must be specified.

References

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