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Interactive comment on "Extension and Inversion of Salt-Bearing Rift Systems" *by* Tim P. Dooley and Michael R. Hudec

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Using an experimental approach based on physical models, this manuscript analyses the role of syn-rift evaporites during extension and subsequent inversion of salt-bearing segmented rift basins. Different authors have addressed this type of studies using rigid basement blocs to simulate the basement with the consequent mechanical limitations that this methodology entails. The present manuscript has partly solved this limitation in an original way: a hybrid system that combines a rubber sheet (classically used to constraint the location of extensional faults during stretching) with polymer slabs (also used in physical modelling as "seeds" to constraint fault nucleation). This hybrid experimental setup allows to achieve a significant degree of inversion of pre-salt grabens (an inherent issue of the models that use rigid blocks). In addition, the en-echelon distribution of the different slabs allows to simulate a segmented rift system. Another interesting point addressed in the manuscript is how syn-rift salt controls the structural style during extension and consequently, how the inherited salt structures at the end of the extension constraints deformation during inversion.

The manuscript reads well and the quality of figures is excellent, they perfectly illustrate the text. The scaling of the experimental program is correct and the analog materials are the classical ones used in physical modelling of salt tectonics. The experimental results are compared with natural examples from the Moroccan High Atlas but they would be perfectly applicable to other fold-and-thrust belts involving inverted salt-bearing rift basins such as the Pyrenees. I am sure this manuscript will be useful to the understanding of inverted salt-bearing rift basins. For this reason, I recommend its publication in Solid Earth journal after few minor revisions (please, see below general and specific comments, suggestions and questions).

Best regards,

Oriol Ferrer

General comments

A point that I consider should be implemented in section “2.1. Model Design and Scaling” is as far as syntectonic sedimentation is concerned. What is the sedimentation rate? Did you keep the pre-extensional regional fixed? How much did you raise it every new synkinematic layer of sand? These are points that the reader should know. These points are addressed in the experimental results section, but they should be moved at section 2.1.

Include a figure like figure 2 of Roma et al. (2018b) could help to understand the procedure applied during model run. This is just a suggestion.

As far as setup is concerned, I don’t understand why did you modify the extension of the basal detachment layer in models 1 and 2/3 respectively. Why not to use the same for the 3 experiments? Can this modification influence the final results in any way?

The section 3 (Experimental results) is clearly described and well ordered. However, I disagree about the harpoon structure described in lines 28-287 (also in line 419, section 4.3. “Shortening in the subsalt section”). According to McClay (1995), the inversion of the wedge shaped synextensional strata produces typical “harpoon” or “arrowhead” geometries, the shape of which depends upon the geometry of the underlying extensional faults. In these lines of the manuscript, this geometry is wrongly applied to the inverted preextensional unit. Please, modify it.

Regarding this section, could the marginal grabens be related to edge effects of your experimental setup?

As it has been pointed out in the manuscript, the contractional reactivation of graben-bounding faults during inversion can be favored by the polymer infiltration into the granular material (sand). This infiltration occurs either at the interphases between polymer and pre- / supra-polymer sand during the setup of the experiments, and throughout the experiment when new surfaces (faults) developed. This process occurs when the sand-polymer interphase is preserved for a long time, so can we interpret that the longer the contact, the wider the area affected by polymer infiltration? If this is the case, the contractional reactivation of the sand-polymer interphase in the fault will be more effective because it will “lubricate” in a more efficient way. Is this observation true? Other prompt questions regarding this topic are: What is the infiltration rate? There is any control of this process during the construction/run of the experiments? What are the main factors that control it? I perfectly understand that these questions are out of the scope of the manuscript, but as a modeler and after to notice similar processes, I consider that this topic should be discussed in the manuscript.

Considering that model 2 was sliced at the end of the extensional stage, and model 1 is similar but with 25 cm of inversion, it would be interesting to include a section discussing the contractional reactivation of primary welds and what is the role that they play during inversion. Are they reactivated as thrust welds? What occurs with their surface? There is any increasing/decreasing on their surface during inversion? Have you noticed the opening of the primary welds during inversion in your 3D voxels? The paper by Roma et al. (2018b) includes some discussion about this topic that could be compared with the models included in the present manuscript.

Due to the few published works in analog modeling addressing the role of synrift evaporites during extension and subsequent inversion, I consider that some additional references such Soto et al. (2007); Ferrer et al. (2014); Roma et al. (2018b) should be included in the manuscript.

Ferrer et al. (2014). The role of salt layer in the hangingwall deformation of kinked-planar extensional faults: Isights from 3D analogue models and comparison with the Parentis Basin. Tectonophysics, 636, 338-350.

Roma et al. (2018b). Weld kinematics of syn-rift salt during basement-involved extension and subsequent inversion: Results from analog models. Geologica Acta, 16 (4), 391-410.

Soto et al. (2007). Geometry of half-grabens containing a mid-level viscous décollement. Basin Research, 19, 437-450.

Specific comments:

Line 50 “Calloway” should be “Callaway” Line 74 at the end of the line, modify “halo kinetic” for “halokinetic” Lines 109-110 Check the sentence Line 113 indicate the thickness of the basal polymer Line 114 indicate the dimensions of the slabs Line 157 indicate the % of sand and ceramic microspheres that you used, and if this % is in weight or volume Line 508 modify “teh” for “the” at the end of the line

Figure 2 is difficult to understand. Is the rubber sheet transparent?

Figure 4 Why did you use green polymer? I did not found the explanation in the manuscript.

The word “stage” is used indistinctly in capital or lowercase to refer to the different evolutionary stages of the models. Please, use uniform criteria.

There are some references that do not match those of the reference list. Among those I have detected: Bonini et al. (2011); Moragas et al. (2016); and Martín-Martín et al. (2016). Please, check which one is correct and unify. Similarly, there are missing references in the reference list: Adam et al., 2005 (line 180); Sibson, 1995 (line 430) and Anderson, 1951 (line 431).

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