The manuscript by Ferrer et al. presents a series of analogue experiments that investigate an extensional domino-like basement fault system with a pre-tectonic unit under extension and subsequent inversion phase. The pre-tectonic unit, comprising a salt layer and an overburden (with different thickness ratio), acts as a mechanical discontinuity between basement-fault system and a syn-tectonic unit on top. The work provides a carefully laid out presentation of salt-related deformation structures and, the influence of the layer thickness ratio on the style of basin inversion. Thanks to a well-established workflow the authors present quantitative results that allow for a detailed discussion of the salt layer that conditions coupling between the basement-fault system and the overburden succession during inversion. While the extensional phase of the experiments has been previously described by the same authorship, the detailed investigation of the rift inversion part provides novel and original insights into coupling mechanisms and structural reactivation.

The manuscript appropriately reviews published work on the role of salt layers and their role during rift system inversion and introduces previous analogue modelling studies on basin inversion. The conducted models explore different thickness ratios between salt layer and overburden (i.e., the pre-tectonic unit) and test how this thickness ratio influences the style of extensional structures and their reactivation during subsequent inversion. The modelling results are presented based on central 2D cross-sections at final extensional stages and after the identical amount of shortening. These cross section show in great detail extensional structures and their role in the subsequent inversion, and highlight distinctive styles of basin inversion governed by the salt-overburden thickness ratio. This contribution seems well suited for EGU Solid Earth and the special issue Analogue modelling of basin inversion. The manuscript is original, overall well written and well organised. Illustrations are clear and concise and mostly guide the reader (see minor comments below). I would recommend accepting this manuscript after some minor to moderate revisions.

Below, I present some key points that are mostly related to the organisation of sections as well as the discussion for which I have some concern followed by a list of minor comments and suggestions:

1) My first comment addresses the somewhat confusing section numbering. At its current state, not all sections have a number. I find the attempt to divide the manuscript into *Introduction* and *Analogue modelling* disadvantageous. Since the *Analogue modelling* is a bigger part of the manuscript, subsection numbering becomes a bit confusing. From the manuscript title (as well as the special issue) it is evidently an analogue modelling study. Hence, the section *Analogue modelling* (which includes the entire manuscript apart from the Introduction) is needless. I suggest a simpler structure where *Introduction*, *Experimental methodology*, *Experimental results*, *Discussion* and *Conclusion* build the top level with pertinent subsections.

2) Overall I enjoyed reading the discussion of this manuscript, however, at parts, paragraphs are lengthy and could benefit from restructuring. Fixing the section numbering (see point 1) allows to structure the discussion within additional subsections for more clarity. This addresses particularly (current) section 1.3.3 (How does inversion affects salt migration, primary welds, and the final geometry of the inverted basin?). I believe this rather long part could use rearrangement into smaller subsections that are more digestible to the reader. Such sections could address the final geometry/topographic relief, primary welds, weld reactivation/opening, coupling/decoupling, according to current section 1.3.3.

3) The reopening of welds is an interesting point and is well described in the manuscript and visually supported by Figs. 9 & 10. In most parts I agree with the reasoning of the authors that salt flow during weld reopening is rather a passive than active process. However, I feel that there is a potential contradiction with the ramp-syncline development in Lines 308-312. Those lines perfectly describe an interplay of passive salt flow (i.e., towards the hanging wall due to the counter-clockwise basement block rotation), followed by active salt flow (due to the subsequent sinking of the ramp-syncline) when salt gets expulsed. In my opinion, lines 308-312 demonstrate, that salt flow occurs in an active and passive fashion at different deformation stages. In this light, I would argue that flow during inversion, most likely, undergoes passive as well as active stages at different times.

   One could envision, for example, that at early stages of inversion (e.g., Fig. 10b), salt may actively flow out-of-plane (i.e., along fault strike) as the basement blocks rotate in a clock-wise fashion, causing an along-strike pressure gradient. Since the presented experiments show a rather regular along-strike evolution, I don't assume this to be the case in this study. However, in nature where along-strike irregularities are more likely this should be considered.

4) The manuscript is generally well written and concise. However, some sentences are relatively long making it difficult to grasp all the information (e.g., Lines 34-39, Lines 265-267, Lines 299-302). Such long sentences might benefit from splitting information into subsequent sentences.

Minor comments:

+ Please check the section numbering. At the moment, not all sections have a number.

+ Line 52: “[…] BUT/HOWEVER, the number of works of inverted basins with mechanical anisotropies […]” – missing conjunction

+ Line 53: “While some of them considered […]” – I suggest replacing “them” with “these studies” or a similar expression.

+ Line 58: “[…] used an original setup based on polymer seedS to constrain […]” – missing the S in seeds.

+ Lines 72-80: It would be good for the reader to know the overall model dimensions. What is the initial length of the model prior to extension? I am also curious how “deep” (i.e., along fault strike) the model setup is.

+ Lines 106-107: “[…] and the positive reliefs caused by salt inflation were episodically eroded.” – This needs clarification. How were the reliefs eroded?

+ Line 118: The subsection number here should be 1.1.2 (but see major comment 1).

+ Line 123: “To color it […]” – T at the beginning of the sentence should be capital.

+ Line 125: Is the polymer viscosity really 10^{-4} Pa s? I assume the sign in the exponent is wrong and 10^{4} Pa s is more reasonable for PDMS. This also concerns Line 127 as well as Table 2.

+ Line 128: (Ferrer et al., 2017) should be 2016? Otherwise this reference is not listed.

+ Line 135: It would be interesting for the reader to know the camera model as well as the actual resolution.

+ Line 140: Please provide the name and version of the commercial software.

+ Lines 210-220: Figure references should refer to Fig. 4d rather than Fig. 4e.
+ Lines 223-281: The color coding for structures in Fig. 5 is not clear to me (see also comment on Fig. 5). I would recommend to describe in the text (where suited) the meaning of fault colors (i.e., red → inherited?) in Figure 5.

+ Lines 239-240: “[…] thrusts affecting the overburden are directed towards the fixed wall […] ” – I find this terminology (i.e., directing) ambiguous. I would recommend to use “dipping towards/away” or “top to the…” to avoid confusion. This also concerns line 254.

+ Lines 308-312: This refers to major comment 3. In my opinion, it well demonstrates the interplay of active and passive salt flow.

+ Lines 630-664: The description of Fig. 7d and Fig. 7h is missing here. I would like to read that for completeness.

+ Lines 372-373: “[…] the trajectory of the salt-detached ramp-syncline depocenters is not lineal but curved thus recording the salt migration process that occurs as extension progresses […]” – I struggle to understand this sentence. Please be more specific here.

+ Lines 386-389: “[…] of structures allowing to generate relief is directly related to the degree of decoupling between […]” – This is an important statement! Unfortunately it is not that clear. I suggest rephrasing for clarification.

+ Line 432: Fig. 10b – this should be Fig. 10c, I believe.

+ Figure 4: Please indicate in the caption the meaning of the white dashed line (Datum?) and the white dots (welding). Additionally, Fig. 4a (DOM4), F4 contains an antithetic fault in the counter-clockwise rotated basement that should be indicated for consistency.

+ Figure 5: As in Figure 4, the white dashed line as well as the white dots should be explained in the caption.

+ Figure 5, Lines 833-834: “[…] colors of the faults keep consistent with Figure 1 […]” – To my understanding, the color coding of the faults is not defined in the manuscript. I suggest to reuse the color coding according to Figure 1 in the text where appropriate (e.g., results section).

+ Figure 5, Line 834: Figure 1 – F should be capital.

+ Figure 6: Both columns (a-d and e-h) could use a title (i.e., “after extension” and “after inversion”) to make the figure arrangement more clear.

+ Figure 7: Similar to Figure 6, titles for both columns would help for clarification (e.g., “top basement/base salt layer” and “basin infill/syn-extension”).

+ Figure 8: Very nice visualization! Just out of curiosity: Before cutting the model, how do you generally manage that the hardening agent pierces through the PDMS layer and reaches the basement configuration?

+ Figure 9: I don’t understand the term “weld widened” in sub-Figure 9a. To my understanding, these areas indicate where welds remain after the inversion (i.e., they remain close). With that respect, the term “widened” should be clarified.

Bern, 04/01/2023
Timothy Chris Schmid

timothy.schmid@geo.unibe.ch