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Editor-in-Chief
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Dear editor,

please consider our revised manuscript “Contribution of gravity gliding in salt-bearing rift basins -- A new experimental setup for simulating salt tectonics under the influence of sub-salt extension and tilting” (by M. Warsitzka, P. Závada, F. Jähne-Klingberg and P. Krzywiec) for publication in Solid Earth.

We revised the manuscript according to the suggestion given in the three reviews by Gaël Lymer, Frank Zwaan and Oriol Ferrer. We thank the reviewers for their constructive comments and improvements on our manuscript and basically agree with most of their suggestions and changed the manuscript accordingly. In case we did not agree with the comments, we gave an explanation for that in the one-by-one replies (see below).

Below we list the major changes in the main text, the figures and the tables, which we did according to the suggestions by the reviewers. We also listed a few minor additional changes. In the following, we provide one-by-one answers to each comment of the three reviewers. Reviewer 2 Frank Zwaan uploaded a commented .pdf file of the manuscript. We replied to each comment directly in this .pdf and uploaded it as supplement in the reply to the review (‘Reply on RC2’).

In the .pdf file ‘Warsitzka-et-al_SE-2021-17_track-changes.pdf’, we used the Latex package latexdiff to mark all changes. All deleted text parts are in red and crossed out. Newly added text parts are in blue.

Along with this manuscript, we submitted a data publication, containing all relevant data shown in the figures of the manuscript, to the Pangaea repository in February this year. Unfortunately, the data has not yet been processed and we are still waiting for a reference to this data publication. We will inform you as soon as the data is available.

We declare that this the manuscript has not been submitted to any other journal and that there is no conflict of interest to disclose.

Kind regards,

Michael Warsitzka on behalf of the co-authors

1. **List of relevant changes according to the suggestions by the reviewers**

Main text:

- Section 3.1 Experimental setup: Additional explanations were given for the experimental setup
- Section 3.3. Scaling: This section including the former figure 4 was moved to Appendix B. In the main text in section 3.3, we left a shorter version of this section, where we describe the basic concepts of the scaling procedure
- Section 3.4 Density stratification: the entire section including the former figure 5 was moved to Appendix C. The most important information of this section were integrated into the section 3.4 Experimental procedure (in the revised version of the manuscript)
- Results: Section 4.1 and 4.2 were divided into smaller subsection to improve the structure of the results chapter
- Section 4.2 Flat vs. tilted flanks: Large parts of the section were rephrased in order to add some more information about the experiments and more emphasize specific differences between both experiments ETv and Ev.
- Discussion: Section 5.1: The section was rephrased to discuss the observed experimental results in more detail and to include comparisons with previous analogue modelling studies in this discussion
- Section 5.3 Application to nature: Since we exchanged some subfigures in the related figure 13, we also changed the descriptions of these subfigures in the main text of section 5.3. Furthermore, we slightly rephrased the descriptions of the other subfigures to give a better comparison with our experimental results.

Figures (referring to the figure numbering in the revised version of the manuscript):

- Fig. 1: Example of rift basin was changed in (b)
- Fig. 2: Schematic cross sections of the silicone basin was added as (c)
- The former figures 4 and 5 in the first version of the manuscript were moved to the new Appendix B and C
- Fig. 3-5: Schematic cross sections of each experiment were added as (a) and (b). Evolutionary plots in (e) and (f) were exchanged
- Fig. 7: Evolutionary plots in (g) and (h) were exchanged
- Fig. 8: Evolutionary plots in (e) and (f) were exchanged and top view photos of the experiments were added as (g) and (h)
- The former figure 12 in the first version of the manuscript was split into two figures (Figures 9 and 10 in the revised version of the manuscript), because we think that the former figure 12 was overloaded
- An additional figure 12 was created summarizing the main experimental results
- Three sub-figures in figure 13 were exchanged
- The former figure 10 in the first version of the manuscript was deleted and 2 of the top view photos have been integrated into figure 8 in the revised version of the manuscript

Tables:

- Table 1 was moved to the new Appendix A and is now Table A1
- Table 2 was moved to the new Appendix B and is now Table B1
- In the new Table 1 in the revised version of the manuscript, a new column "T_{post}" was added and the last column "Comments" was deleted

Minor changes were made additional to the suggestions by the reviewers:

- we change the word “analog” to “analogue” throughout the manuscript
- we replaced the word “ductile” by “viscous”, because the latter is more accurate to describe the fluid-like material behavior of silicone and salt and is more widely used in analogue experiments of salt tectonics. Consequently, all variables: h_d, S_d, etc. were replaced by: h_v, S_v, as well.
- Color maps used for displacement and strain patterns in figures 3 to 10 were changed

2. Reply to review 1 by Gaël Lymer (https://doi.org/10.5194/se-2021-17-RC1)

- Line 17: Jackson and Hudec, 2017; You could also refer to Vendeville B.C. and Jackson M.P.A., 1992. The rise of diapirs during thin-skinned extension. Marine and Petroleum Geology, 9: 331–354 ; 25 yrs before Jackson and Hudec, 2017.

>> Reference was added.

- Line 49: "The oppositely acting processes of gravity spreading and gliding in SBRB provoke the question" > Arise? Raise? Ask?

>> Sentence was rephrased

The question as it is in the text feels to me that it has been answered in works by Vendeville, 1987; Vendeville and Cobbold, 1987; Cobbold et al., 1989, 1995; Brun and Fort, 2004: Gravity gliding process can occur when the base of a mobile salt layer is tilted by an angle as low as 1°. Maybe rephrase or specify "for such a case"?

>> The references listed above focus on passive margin salt tectonics, where relatively thin overburden sediments occur in downslope regions. In contrast, we focus on rift basins in which thickest sediments occur in the basin centre. Thus, results of these previous works cannot be fully translated to the case presented in our study. We rephrased the question:

"The oppositely acting processes of gravity spreading and gliding in SBRB prompt the question of which geological configurations have to be fulfilled to initiate gravity gliding in the case of SBRB, i.e. which minimum topographic gradient and basin slope is required?"

- Line 62: "However, the influence of basin-wide tilting of the subsalt basement and, hence, effects of gravity gliding, on the evolution of supra-salt sedimentary structures and salt flow pattern have not been investigated yet."

You cite my work line 58 (Lymer et al., 2018) and I thank you for your interest in my research. This article investigated the effect of gravity gliding triggered by tilting of sub-salt basement on the evolution of geometry and structures of the supra-salt layers, thus dealing with the "effects of gravity gliding on the evolution of supra-salt sedimentary structures", although not as thoroughly as your study, hence maybe you could nuance the sentence above (L62)? :)

>> We are aware that tilting of a graben structure was modelled in Lymer et al., 2018. We rephrased to sentence slightly to emphasize that we address basin-wide tilting.

- Table 1: Your average values in Table 1 are systematically different than those I obtain, for example for max offset of sub-salt faults I obtain 2400 vs 2200 for you, which is still ok, I assumed you rounded the result? But I do not get how you obtained 60000 km for the average of the width of the flanks (I obtain 1328 from your data)? There are also inconsistencies between table 1 and the text lines 74-87 that describe the table. For example, average salt layer is 1500 m in table, stated 1800 m in text, whereas I obtain 1637 m. Please check consistency between table and text and clarify these values.

A question arising from this: For the width of the flanks, did you use 0.6 in your model (for 60000 in nature) and if so why?

>> The reviewer is correct. Some mistakes happened when inserting the values into the Latex table. We checked the values for each basin again and corrected the, if necessary (see red values in the marked version of the manuscript). Furthermore, we changed the parameter "Width of the flanks" to "Width of the basin". The graben flanks cannot be clearly distinguished from the central graben structure in some basin. Thus, the width of the entire salt basin is more meaningful.

Furthermore, we corrected the related values in the text to be consistent with the Table. The table was moved to the appendix (Appendix A in the revised version of the manuscript) to save some space in the main text.

- 3 Method - The apparatus and scaling of the models are well presented, the relationship between model and natural scales are well defined for a large range of parameters; they are in agreement with typical other studies and seem to fall within natural ranges of parameters rates. However, I think it would be good to clarify how you designed the dimensions to the model in link with your table 1 (see question above).

>> We agree and added a sentence in the first paragraph of section 3.1.

- Line 208: "We choose intermediate rates 1mmh−1 for practical reasons so that a simulation duration of several hours to a few days is achieved." I was about to ask for further discussion on effect of rate of extension but lines 366 & 437 suggest that it is in your plans; I am looking forward to it :)

>> Yes, we are working on that topic and plan to perform a parameter study in future.

- 4 Results - If there is a room in the figures presenting the different experiments (§, 7, 8, 9, 10, 12), I think it would help the reader to show a schematic cross section of the model geometry, maybe at the beginning and the end of experiment?

>> We agree and added a cross section view of each experiment, which illustrate the final deformation structures and the topography. This cross section will help the reader to better understand and interpret the top view displacement and strain patterns.

-Line 356: "the the graben centre." delete "the"

>> Word was deleted

- Line 356: "We suggest that basin-margin fault zones and shortened graben fault zones, in particular if they are active subsequent to the rifting, are diagnostic indicators for the influence of gravity gliding on the structural dynamics in natural rift basins."

This phrase has been a little confusing to me (e.g.; "shortened graben fault zones" could refer to fault inversion for some people), maybe reword? Also, I wonder how an observer could identify a fault zone that has been shortened and thus use this as a diagnostic indicator for the influence of gravity gliding. Can you provide clues?

>> "shortened graben fault zones" might be indeed laterally compressed extensional faults zones. At the beginning of the model evolution, the cover next to the graben is extended. Later, during increased flank tilting, these extended zone is shortened due to gravity gliding. Thus, clues for gravity gliding could be thin-skinned inverted normal faults in downslope regions of the rift basins. Nevertheless, we rephrased the sentences to emphasize that we mean thin-skinned shortening in the supra-salt overburden.

- Line 359: "The experiment with syn-kinematic sedimentation (ETS1) demonstrates that the ability of downward gliding is reduced by sediment accumulation in the basin centre. In particular during the post-extensional phase, gravity driven deformation decreased rapidly after the first post-extensional step of sand accumulation."

This is similar observation than for gravity spreading (Rowan, M. G., F. J. Peel, and B. C. Vendeville, 2004, Gravity-driven fold belts on passive margins, in K. R. McClay, ed., Thrust tectonics and hydrocarbon systems: AAPG Memoir 82, p. 157–182) and could be worth mentioning as these gravity-driven mechanisms are essentially similar.

>> We agree and added a sentence citing this reference.

- *Please move figure 14 to main text (currently after the references).*

>> Latex does this automatically. This will be fixed during final editing of the manuscript

3. Reply to review 2 by Frank Zwaan (https://doi.org/10.5194/se-2021-17-RC2)

- *Perhaps this is a bit nitpicking, but the authors present the set-up as a novel method and while it is true that (to my knowledge) this specific set-up has never used before, it is in fact a combination of techniques used in previous models. The authors then go on to test a quite a couple of parameters and do a comparison with natural examples. So I was wondering the focus should really be shifted a bit to the new set-up, or rather on the various parameters tested in the models?*

>> Our main intention with this paper was to show the functionality of the apparatus, i.e. tectonic extension + tilting + sedimentation. These processes were simulated and compared. The presented experiments reflect the main processes, which can be simulated with this apparatus and not necessarily the effect of realistic parameters. The investigation of specific parameters, e.g. which amount of tilting is necessary to induce gravity gliding, will be the main focus of our next studies. Therefore, we would like to maintain the focus of the presented study on the technical aspects of the apparatus.

We added a sentence at the beginning of section 3.1 stating that our apparatus represents a combination of approaches modelling with previous analog modelling setups.

- *The set-up itself is very nice I think, and the experimental results very clear and interesting. However, what is really missing is a thorough description with detailed information on the model parameters/set-up. Now there is a lot of info on the scaling and sedimentation (see also next points), but crucial information such as model duration, extension/subsidence rates, thickness of sedimentary infill, angle of the basement faults, inclusion of a post-rift phase etc. are not really mentioned in the text I believe, and only provided in table 3. (NB: I noticed that model durations seem to vary form model to model, and are not always consistently reported.) I think these details should be expanded a bit as it remains a bit vague how the models are actually constructed and colleagues would not be able to reproduce the models.*

>> We agree and added more information in the chapter 3.1 explaining details of the apparatus and the setup. Furthermore, we added some columns in Table 3 (now Table 2, list of experiments) and an additional paragraph in chapter 3.4 “Experimental Procedure” specifying important parameters of the experiments.

- *Very importantly, it is not 100% clear to me what the initial geometry of the PDMS layer is. Fig. 2 shows a deformed state with a silicone layer that pinches out towards the sides. What is the extent of the salt layer (I noticed that there are static borders in the DIC results), what is the thickness of the salt/silicone layer? In table 3 it says that the layer thickness is between 5 mm and 3 cm. Is this the same in all models? (I would assume so) and what would the distribution of these thicknesses be? The same goes for the brittle layer (ranging between 1 mm and 3 cm?, where fig 2 shpws it pinching out to the sides as well). Confusingly, different (the correct?) values are presented in Table 3… It would probably be good to include a figure with the initial layer thicknesses, and please be consistent with these details thoughout the manuscript to avoid confusion.*

>> We added a sketch in Fig. 2b showing a schematic cross section view of the initial shape of the silicone layer and the sand cover.

>> In Table 3, it is written that the initial sand cover layer h_b is always 3mm for all experiments. The initial thickness of the ductile layer h_d (now h_v) means the maximum thickness in the centre of the model. Yes, the ductile layer pinches out towards to edges of the flanks. We specified this in the caption of Table 3.

Furthermore, we divided the column "T" into two columns T_{syn} and T_{post} specifying the duration of the syn-defomration phase (extension and/or tilting) and the post-deformation phase. Thus, it should be clear now how long each phase was in each experiment.

- *There is a rather long section on model scaling. Now I agree that this is very important, but this part is not very new I believe (?), and perhaps distracts a bit from the main point: the new set-up/models. It might be better to shorten it a bit or perhaps move part of it to the appendix? (NB: I think it would be good to keep it in the main paper and not in the a supplement)*

>> We agree with the reviewer that the scaling chapter is long and contains many detailed information, which are not required to understand the modelling procedure and the description of the results. Thus, we moved the long version of the scaling (including Table 2 and Figure 4) to the digital appendix (now: Appendix B) and left a short version of the scaling chapter in the main text of the manuscript. In this short versions, we summarized main parameters and scaling ratios required to understand the models.

- *There is also a section on sedimentation and using different densities to reproduce (more) realistic sedimentary loading. However, this is not addressed any more in the results or discussion. Only in the conclusion it is mentioned again. If this is indeed an important part of the model series or set-up, I would expect some comparison of the impact of different sediment densities on the models. This is however missing. I also understand that this concept is already tested by previous modellers? (Dooley & Hudec?) Perhaps it would therefore be better to move the detailed description to the appendix as well?*

The density stratification was not discussed further, because it was applied only in one experiment and it is rather considered as general approach, which we would like to vary in future experiments. Although this effect of density inversion is considered in Dooley & Hudec (2020), our approach is more detailed and supported by exact quantifications of the bulk densities and related mixing ratios of the granular materials. Thus, we think it is an important part of the coherent experimental approach. However, we agree that this detailed description distracts from the main aim of this study. Thus, we moved main parts of section 3.4 into the digital appendix (now: Appendix C) and left a short description of this topic in section 3.5 (now: 3.4 experimental procedure).

- *I think that by moving parts of the scaling and the sedimentary infill to the appendix, there would be a bit more focus on the set-up itself (now, the key parts are really a bit separated by the scaling and sediment description). And as stated before the description of the set-up should really be expanded a bit to include more of the important technical details that I think are missing.*

>> We moved the section scaling and density stratification to the digital appendix and extended the section 3.4 Experimental procedure.

- *Extension is forced by the downward moving central block, which pushes apart the "rift shoulders". I assume that this central block is controlled by motors? This is not clearly specified I believe. In the results I also noticed that the motion of the "rift shoulders" is a bit "shaky" (which is addressed in the discussion, but should also mentioned in the results part I think, it is very obvious). I figured a solution would be to also have motors control the horizontal motion of both rift shoulders, to ensure a smoother deformation?*

>> We added a brief description in section 3.1 to explain how the basal parts of the apparatus are controlled and moved. Furthermore, we added a comment in the results about the shaky lateral movement of the graben flanks. Adding more motors could certainly be a possible solution to fix this technical problem. We will consider this for future experiments.

- *Since the rift shoulders are simulated by a bendable plate, does the force of the central block moving down cause flexure? And what kind of materials is used for building the set-up? The type of material surely influences the degree of bending, which would surely affect gravity gliding. I could also imagine the weight of the model materials (and the extra weight of sediments) would affect the bending of the plates. How is this checked?*

>> The parts of the „rift shoulders" close to the graben consist of solid steel supported from below so that the downward moving graben block does not cause any flexure. The bendable plates begin roughly 2 cm next to footwall graben blocks and consist of 2mm thick steel plates, which are additional supported by 2mm thick stripes of steel tied to the base of the plates. It was tested that these thick steel plates are only insignificantly bended by loading of analog materials while the plates are still flexible enough to be bended by the force of the motors at their edges. To clarify this, we added a sentence in section 3.1.

- *A general remark on the thermal sag applied in the models: Would it not be better to first form the rift, and have the sag act after rifting? During rifting, one would expect rift shoulder uplift instead, which would counteract the riftward tilting induced in the models I'd say? Could also the angle of tilting in the models be specified? This might be a rather crucial detail to interpret the results.*

>> We agree with the general remark by the reviewer that thermal subsidence usually follows rifting and is not simultaneous to it. However, we introduced the geological configurations of many SBRB in Tab. 2 (now Tab. A1 in Appendix A in the revised version of the manuscript) for that reason. These examples show that in most cases, there was a pre- or syn-salt phase of rifting, which supposedly was followed by thermal subsidence. Furthermore, deposition of thick evaporites results in loading induced, wide-spread subsidence. Hence, we assume that the early post-salt history was normally characterized by thermal sag basin subsidence interrupted renewed rifting. For that reason, we found it more realistic to simulate rifting and tilting simultaneously in the simple experiments presented in the manuscript. However, we mentioned at the end of the discussion section 5.3 that in future, advanced models, we will investigate differences in timing of extension in relation to tilting. We specified this point in an additional sentence at the end of section 5.3.

- *Analysis: perhaps it should be mentioned what the frequency of photography is. It should also be mentioned that sections were made (and how, I assume by simply soaking the sand and cutting by hand?)*

>> We added these information in the description of experimental setup section 3.1 and procedure section 3.5.

- *In general the style of writing is very pleasant, but sometimes it is a bit too efficient I think. See for instance the model set-up description I mentioned above. I also think the result section could be expanded a bit. As it is, the authors do not really introduce the models and their general characteristics, but directly describe detailed results. I think there could be a couple of extra sentences to help the reader "get used to" the models. Here and there it would also be good to remind the reader about the parameters used in specific models. Also, it might be nice to spend a bit more attention on the evolution of structures (e.g. the faults, I noticed that these may be moved, i.e. translated downslope, as gravity gliding occurs).*

>> We agree and added a few more information in the description of each experiment, where it is appropriate. In particular, section 4.2 in which we compare displacement and strain patterns between the experiment with and the exp. without tilting, we rephrased the text and added more and specific information about the experiments and the related figures.

- *I felt that it could be useful to finish the results section with a summary figure of the main observations. (perhaps simple section views with the main processes indicated?) This could serve as a key figure and a framework to refer to in the discussion.*

>> We agree with this idea and drew a new figure (now: Fig. 12 in the revised version of the manuscript) with 3D sketches of the main experiments (those with a silicone layer) illustrating strain patterns and structures in a simplified manner.

- *The discussion seems to lack some comparison with previous models (e.g. the ones mentioned in the introduction). Discussion section 5.1 is indeed a bit short. I would suggest adding some as especially the gravity-gliding without sedimentation looks like what one finds in models of passive margin style salt tectonics. Some of the other models have features (e.g. flexure of overburden over a normal fault) that may also be find in analogue modelling studies of rifting? E.g. Withjack & Calloway (2000) in AAPG Bull. or perhaps the work by Tim Dooley.*

>> We agree and rephrased parts of section 5.1 to discuss some aspect of the experiments in more detail and compare our experimental results with previous modelling studies, in particular those of passive margin salt basins.

- *The comparison with natural examples was a bit hard to follow. Perhaps such a summary image of the results could help to illustrate the points made there a bit better (e.g. how compressional supra-salt structures can be "hidden" by sub-salt deformation).*

>> We agree and rephrased large parts of section 5.3 "Application to nature and outlook" in order to better relate the specific natural example to the experimental results. Furthermore, we exchanged 3 subfigures in Fig. 14 (now Fig. 13 in the revised version of the manuscript) to show examples which better fit to the argumentation in the text.

Figures:

- *I really like the side-by side presentation to show the effects of specific parameters. But would it be possible to systematically include a (final) top view of the models as a first row of each column, to show what each model looks like? The same for sections, these could be easy to add and be very insightful, if available. And surface evolution graphs, or final topography profiles might also be nice to add.*

>> Unfortunately, final top view images are not available for some experiments. In case of the experiments without silicone layer, such images are also not illustrative and only show a subsided graben structure and/or the tilting flanks. However, as suggested by reviewer 1, we added sketches of cross sections of each experiment showing the final structures. These also include profiles of the final topography. We think that these are more illustrative than top view images and help the reader to understand and interpret the shown top view displacement and strain patterns.

- *In some models, strain plots are shown as well. Could these be provided for each model? This could allow a more detailed description. Now it may seem that some models are perhaps not fully explored, which would be a pity.*

>> We think that adding images of the strain patterns would overload the manuscript. Strain patterns of experiments without a silicone layer are relatively simple and not very illustrative, because they only show straight extensional strain above the graben faults (exp. E1 and ET1). In the experiment including a silicone layer and with only tilting (T2), extensional fault zones occur at the basin margins. These strain patterns are now illustrated in a new summary Figure 12 in the revised version of the manuscript. Strain patterns of the most interesting experiments E2, ET2, ETS2 are already included in the manuscript and we think that these are sufficient to explain the main observations of the experiments. Thus, we did not include strain pattern figures of all experiments in the main manuscript.

- *Please note that the text in the figures is really very (too) small. It is hard to read when printed. Please make the text larger. Perhaps it would also be nice to add some headers/labels to the figures so that it's directly clear what is presented, it took me a moment to see what is what (also due to the small text I guess).*

>> We agree and increased the font size in all figures. Furthermore, titles were added above each subfigure in all figures.

- *As said, I think that a summary figure might be a good addition to the paper.*

>> A new summary figure was included in the revised version of the manuscript.

- *14 seemed missing, until I found it at the very end of the manuscript.*

>> That will be fixed during final editing of the manuscript.

4. **Reply to review 3 by Oriol Ferrer (https://doi.org/10.5194/se-2021-17-RC3)**

Section 1: Introduction.

Line 46: *Include also the reference Stewart, 2014.*

Stewart, S.A. (2014). Detachment-controlled triangle zones in extension and inversion tectonics. Interpretation, 2 (4), SM29-SM38.

>> Reference was added

Line 51: *"prompt the question of which" instead "provoke the question"?*

>> Words were changed.

Line 58: *Include also the reference Roma et al. 2018.*

Roma, M; Ferrer, O; McClay, K.R; Muñoz, J.A; Roca, E; Gratacós, O; Cabello, P. (2018). Weld kinematics of synrift salt during basement-involved extension and subsequent inversion: Results from analog models. Geologica Acta, 16 (4) 391-410 DOI: 10.1344/GeologicaActa2018.16.4.4

Roma, M., Vidal-Royo, O., McClay, K., Ferrer, O., Muñoz, J.A. (2018). Tectonic inversion of salt-detached ramp-syncline basins as illustrated by analog modeling and kinematic restoration. Interpretation, 6 (1), T127-T144. http://dx.doi.org/10.1190/INT-2017-0073.1.

>> We added the second reference, since this best fits to content of the related sentence.

Line 65: *"work, paper or article" instead "project".*

>> Word was changed to "work"

Line 66: *We designed a new "experimental" apparatus…*

>> Word was added

Line 70: *Please remove "of the presence", not necessary in the sentence.*

>> Words were deleted

Figure 1: *May be is preferable to use "rollers" instead "roll-over" in Fig. 1d.*

>> Fig. 1d has been exchanged and the word roll-over is not included anymore.

Figure caption 1: *(c) Upper Triassic showing "tilted" flanks (instead inclined?).*

>> Word was changed in the new Fig. 1c

(d) change "rollovers" by rollers. If not, please be consistent with the style used in Fig. 1d (roll-overs).

>> Fig. 1d has been exchanged and the word roll-over is not included anymore.

(d) as well as "diapirs". An "i" is missing.

>> Fig. 1d has been exchanged and the word “diapir” is not included anymore.

(e) “Conceptual sketch” of the experimental setup applied in “this” study.

>> Words were rephrased.

Section 2: Geological prototype and experimental setup.

There are some inconsistencies between the values indicated in the manuscript and those in Table 1. Please check and modify if necessary.

>> This point was also criticized by reviewer 1. Values were checked and corrected. The table was moved to the appendix (Appendix A in the revised version of the manuscript) to save some space in the main text.

In Table 1, please introduce the reference O’Sullivan et al. (2021) for the Slyne Basin.

O’Sullivan, C. M.; Childs, C. J.; Saqab, M. M.; Walsh J. J.; Shannon, P. M. (2021). The influence of multiple salt layers on rift-basin development; The Slyne and Erris basins, offshore NW Ireland. Basin Research, https://doi.org/10.1111/bre.12546.

>> Reference was added in the table

Is correct the value of 60000 km for the width of the flanks in Table 1? Please check it.

>> Value was corrected

Please check “Parentis Trough” for “Parentis Basin” in Table 1.

>> Term was changed to Parentis Basin

Table 1: for Central Graben, please use also capital letters for “Central” and “South”.

>> Word were corrected.

Section 3: Method

Line 90: *How was controlled the vertical movement of the central graben structure? Gravitationally as bendable plates were pushed or with a motor? Please explain it.*

>> We added a short explanation in section 3.1 experimental setup

Line 93: *“tilted” instead “inclined”?*

>> Word was changed

Line 96: *According to the Figure 2, the y-axis correspond to the width of the apparatus and z-axis to the depth. Please, check the text and modify. In this line you also indicate the width of the experimental apparatus is 60 cm, but the number that appears in figure 2b is 50 cm. I understand this last value refers to the effective width of the model without the width of the two lateral sand walls. This is not clear in the text and should be better explained.*

>> We improved this in Fig. 1 and added a few sentences in section 3.1 to explain the shape of the silicone layer.

Lines 109 to 111: *Could you explain a little more the proportions of quartz sand and silicate cenospheres mixtures? Which were these mixtures? Why did you use different mixtures and not the same? Please explain it.*

>> It is mentioned in section 3.2 "experimental materials" why we used different mixtures. We added a sentence to explain this further. The detailed explanation of the density and the granular mixture was moved to the Appendix C in the revised version of the manuscript. We added a short description, how we determined the appropriate mixing ratio. Furthermore, we added a reference to Appendix C in section 3.2.

Line 172: *Maybe you can also use the work of Carter & Hansen (1983) as a reference.*

Carter, N. L. and Hansen, F. D., (1983). Creep of rocksalt. Tectonophysics, 92, 275–333, https://doi.org/10.1016/0040-1951(83)90200-7.

>>Reference was added.

Section 3.5: Experimental procedure

The number and/or the name of the experiment are used indistinctively both in the manuscript and in table 3. Personally, I think the name of the experiment is something that as a modeler we use to organize our experimental program. Nevertheless, I suggest to use just the number of experiments in manuscripts because it greatly simplifies reading and understanding of the article.

>> We think the name of the experiments are useful for easily recognizing the modelled parameter in each experiment. Thus, we prefer to keep the experiment names in the text. Nevertheless, we changed their names according to the suggestion by reviewer 2.

Table 3: *Please, use the same units in all the table. Change cm for mm in the thickness of pre-kinematic sand layer and thickness of the polymer.*

>>We changed all units to mm

Line 266: *"sedimentation" instead "accumulation".*

>> Word was changed

Line 269: *"syn-kinematic sand is sieved on the model surface". Did you label the sand after sieving? How? I noticed a positive surface elevation over the basin in Fig. 9 a and b. Is this because the surface of the experiments was not levelled after the sedimentation of each syn-kinematic layer?*

>> We explained the sieving procedure in some additional sentences in section 3.4 experimental procedure in the revised version of the manuscript.

We did not levelled the sand surface until it was completely even. Instead we filled up the depocentres above the graben and added ~1mm of sand uniformly on the graben flanks. The total volume of the added sand was the same in each sieving interval, whereas the density of each new sand layer was increased during each interval. We admit that this procedure is probably not the most accurate, but different sieving procedures could be test in future experiments.

Section 4: Results

Line 288: *movement of the cover layer… Please, remove layer. Not necessary here.*

>> Word was deleted

Figure caption 6: *At the end of the (e/f) explanation, please add "for experiment E1 and E2 respectively."*

>> Words were added.

Figure caption 7: *At the end of the (e/f) explanation, please add "for experiment E1 and E2 respectively."*

>> Words were added.

Figure caption 8: At the end of the (e/f) explanation, please add "for experiment E1 and E2 respectively."

>> Words were added.

Line 326: *"This implies that sequentially filling of the downslope depocenter with syn-kinematic sand significantly reduces downslope glidding of the cover". à Equivalent observations were done by Rowan et al., 2004 and Rowan 2020.*

Rowan, M.G.; Peel, F.J.; Vendeville, B.C. (2004). Gravity-driven fold belts on passive margins. In: K.R. McClay (Ed.). Thrust tectonics and hydrocarbon systems. AAPG Memoir 82, 157-182.

Rowan, M.G. (2020). Salt- and shale-detached gravity-driven failure of continental margins. In: Scarselli, N.; Adam, J.; Chiarella, D. (Eds.). Regional Geology and Tectonics: Principles of Geologic Analysis. Elsevier, 894 pp.

>> We added the first references (Rowan et al., 2004) in new sentences in the discussion at the end of section 5.1

Figure 11: *Fig. b looks more like an overhead detail than a perspective or oblique view. Please, change it in figure caption.*

Section 5: Discussion

Line 356: *Please, remove one of the two "the" at the beginning of the sentence.*

>> Word was deleted

Lines 359-360: *You should refer the work that I have commented in line 326 of my review.*

>> We added these references in new sentence in the discussion at the end of section 5.1

Line 375: *Are you referring to inherited pre-salt relief? Please specify their origin as Dooley et al. (2017) or Ferrer et al. (2017) did. Other works have recently studied the effect of base-salt relief on the dynamic of salt-bearing passive margins and should be also included in this section of the discussion:*

Ferrer, O., Gratacós, O., Roca, E., Muñoz, J.A. (2017). Modelling the interaction between presalt seamounts and gravitational failure in salt-bearing passive margins: The Messinian case in the northwestern Mediterranean Basin. Interpretation, 5(1), SD99-SD117. https://doi.org/10.1190/INT-2016-0096.1

Pichel, L. M., Jackson, C. A.-L., Peel, F., and Dooley, T. P. (2020). Base-salt relief controls salt-tectonic structural style, São Paulo Plateau, Santos Basin, Brazil: Basin Research, v. 32, no. 3, p. 453-484, http://doi.org/10.1111/bre.12375.

>> We slightly rephrased the sentence specifying the origin of a base-salt relief and added the proposed citations.

Figure 14: *Please move this figure before Conclusions chapter.*

>> This is done automatically by Latex and will be fixed during final editing.

Line 397: *A nice field example that maybe you can include in the application to nature and outlook section is the Cotiella Basin in the Pyrenees. This is an outstanding and incredible outcrop example of the structures developed in the outer rift basin during the post-rift stage. It might be interesting to take a look to the works of Lopez-Mir and co-authors.*

López-Mir, B., Muñoz, J.A., García-Senz, J. (2015). Extensional salt tectonics in the partially inverted Cotiella post-rift basin (south-central Pyrenees): structure and evolution. International Journal of Earth Sciences, 104, 419-434.

López-Mir, B., Muñoz, J.A., García-Senz, J. (2016). 3D geometric reconstruction of Upper Cretaceous passive diapirs and salt withdrawal basins in the Cotiella Basin (southern Pyrenees). Journal of the Geological Society. 173, 616-627. doi:10.1144/jgs2016-002

>> We agree and are aware of this example. We exchanged the example of the Parentis Basin by Jammes et al., 2020 (Fig. 14b in the old version of the manuscript) with a new cross section showing growth faults at the margins of the Pyrenean rift system by Lopez-Mir et al. (2014). Furthermore, we added a description of the Pyrenean rift system the main text of section 5.3.

(Lopez-Mir, B., Muñoz, J. A., & Senz, J. G. (2014). Restoration of basins driven by extension and salt tectonics: Example from the Cotiella Basin in the central Pyrenees. *Journal of Structural Geology*, *69*, 147-162.)

References

I have not located the following works from the reference list in the text of the manuscript. Please check.

Rudolf et al. (2015) and Vendeville (2005).

>> The citations were added to the reference list:

- Vendeville, B. C. (2005). Salt tectonics driven by sediment progradation: Part I—Mechanics and kinematics. *AAPG bulletin*, *89*(8), 1071-1079.

- Rudolf, M., Boutelier, D., Rosenau, M., Schreurs, G., & Oncken, O. (2016). Rheological benchmark of silicone oils used for analog modeling of short-and long-term lithospheric deformation. *Tectonophysics*, *684*, 12-22.