Dear Editor,

Please find below our response to the reviews by Nemanja Krstekanic and Marco Bonini. Following your suggestion, we now include studies by Cruden et al. (2006) and Rosenberg er al. (2007) in the discussion.

Best regards,

Benjamin Guillaume

Reviewer 1

We would like to thank Nemanja Krstekanic for his constructive comments and suggestions. They helped us improve the clarity of the manuscript and to develop the discussion on both the orientation of strike-slip faults and the role of the amount of deformation. Our replies to the specific comments (in italics) are given point by point below:

General comments

In this manuscript, the authors use crustal-scale analogue modelling to study a complex tectonic system in which indentation-driven and extrusion-driven deformation overlap in space and time and result in different coeval tectonic regimes. The topic of this research is very welcome as there is still a lack of knowledge on various controlling factors of such processes' interplay. The manuscript is well structured and written, scientifically very valid, with a clear description of the methodology, results, interpretations and conclusions. The title is informative and reflects the content of the manuscript, while the language is good. Taking all of that into account, I consider it a nice contribution to Solid Earth.

I have a few moderate to minor comments that I'll point out below. Also in the attached annotated pdf of the manuscript, I have smaller comments that I hope will help the authors clarify a few minor things in the text.

Specific comments

1) *Referencing existing publications is generally very good in the manuscript. However, I would suggest to slightly expand the comparison with existing studies of the complex interplay of different tectonic regimes, both in the Introduction when introducing the studied problem and in the Discussion when comparing to the novel results of this paper. Several relatively recent papers deal with indentation and extrusion or interplay of different tectonic regimes, using both analogue modelling and field data. See also annotated pdf.*

Reply: We now refer to additional modeling studies in the Introduction, in particular those by Duarte et al. (2011), Rosas et al. (2012, 2015) on thrust-wrench interferences, by Krstekanić et al. (2021, 2022) on backarc-convex orocline formation, by Philippon et al. (2014) on the indentation of Arabia, lateral escape of Anatolia, and backarc extension in the Aegean region, by van Gelder et al. (2017) on the lateral escape and extension in the eastern Alps, and by Corti et al. (2006) on strain distribution along the Maghrebides-Apennines accretionary prism and the Sicily Channel rift. In the discussion section, we have included additional comparisons of our experiment results and set-up with previous studies by Corti et al. (2006), Duarte et al. (2011), Rosas et al. (2011,2015) as suggested by the reviewer, and also with 3D numerical models by Le Pourhiet et al. (2014).

2) *Section 4.1: While I generally agree with the content of this section I think it is too long and can be shortened. Also, this section would apply more to the homogeneous system, while in all models in this study there is a rheological and/or structural heterogeneity, which, in my opinion, significantly influences the deformation. It is not only the distance to the model margins (i.e., indenter and extrusion-related pull). I think the limitation of the modelling setup (i.e., relatively low amount of total shortening) has an impact on the evolution of thrusting, as thrusts will form after a certain accumulation of shortening. I think it is not only the extension/shortening ratio but the total accumulation of strain that plays an important role. This issue should be discussed more in this section. Another factor that should be taken into account is the compressional wedge that forms close to the indenter. This wedge increases the vertical load in the model, therefore increasing the vertical stress, which significantly affects the distribution of stress and strain in the model. I think all these factors should be considered and better discussed in this section. So, my suggestion is to modify section 4.1 to make it more concise and focused, while discussing all factors that affect the tectonic regime(s) in the models.*

Reply: We agree with the Reviewer that the accumulation of strain can play an important role on the distribution of deformation within models. To be fair, we already indicated the possible role of the limited amount of shortening that we are able to impose in the model to explain the lack of thrust faults for brittle-ductile models. We now also indicate that it could be the case for brittle models with high V_e/V_s ratios. We also discuss how the accumulation of material in the wedge during deformation may modify the distribution of stress and location/type of structures over time:

Line 304: *"Progressive increase in the southern wedge thickness accompanying N-S shortening in the absence of erosion would imply an increase of the vertical stress. As a consequence, thrust faults would propagate toward the north (as evidenced between 4.2 and 7.7% of shortening for model BI05; Figs. SM1C and 4C) and could possibly reach places where strike-slip faults and normal faults were previously active. Therefore, the redistribution of stress resulting from deformation could drive temporal variations of the tectonic regime at a specific location."*

Line 315: *"However, we cannot preclude that thrust faults could also develop at later stages for these models with high V_e/V_s ratios, as experimental limitations prevent us from imposing large amounts of N-S shortening. In model BI07 for instance it is at maximum 3.6%, which may be insufficient to locate deformation along an E-W thrust fault."*

3) *Orientation of strike-slip faults: I made several comments in the annotated pdf about the change of strike-slip fault orientation as this is one of the important results of this study. Please consider that some of them can be boundary effects, or that some of them are indentation-driven or extrusion-driven. This last terminological distinction can be considered, but it is just a suggestion, authors do not have to accept it. Anyway, I think a bit more discussion about what controls the strike-slip fault orientation is needed in section 4.2.*

Reply: We completed section 4.2, now adding a discussion on the parameters that may control the formation of the high angle strike-slip faults that develop at model boundaries, including boundary effects. However, we still think that the orientation of these faults is also controlled by the formation or pre-existence of a graben in the center of the model, as indicated by the variability in orientation of these strike-slip faults within the same model, or between models with or without a former stretching phase:

Line 336: *"These anomalously oriented strike-slip faults could result from a combination of factors. As they nucleate from the edge of the model, we cannot exclude that they result from some unwanted boundary effects associated with the high friction wall-sand interface. As a result of the applied stretching and boundary effects, the maximum principal stress σ_1 may have rotated from a N-S direction toward a NE-SW direction in the eastern part of the model and NW-SE direction in the western part of the model, possibly explaining why these strike-slip faults do not lie at 30° with respect to the imposed N-S shortening. However, one can also notice that not all of these strike-slip faults have the same exact orientation (Figs. 4D and 4E), some of them being directed toward the northward termination of the normal faults bounding the central graben. Their orientation therefore could also be controlled by the graben structure that forms in the center of the model above the crustal seed."*

Line 347: *" While we cannot preclude some boundary effects here too, these faults with a larger than expected angle with respect to σ_1 also connect with the normal faults bounding the central graben formed during the initial stage of stretching. In comparison, model BI05, which shares the same stretching/shortening ratio and almost the same amount of total stretching (~10%) does not show any anomalous strike-slip faults in the southern part of the model (Fig. 9A). Pre-existing structures may also exert a control on the geometry of subsequent structures even for areas close from where shortening is applied. "*

Line 357: *" In particular, at model corners, strike-slip faults bend with angles that become larger with respect to the N-S shortening direction, possibly indicative of some boundary effects. More interestingly, some strike-slip faults are oriented almost parallel to the shortening direction in areas that were previously affected by normal faulting (Figs. 6 and 9C). "*

4) *Referring to figures should be stronger in the text. I suggest to authors to refer to figures more often. This will make the connection between the text and figures much stronger and will help readers to follow the text more easily.*

Reply: We followed the reviewer's suggestion and refer more often to figures in the manuscript.

5) *Figures are generally good and informative. However, I have a few suggestions on how to improve them:*

   Maybe it would be good to have an additional figure (maybe new Fig. 1) to accompany the problem statement and to illustrate the processes and some natural examples mentioned in the Introduction.

Reply: We have added a new introductory figure (new Fig. 1) with maps highlighting zones of active or past coeval activity of tectonic regimes.

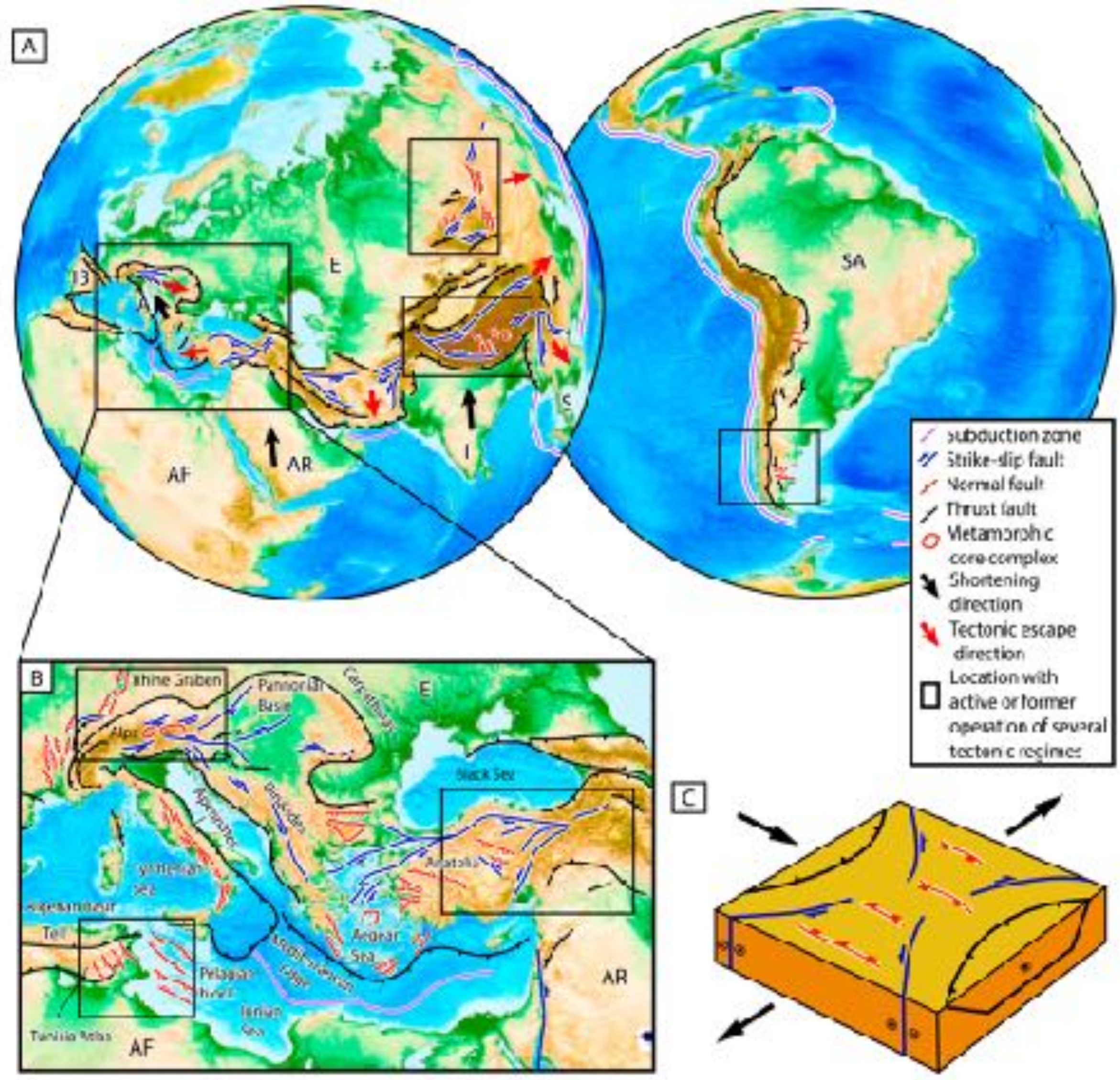

Fig. 1. A) Geological sketch maps showing locations with active or past multiple coeval tectonic regimes (Tapponnier et al. , 1982, 2001; Davy and Cobbold , 1988; Martinod et al. , 2000; Fournier et al. , 2004; Scharf et al. , 2013; Sengör , 1976; Dèzes et al. , 2004; Corti et al., 2006; Gianni et al. , 2015). B) Close-up map showing the tectonic setting of the Alpine-Mediterranean region. Structures are modified from Faccenna et al. (2014). C) Block diagram illustrating typical structures formed in settings involving coeval shortening and extension. Abbreviations are SA:

South American plate, I: Indian plate, E: Eurasian plate, AF: African plate, AR: Arabian plate. IB: Iberian plate, and A: Adria plate.

6) *When a figure has more than one panel, I suggest putting a letter on each panel to make it clear which part of the figure authors refer to in the text (e.g., Fig. 3c). Panels in figures 3, 4, 5, 8 and 13 are too small and it is difficult to read them. Try to make panels larger.*

Reply: We followed the reviewer's suggestions and lettered panels in the revised figures. We also modified figures in order to make them as readable as possible without losing information.

7) *I understand why it is important to show plots of principal stretches because they are used to derive strain type. However, these plots are not necessary here and are not discussed in the text. They also take space that can be used to make other panels larger. I suggest moving principal stretches plots from figures 3, 4, 6, 7 and 9 to Supplementary Material and maybe combining figures 3 and 4 and also figures 6, 7 and 8. This will reduce the number of figures (which is already large), while no information will be lost.*

Reply: We followed the reviewer's suggestions by only showing interpreted pictures and strain type maps. Plots of principal stretches now appear as supplementary figures SM2 and SM3. We also reorganized figures by combining figures 3 and 4 into the new figure 3, as well as figures 6, 7 and 8 into the new figure 5.

8) *Other smaller comments about figures I added in the annotated pdf.*

Reply: We also took into consideration the other comments given in the annotated pdf.

Technical comment

1) *There are just a few typos and some technical errors I managed to see. I marked them in the annotated pdf. Otherwise, the text is technically very good.*

Reply: We took into consideration the corrections suggested by the reviewer.

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Reviewer 2

We would like to thank Marco Bonini for his constructive comments and suggestions. They gave us the opportunity to better explain the rationale of our modeling study as well as to precise some modeling technical points. Our replies to the specific comments (in italics) are given point by point below:

The manuscript by Guillaume et al. presents the results of a series of analogue models addressing the role of simultaneous shortening and orthogonal extension under different rheological conditions, including the role of inherited crustal heterogeneities. The paper is concise and well written, and figure are well drafted. In addition, the aims are clearly stated, and the modeling results are analyzed through up-to-dated techniques (Particle Image velocimetry (PIV), and subsequent velocity and strain analysis). The extent of the conclusions is generally supported by the presented data, and the results may be attractive for an international readership. The manuscript is thus suitable for being published in Solid Earth (SE) after a minor/moderate revision. The issues that should be addressed during the revision are listed below and keyed to line number in the text.

General points

> *1) Conceptual simulation of indentation and lateral extrusion. In this experimental study, lateral extrusion is achieved by applying a shortening-orthogonal extension to the model. However, in this experimental procedure the system is not developing spontaneously, but its evolution is imposed by the boundary conditions (i.e., the shortening-orthogonal extension). In other models, lateral extrusion (associated with V-shaped strike-slip systems) simply resulted from a model set-up with lateral strength/thickness variations (Sokoutis et al 2000, Tectonophysics), and/or accomplished by weak lateral confinement (e.g., Ratschbacher et al., 1991, Tectonics). The authors agree on that and acknowledge the primary role exerted by a weak crust in favouring a lateral tectonic escape (Lines 403-404). On this basis, I think that some more discussion on the rationale of the modelling and its comparison with previous models would be necessary for a more in- depth comparison with the natural cases sketched in Figure 14.*

Reply: Indeed, in our series of models, crustal lateral extrusion is controlled by applying a shortening-orthogonal extension. We made this choice for two reasons. The first one was to be able to systematically vary and precisely control the relative rate of extension over shortening. The second reason was to be able to reproduce systems in which far-field forces may also produce orthogonal stretching participating in crustal deformation. We now make it more clear in the Introduction and Procedure section:

Line 72: “While these studies provide some elements for understanding the coexistence of different tectonic regimes and associated structures, there is a lack for a systematic investigation of the role of the relative ratio between shortening and stretching rates, as horizontal extrusion may not always only result from orthogonal indentation, but may also be controlled by far-field forces leading to non-plane strain deformation.”

Line 166: "The layer(s) are then deformed by applying a constant velocity boundary condition at the edges of the model through pistons activated by step motors, which allows us to precisely control the stretching rate to shortening rate ratio."

We have also extended the comparison of our models to those in previous studies as suggested. We added a comparison of our models with those by Corti et al. (2006), Duarte et al. (2011), Rosas et al. (2012, 2015).

2) *Adopted terminology. The reasons of using the terms 'extrusion rate' (stretching velocity) and 'indentation rate' (shortening velocity) should be discussed in more detail. Indentation refers to a case where the colliding block is much shorter than the indented continent. However, I cannot identify this condition in the model setup of Figure 1. So why not use the terms shortening rate and extension rate? Please comment on this.*

Reply: In the initial set-up, the colliding block indeed appears as large as the indented continent. However, in most of the models (except BI01 and BI10), applied stretching makes the colliding block width progressively smaller than the continent. We acknowledge that the difference in size remains small, and to avoid confusion and keep things simple, we followed the suggestion of Reviewer 2 by replacing "indentation rate" by "shortening rate" and "extrusion rate" by "stretching rate" in the manuscript and figures.

3) *Rheology of analogue materials. The lower crust has been simulated using PDMS silicone putty (Lines 85-86). Consequently, this silicone has a lower density than the overlying Fontainebleau quartz sand that simulates the upper crust. As correctly stated by the authors, this implies an inappropriate density ratio between upper and lower crust in the models. Density contrast in the model should be equal- or at least similar - to nature. In other terms the viscous layer is too buoyant (or the sand too heavy). This may produce a strong vertical instability that may amplify the folding of the brittle-ductile interface during shortening or extension, and ultimately affect the modelling results at some extent. Please clarify the choice of the PDMS silicone (technical advantages?).*

Reply: The choice of using low-density PDMS silicone was made for practical reasons. Increasing the density of the PDMS silicone by adding iron powder to make it as dense or slightly denser than the sand (~1400-1500 kg/m^3) would have also modified its viscosity. It would have increased to values around 10^5 Pa.s (e.g., Fernandez-Garcia et al., 2019), which has implications on the scaling of the brittle-ductile models. Maintaining stretching/shortening rates equivalent to values in nature in the range ~1.5-6.5 mm/yr would have required imposing piston velocities as low as ~6 mm/h, which was too slow for the motors we used. We now explain better the technical reasons that led us to this choice:

Line 120: "For the two-layered models, the density ratio between the brittle and ductile parts of the crust is high (1.45). Increasing the density of the viscous layer would have resulted in a strong increase of its viscosity, which would have required applying speeds too low for the capacities of the engines used.”

We agree with reviewer 2 that such low density may favor vertical instabilities, which is now indicated in the manuscript. To further test its potential impact, we did the exercise of performing additional 2D numerical models of crustal shortening based on a modified example of the benchmarked code from Gerya (2019). We varied the initial configuration (including a seed or not) and the density of the viscous layer considering values similar to that in our analogue models (965 kg/m^3) or similar to that of the sand in our analogue models (1400 kg/m^3) (Fig. 1). For an amount of deformation in the range of our models (11% of shortening), we show that the effect of the low-density viscous layer on deformation location is almost null when a seed is present (Fig. 1A) while for models without any initial seed (horizontal layering), some differences appear, e.g., the width between conjugate thrust faults, or the localization of deformation along a single or multiple close thrust faults (Fig. 1B). However, the overall pattern of deformation remains similar. We therefore consider that the first-order results for the brittle-ductile models should not be very different with respect to models that would use a higher density viscous layer, especially for models with low or moderate amount of deformation and short duration.

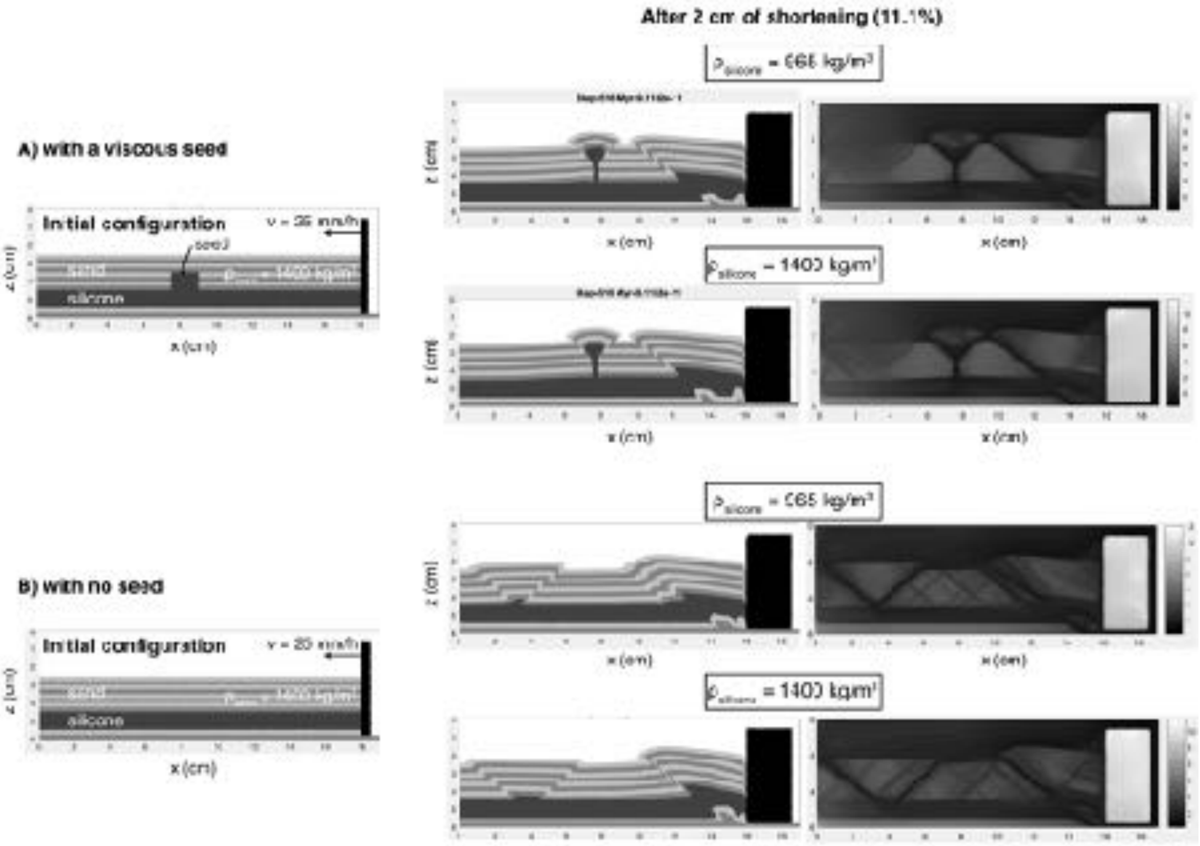

Fig. 1. Illustration of the influence of the density of a viscous layer in 2-D numerical models of crustal shortening, based on the Sandbox_shortening_ratio example of Gerya (2019). The initial configuration embeds layers of materials with properties equivalent to the sand and silicone used in the analogue models with A) a seed in the central part of the model and B) no seed. The model is shortened by moving toward the left a “rigid” mobile wall at a constant velocity of 25 mm/h. The right panels show the geometry and strain location after 2 cm of shortening (11.1%) for models with a silicone layer with a density of 965 kg/m^3 or 1400 kg/m^3.

4) *Line 83. Was the cohesion of the sand measured in this study? If not, please provide a reference.*

Reply: We now refer to previous studies by Klinkmuller et al. (2016) and Schreuers et al. (2016) for the cohesion of the sand we used. We also refer to Rudolf et al. (2022) for the measurements of friction coefficients.

5) *Lines 126-130. Please report in Table 2 the Ramberg and Smoluchowsky-like numbers (Rm, Sm) for both model and nature.*

Reply: We now report R_m and R_s numbers in Table 2.

6) *Lines 147-149. Corti et al. (2006, Spec. Paper GSA) performed similar analogue models characterised by coeval shortening and orthogonal extension, which were applied to the Sicily Channel.*

Reply: We now refer to the models by Corti et al. (2006) in the Introduction, when presenting the distribution of deformation around the Sicily Channel. We also discuss how the models by Corti et al. (2006) compare to our models and those by Dhifaoui et al. (2021):

Line 446: “Instead, models by Corti et al. (2006) that used a similar set up than \citet{dhifaoui21} with extension applied on one wall during compression but with a brittle crust, do not show V-shaped conjugate strike-slip faults. Interestingly, in these experiments, the authors used a low V_e/V_s value of 0.2. The absence of strike-slip faults is therefore consistent with our minimum value ($V_e/V_s>0.9$) for effective lateral crustal escape.”

7) *Lines 180-182. The convexity observed in some models could result from a high friction of the side walls. Have you adopted any technical practices to minimize this effect?*

Reply: We did not adopt any particular method to decrease the friction on the side walls. On the other hand, having friction-free side walls would imply that the edges of the models correspond to perfectly lubricated strike-slip faults in nature, which would not necessarily be more suitable. The effect of high friction in the models on fault shape is visible for model BI10 for instance where it is limited to the first 2-3 cm from the W-E edges. Our fault mapping for thrust faults discarded these areas. The convexity we describe is generally in the center of the models where boundary effects should be limited and we therefore consider these features a result of the models and not simply related to edge effects.

8) *Lines 301-305. Have you considered the possibility that these anomalous faults departing from the edges of the graben could represent only boundary effects?*

Reply: We now indeed include boundary effects as a possible explanation for the orientation of these anomalous faults. See our reply to comment #3 by Reviewer 1.

9) *Lines 413-416. It is not clear why shortening-parallel thrusting should develop in this model. Has shortening-parallel thrusting been identified in any model of this series? Pleaseclarify. Spontaneous shortening-parallel thrusting resulted in the above-mentioned model by Sokoutis et al (2000), which - differently from this experimental series - were isostatically compensated. This may represent a key difference with respect to the current series of models.*

Reply: We did not mean that shortening-parallel thrusting should necessarily develop in this model, but rather that model BI01 should be the most favorable model given that it shares common boundary conditions (i.e. a first phase of stretching and a second phase of orthogonal shortening) with previous models that showed basin inversion, or formation of thrust faults with a low angle with respect to the shortening direction. We modified this paragraph to clarify. We also now refer to the study by Sokoutis et al. (2000) to indicate that lateral variations in crustal thickness/strength and isostatic compensation could be important parameters in promoting basin inversion under these boundary conditions.

10) *Lines 422-423. Please give more details about the characteristics of fault reactivation. From the tectonic setting (basin-parallel shortening) I would expect some component of strike-slip movement. What is the dominant kinematics of reactivated normal faults?*

Reply: The reviewer is right. We now indicate that preexisting faults are reactivated in transtension.

11) *Figure 1. Please indicate the 'seeds' in the model setup.*

Reply: We now indicate in Fig. 1 the position of the seeds in the entire model and not only on the lateral visible section.

12) *Figure 12. What does the yellow layer at the base of the model configuration in the left panel represent? This model is referred to as purely brittle, but this yellow area is equivalent to the basal ductile silicone of the models shown in the middle and right panels. Please clarify this.*

Reply: The yellow layer was representing the ductile (silicone) layer. In the bottom figure of the left panel, it was representing the seed located in the center of the model. To make it less confusing we now indicate "seed" on the figure and we changed the colors to make them consistent with those of Fig. 1 (set-up). We also now indicate the meaning of the colors in the caption.

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