**Review of "Role of inheritance during tectonic inversion of a rift system in a thick- to thin-skin transition: Analogue modelling and application to the Pyrenean – Biscay System".**

Authors: Jordi Miró, Oriol Ferrer, Josep Anton Muñoz and Gianreto Manastchal

I enjoyed reading this manuscript - it presents well designed and executed sandbox modelling experiment, and analysis of obtained results with clear and convincing comparison to the real-world case study in the Pyrenees. I spotted only bunch of technical corrections that might be considered by the authors, see attached annotated pdf file, other than that this ms is basically good to go.

I'm looking forward reading the final version.

Piotr Krzywiec

> ***Author's reply:*** *We thank Piotr Krzywiec for his review and the positive assessment of our manuscript. We have processed the minor comments in the revised manuscript. All of them can be checked in the tracked change version of the manuscript.*

**Review of "Role of inheritance during tectonic inversion of a rift system in a thick- to thin-skin transition: Analogue modelling and application to the Pyrenean – Biscay System".**

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*In this study the authors use a set of newly designed analogue experiments to examine the role played by 1) the geometry of the basement inherited from the Mesozoic rifting and 2) the presence and absence of weak evaporites layer on the structural style during tectonic inversion. Although the title suggests that such modelling applies to the situation of the plate boundary between Iberia and Europe, along the Pyrenean-Biscay System, it in fact focuses on the case of the inversion of the Basque-Cantabrian basin. This basin is characterised by Triassic salt tectonics during rifting and tectonic inversion stage. It is in lateral continuity with the Asturian Massif where the Triassic evaporites are lacking and the deformation is dominated by basement imbrication.*

*The paper covers an interesting topic but as indicated below I think there are some errors in the reasoning, not in the models themselves, but on the application of these models to explain the structure of the region.*

***Author's reply:*** *We have responded these concerns in the annotated pdf manuscript*

*My main concern is whether the model design is suitable to address the question of thin vs thick-skinned deformation of the region. As far as I understand the model set-up it is made to reproduce lateral changes in the mechanical resistance (in addition to lateral change in the geometry of the basement boundaries) to sliding along a single basal decoupling surface. Because the main regional decoupling is located at the same depth, the lateral variations in structural style are due to the frictional/viscous properties at the base of the tectonic wedge and not to the depth of decoupling (at 2.9 cm in both domains). As a result, the authors are modelling the transition from high friction to weakly viscous wedges, which is interesting, but not thick vs thin-skinned tectonics. I would expect two basal decoupling levels at different depths. Alternatively, it is designed to reproduce two thin-skinned wedges one with basement-involved deformation and the second is a salt-based wedge. This difference between the natural observations and the model design should be clearly stated in the text and the indication of thick vs thinskinned model throughout the text removed. The definition of a decoupled versus coupled domain does not apply here as the basement-involved domain is also decoupled (highfriction but decoupled). Can be replaced by strongly coupled vs weakly coupled.*

***Author's reply:*** *We have already answered these concerns in the annotated pdf file. Enclosed here is a summary of them*

*Any model experiment, either numeric or analogic introduces a simplification of the nature, and consequently you must consider these simplifications when interpreting the modelling results. In our case the rigid basal plate of the model is planar and would represent the sole detachment of the entire thrust wedge. However, it is aimed to represent in nature a detachment horizon climbing up section eastwards from the basement (rigid blocks) into the salt layer (silicon), that is, from a thick-skinned domain to a thin-skinned one. So, in nature this detachment should show a staircase geometry from the thick-skinned domain where the basement is involved (rigid blocks in the experiments) to the eastern thin-skinned domain where structures decoupled from the basement along the weak salt layer. In the transition from the Asturian massif to the Basque-Cantabrian Basin we may consider that this detachment dips initially westward and gradually climbs up section eastward and thus would not be merely dipping northwards as we have reproduced during the experiments. Adding and additional tilting component would be not possible because the silicon layer would flow in a non-desirable way. In any case, such tilting would have not introduced significant changes in our model results as we focus on the structures that form in the transition between domains as different structures link during the structural evolution.*

*It has been interpreted that the basal detachment during the Pyrenean contractional deformation in the Asturian massif is located at upper crustal levels, at about 15km, and reactivate former Variscan structures (Pulgar et al., 1999; Gallastegui et al., 2016). This basal detachment deepens and wedges northwards* *rooting into deeper crustal levels. So, it is part of a thick-skinned system. In the modelled area we only deal with the shallower part of this thick-skinned system, and it would be represented by the detachment above the rigid basal plate and underneath the wooden blocks in the western part of the model.*

*The eastern adjacent domain is characterized by a thick silicon layer (salt) that produces the decoupling of the upper sand package with respect the basal rigid plate, which in that case would represent the top of the basement. In our experiments detachment above such plate would represent an intra-basement detachment below the basement blocks in the western domain and a detachment above the basement in the eastern domain.*

*We have clearly stated all these concepts and explain what we understand by differences in the coupling between the basement and the cover in our experiments (see detailed answers in the annotated pdf file and the new sentences added in the original manuscript).*

*The introduction should be reorganised and better introduce the study (what is really new) perhaps by introducing the number of previous models that have been done on a similar topic. The role of the basement depth (in addition to the role of the weak decoupling) to explain the topography of in the Basque-Cantabrian basin is key but not exploited here. The crust in the Basque-Cantabrian is originally thinner so for the same shortening in the Asturian Massif, a lower topography is expected. The role of the strength of the décollement may be second order here.*

> ***Author's reply:*** We have reorganized the introduction following reviewer's suggestions

My comments are listed below and also indicated in the pdf attached.
Introduction

L#31: 1st sentence. You need to be more specific otherwise this statement does not need citations. It is so well established that you can go back to the birth of mechanical models of accretionary prisms (eg. Chapple, 1978) or to the first description of nappes (Argand, 1916). All these models require rheological layering with a mechanically weak basal layer, which is inherited, indeed. Why not starting with the second and third sentences which I think are more specific.

> ***Author's reply:*** We have removed the references. Indeed, it is a very general statement, but yet we think it can be valid to introduce the following sentences.

L#33: why it is crucial ?

> ***Author's reply:*** We have rephrased the sentence.

L#35: It seems you oppose inheritance (rift architecture) and the pre-existing weak horizons. This separation needs to be better argued. To me pre-tectonic lithologies and structures are all part of what is generally called "inheritance".

> ***Author's reply:*** No, we are not opposing inheritance to the existence of pre-existing weak layers, and we agree with your view. We pretended to specify that apart from the pre-existing structural grain (meaning structural configuration), the distribution of weak horizons is also part of the inheritance (this is why be started the sentence with "In addition"). We have modified the sentence hoping to be more clear now.

L#37-41: About references cited. In the Zagros, the along-strike variations in structural styles depend on the thickness of the Hormus salt (including pre- and syn-folding diapirs mainly in the SE), the presence of shallow evaporitic layers (in the NW) and the magnitude of basement-involved deformation. This is investigated in a large number of papers. Please add reference to Sherkati et al., 2006; Mouthereau et al., 2007; Jahani et al., 2009.

> ***Author's reply:*** Thanks for your detailed comments on the Zagros. We know and agree with them, but these details are beyond the general statement of this sentence. We only pretend to give some references to other F&T belts.

For the Andes, Kley et al. 1999 present a review of the structural variations along the strike of the Andes FTBs but don't address the role of the distribution of salt layers. The discussion is more on the variations between thin and thick-skinned deformation. Overall I don't think the Andes can be compared to the other FTBs you are mentioning. Actually the variations observed are mostly related to variations in the rheology at the lithospheric scale (see review in Mouthereau et al., 2013; Sobolev and Babeyko, 2005 and the recent Liu et al., 2022 from Sobolev's group).

> ***Author's reply:*** We have changed the references to the Andes and Apennines for the Sierra Madre Oriental.

Tozer et al. Not adapted. This paper aimed to demonstrate for a specific structural example along the FTB that a thick-skin interpretation is a viable solution compared to the thin-skinned interpretation. I don't see where in this paper there is a discussion of the structural variations in the Apennines and the role of weak layers.

> ***Author's reply:*** Please, see comment above.

Pyrenees. This paper applies to the Asturian Massif and Basque-Cantabrian basin not to the whole of the Pyrenean FTB. Indeed, the role of weak layers (evaporites mainly) in the Pyrenees has been well documented and there is no first-order lateral structural variations (thick-skin in the Axial Zone and thin-skin in the external FTB). Actually variations do exist between the Pyrenees and Basque-Cantabrian basin that are related to the inversion of different rift basins. And their structural inversion results in the different topographic expression (see modelling in Jourdon et al., 2020).

> ***Author's reply:*** We don't pretend to give references to papers dealing with the entire Pyrenees, just some examples. We don't understand your comment. In any case, it is beyond the scope of this sentence. We have added the reference to Jourdon et al., 2020.

L#42-47: What is the point here ? It reads like a repetition of what has been said above. Rephrase and reorganise.

> ***Author's reply:*** We have reorganized and rephrased all the paragraph to be clearer with the main point to be emphasized. We pretend to introduce the differences in the inversion tectonic features depending on the presence of a salt layer. This is a special volume on modeling of inversion tectonics. So, this is a main point to introduce and discuss. We think there is not a repletion with the previous general statements.

L#47-49: Not clear to me what is the relationships with the sentence above.

> ***Author's reply:*** Please, see comment above.

L#51-54: so what ? how this affects the contractional deformation you have introduced above ?

> ***Author's reply:*** Please, see comment L#42-47.

Geological setting.
L#73: The geological setting on Biscay-Pyrenean domain cites a selection of papers from the same group of co-authors. But you are not including the bunch of papers in the Pyrenees that were published by other groups on the same topic. As far as I understand from the title, Miró et al. study is not about the Basque-

Cantabrian basin and Asturias massif, but on the large Pyrenean-Biscay system, otherwise you must change the title. Here I see only reference to Basque-Cantabrian and Asturias massif. Please modify.

***Author's reply:*** We hope the added references will be enough.

L#74: Tavani et al. 2018 is far from being the only paper on this specific topic. It provides an interpretation of the possible segmentation of the Iberia-Eurasia plate boundary east of the Pyrenees but do not specifically focus on the kinematics and the tectonic evolution of the connection between the Alpine Tethys and the Atlantic which is made in other more "quantitative" papers like Angrand et al. (2020); Angrand and Mouthereau (2021), Nirrengarten et al. (2018); Frasca et al. (2021) for some recent papers.

***Author's reply:*** This manuscript was written before the publication of some of the papers you refer. We have included some of the suggested recent references.

L#75: paper by Manatschal et al. 2021 focuses on rift inheritance in general and on the western Pyrenean-Cantabrian segment mainly. If you want to introduce segmentation of the margin along the whole of the plate boundary move Tavani et al. 2018 here and add citation to Chevrot et al. 2018 who actually provided geophysical evidence for the segmentation in the Pyrenees.

***Author's reply:*** Done.

L#79-80: Also here. the references chosen here are ok if you are focusing on the evolution of the rifting of the Basque-Cantabrian margin, or on the kinematic reconstructions of W Iberia and Atlantic using magnetic anomalies but not on the northern Iberia as a whole. This should include studies that focused on reconstruction including data in Pyrenees or the Iberia Range etc...

***Author's reply:*** We are referring to northern Iberia in general, although focusing on the Cantabrian realm. So, these references should be fine. In addition, the paper by Frasca et al., 2021 deals with the entire northern Iberia margin. However, and following the reviewer suggestion we have included the reference to Asti et al., 2022, which also deals with the entire northern Iberian margin.

L#81-Here again same comments. It reads like you refer only to the works being done in the Cantabrian region not in larger scale Biscay-Pyrenean system.

***Author's reply:*** Please, see comment above.

L#92: Repetition. Again your references are not fully adapted to the entire Pyrenean orogenic system.

***Author's reply:*** Please, see comment L#79-80.

L#94: "..addressed the study of the reactivation..". The role of rift evolution with application to the deep structure of the Pyrenees has been investigated using "accordion" numerical modelling e.g. in Jourdon et al. (2019). Why it is not cited here. It focuses on the same question and using numerical modelling.

***Author's reply:*** We have added this reference (Jourdon et al., 2020). However, the scale, approach and methodology completely differ from the modelling work done in this study. We have added a sentence explaining what has been done by previous numerical modelling studies.

L#94-95: Could you be more specific to justify you study. What you don't understand in rift inheritance that could help understand the structural changes ? Also it is not clear what structural changes you are referring to, thick vs thin-skinned ? for instance in case the structural changes are due to salt thickness variations then the structural changes are not 100% related to rifted margin architecture.

***Author's reply:*** We have explained the aim of our work.

L#101: awkward. The tectono-sedimentary evolution refers to the pre-Pyrenean tectonic evolution and the structural style is a present-day feature. Moreover, one is the cause whereas the other is the consequence.

> ***Author's reply:*** Agree. We have rephrased the sentence.

L#102-108: this clarifies a bit what was unclear before. Perhaps should think of moving part of the section upwards.

> ***Author's reply:*** OK. We have moved this section upwards.

L#110 : "rift structures". Be more specific since the role of the rift architecture (hyper thinned crust in distal domain versus thick crust in proximal domain) has already been exposed.

> ***Author's reply:*** We have explained better this statement and emphasized the differences with previous studies. Previous numerical modelling studies are 2D experiments dealing with lithospheric scale features. Our study is a 3D modelling approach dealing with upper crustal levels and focusing with the structural features developed in the transition areas between adjacent realms. So, significantly differ from other papers like the one by Jourdon et al., 2020.

L#112: looks very close to the work of Jourdon et al. 2020.

> ***Author's reply:*** It is not close at all. The scale, the methodology and the problems addressed are completely different from the work done by Jourdon et al., 2020. We have explained better the work done and the differences with the previous studies, including the work by Jourdon et al., 2020.

L#116-117: should come before the sentence above that mentions the thickness of the crust.

> ***Author's reply:*** We don't agree.

L#121-122: the modelling of Jourdon et al., 2020 applied to the Pyrenees and the Basque-Cantabrian basin should be cited here.

> ***Author's reply:*** These references concern studies based on field and subsurface data. We have already cited the numerical modelling work by Jourdon et al., 2020 before. We have also added a sentence explaining the main result of this work. So, this paper is now extensively acknowledged and cited.

L#130: This statement must be clarified. Salt mobility is accounted for in the sections published by Pedrera et al. through diapirism and evacuation process do exist as the Mz deformed with a difference wavelength compared to the basement. But the Triassic evaporitic level is indeed only slightly reactivated during inversion so shortening style is mainly thick-skinned.

> ***Author's reply:*** Pedrera et al. do not infer any decoupling during extension (well just some during late stages in the northern part of the rift system). Of course, formation of salt structures implies the flow of salt from minibasins to diapirs, but this is independent of decoupling. Moreover, we only refer in this sentence to the completely absence of decoupling during shortening following their interpretation, as you also describe. So, we think this statement is clear enough. However, we have slightly modified the sentence following your comment.

L#176-178: The distinction between a decoupled region and a coupled region appears to me weird because your models include both a frictional decollement (white sands) and a viscous decollement (polymer) at the base of the wedge that decoupled the upper orogenic wedge from below. So you should better called these two endmember cases "weakly coupled" and "highly coupled".

***Author's reply:*** For this distinction we refer to coupling/decoupling between the "basement" and the cover succession. In the western domain this is between the wooden blocks and the sand, whereas in the eastern domain this is between the thin wooden plates or the basal plate and the sand pack. Models (Figs. 5, 7 and 9) show that there is not any decoupling in the western domain between the basement (wooden blocks) and the sand, whereas it is in the eastern domain as the silicone polymer rests directly on the basal plate. Of course, we allowed the entire system to detach from the basal plate to reproduce deformation in the entire wedge. So, the distinction is based on such differences. We have better explained this.

L#237-239: In your model both regions include a décollement positioned at the same depth. How to differentiate between a thick-skinned and a thin-skinned wedge. At best there are a high friction wedge versus a weak décollement/salt-based wedge.

***Author's reply:*** Any model experiment, either numeric or analogic introduces a simplification of the nature, and consequently you have to consider these simplifications when interpreting the modelling results. In our case the rigid basal plate of the model is planar. However, it is aimed to represent in nature a detachment horizon climbing up section eastwards from the basement (rigid blocks) to the salt layer (silicone polymer), that is, from a thick-skinned domain to a thin-skinned one. We are not able with the available rigs and the desired set-up involving silicone to reproduce such geometry of the basal detachment between thick- and thin-skinned domains. However, we think that this simplification is still valid for our objective and does not invalidate our modelling results. We investigate the transition of the structures in the western domain where there is not decoupling between the cover (sand) and the basement (wooden blocks) to the eastern areas detached in the salt (silicone). The basement in the thick-skinned domain is involved in the deformation and has detached in nature in the upper crust, more probably reactivating former Variscan faults (see Pulgar et al., 1999 among many other papers). So, this detachment in the experiment corresponds to the rigid basal plate underneath the wooden blocks.
We have added a paragraph at the end of section 3.1.3 (Procedure in the experimental methodology chapter) explaining these arguments and simplifications.

L#290: see my comment above.

***Author's reply:*** Please, see answers to the comment above.

L#368-370: this domain is not thick-skinned because the part of the model with weak basal decollement has the same thickness. But it is equivalent to deformation dominated by basement involvement.

***Author's reply:*** See answer to the comment above. Regardless the limitations of the modelling procedure, we pretend to simulate basement-involved faults ("thick-skinned" deformation). Be aware of these limitations and not directly take the geometry you observe in the sections.
Now we have explained these limitations and the way we have simulated thick-skinned deformation.

L#371: see my comments concerning your definition of coupled/decoupled and thinskinned.

***Author's reply:*** See answers above as well as the new sentence justifying the simulation of thick- versus thin-skinned modes of deformation.

L#396: Section 5.2. Title. you did not model distinct thick-skinned and thin-skinned deformation, neither coupled and decoupled models. At best you design an interesting model with two different frictional/viscous decollement and different initiale/inherited geometry.

***Author's reply:*** See answers to comments above.
This looks like more a terminological question than a conceptual one. Another issue is the limitations of the modelling techniques.

The western part of the modeled area is characterized by rigid basement blocks and no decoupling between these blocks and the cover (sand) above. These blocks and the sand package (above and adjacent to the rigid blocks) have been deformed above a frictional detachment (the basal plate) that would represent the reactivation of basement inherited features in the upper crust, such as previous Hercynian thrusts. The eastern adjacent domain is characterized by a thick silicon layer (salt) that produces the decoupling of the upper sand package with respect the basal rigid rig plate, which in that case would represent the top of the basement. There is a fundamental misunderstanding of the significance of the basal plate during the experimental procedure. In our experiments detachment above such plate would represent intra-crustal detachment below the basement blocks in the western domain and a detachment above the basement in the eastern domain. Be aware of the simplification of the setup. We think that once we have rephrased and added additional comments following reviewer's suggestions these concepts are now clearer.

For us, and regardless the modelling limitations, it would reproduce two distinct deformation domains characterizing thick- and thin-skinned deformation respectively. You may consider that these terms are not appropriate, but for us is quite clear what we refer when describing coupling and decoupling domains (see all the arguments above). It is not fair just to argue that "at best we have designed an interesting model with two different frictional/viscous decollement".

L#418-419: useful?

***Author's reply:*** We have removed this sentence.

L#421-428: Rephrase and shorten.

***Author's reply:*** We have not found a simpler way to explain these features. An option would be to remove all this paragraph, but yet we think is quite significant to explain how the different linkages controlled by the initial position of the structures because the effect of the syntectonic sedimentation results into different geometries in the transitional domain.

L#418-419: useful?

***Author's reply:*** We think so.

L#452: Indeed. higher friction not thick-skinned. It is thick-skinned if thrusts are rooted into the mid-lower crust or even the mantle.

***Author's reply:*** Yes, it is a higher friction detachment but representing the upper parts of a thick-skinned system. We have better explained this statement adding a sentence.

L#454-455: Just remind the reader that such contrast between high and low friction saltbased wedges is very basic and very well documented in analogue modelling for decades at least since Davis and Engleder (1985), Liui Hiuqi et al. (1992), or Costa and Vendeville (2002).

***Author's reply:*** Indeed, this is a well stablished concept, and we refer to it to reinforce the comparison between our modelling results and the structural style and topography observed in the Basque-Cantabrian Pyrenees. We have added these reference and additional ones.

L#456-460: Although I agree that a salt decollement control is an important feature to explain the topography, most importantly this is the fact that you have also a basin (Basque-Cantabrian basin). What is relevant here is the combination of both the weak décollement allowing efficient transfer of deformation outward and the originally lower topography in the Basque-Cantabrian basin compared to the Asturian Massif. This is well reproduced in Jourdon et al.(2020). I agree that the occurrence of a salt layer leads to thin-skinned deformation. But thick-skinned could also be present as deformation progresses towards the basin margins where the salt may be lacking due to salt migration during extension. I suggest to better explain why you totally reject the possibility of a late stage of basement-involved deformation.

***Author's reply:*** We have specified that the thin-skinned style of the Basque-Cantabrian Pyrenees refers to the frontal part, as the sole detachment there is connected with the extensional detachment that produced the exhumation of the mantle in the internal parts of the thrust system. Basement is involved in the hinterland, but it is not in the foreland at the margins of the salt basin, contrary to the interpretation by Pedrera et al. and coauthors (they even argue for a basement-involved thrust to the most frontal structure) and consistent with the modeling results of Jourdon et al., 2020. We have added this reference here.

L#488: This aspect of the modelling is important as syn-convergence piggy-back sedimentation reveals the domain was maintained at a low elevation and this is probably linked to both salt tectonics and the originally subdued topography of the Basque-Cantabrian basin which allows maintaining a central depression (see also Jourdon et al., 2020).

***Author's reply:*** We have added the reference to Jourdon et al., 2020.

F. Mouthereau
Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-1175/egusphere-2022-1175-RC2-supplement.pdf

***Author's reply:*** Additional comments included in the supplement have also been considered in the revised version of the manuscript. You can check them in the tracked changes version of the manuscript.

**Review of “Role of inheritance during tectonic inversion of a rift system in a thick- to thin-skin transition: Analogue modelling and application to the Pyrenean – Biscay System”.**

Authors: Jordi Miró, Oriol Ferrer, Josep Anton Muñoz and Gianreto Manastchal

This is an interesting, well-written, and presented manuscript dealing with a major topic in structural geology and geodynamics: The impact of the geometry and internal structure of rifted continental margins on mountain-building processes and collisional orogen evolutions. This study is also ambitious since the authors propose to reconstruct the early stages of the Pyrenean orogeny using analog modeling, which is certainly a relevant approach but not an easy task. 3D modeling (analog and numerical) is always very challenging in such complex and long evolutionary geological settings. The risk is to fail to correctly reproduce the rheological and kinematical boundary conditions, the geological processes, and couplings controlling the crustal deformation and associated sub-surface structures. Fortunately, the authors decided to address a focused topic: how passive margin inversion is accommodated in the transitional domain between basement controlled and salt-decoupled domains. The case study region corresponds to the transition between the Asturian Massif, and the Basque-Cantabrian Pyrenees where, as evoked by the authors (L152), surface and sub-surface geological observations are difficult to perform. This study can be, then, considered a valuable and very interesting attempt to better interpret the structure of this debate portion of the Pyrenean orogen.

Generally speaking, the science sounds good and the authors' interpretations are quite well supported by the analysis of the analog models they performed. Extrapolation of these experimental results to Nature through the comparison of available geological cross-sections and analog model final stages brings interesting insights but some points should be strengthened.

> ***Author's reply:*** *We thank Stephane Dominguez for taking the time to go trough out work and for the constructive feedback to our manuscript. We have addressed and processed most of his comments and suggestions in our revised manuscript.*

**GENERAL COMMENTS:**

Here are my main comments and questions to be considered by the authors:

1- Despite what is mentioned in the manuscript title, I'm not sure the authors' experimental setup allows them to investigate stricto sensu the transition between thick- vs thin-skinned crustal deformation. As shown in figure 3 and, taking into account the estimated spatial scaling (Table 1: 1 cm = 1 km), the initial model thickness does not exceed 3-4 km. Even at the very end of the experiments, model thicknesses remain in the order of 10 km, including positive topography. There is only one main décollement, situated at a shallow depth of a few km, and the deformation is forced to locate above it, involving essentially the Mesozoic and Cenozoic sedimentary cover. To model the thick-skinned crustal deformation shown in the geological cross-section n°1 in figure 2, I would have expected the simulation of a second, deeper, décollement level allowing for the whole upper crust (at least) to be involved in the tectonic inversion of the margin. Consequently, I suggest modifying the manuscript title to less emphasize this specific point.

> ***Author's reply:*** *Any analogical model introduces a simplification of the nature when trying to reproduce a natural case study and the processes that occurred in the analyzed area. The western part of the modelled area corresponds to the eastern termination of the Asturian Massif where thrusts climb up section from the Hercynian basement to the Triassic evaporites of the Basque-Cantabrian Basin eastwards. It is has been interpreted that the basal detachment during the Pyrenean contractional deformation in the Asturian Massif is located at upper crustal levels, at about 15km, and reactivate former Variscan structures (Pulgar et al., 1999). This basal detachment deepens and wedges northwards rooting into deeper crustal levels (Gallastegui et al., 2016). So, it is part of a thick-skinned system. In the modelled area we only deal with the shallower part of this thick-skinned system, and it would be represented by the detachment above the rigid basal plate and underneath the wooden blocks in the*

*western part of the model. Thus, we focus on the easternmost part of this thick-skinned system as our aim is to analyze the transition area to the thin-skinned domain located further east. The basal thrust represented in the cross-section 1 of figure 2 you refer shallows eastwards in the modelled area as imaged by the available seismic lines (Carola et al., 2015), consistent with the thickness and geometry of the wooden blocks of the model setup. In the setup of our models the rigid basal plate is planar, but in nature it would represent a detachment horizon climbing up section eastwards from the basement (rigid blocks) into the salt layer (silicone), that is, from the eastern edge of a thick-skinned domain to a thin-skinned one.*
*We have explained these ideas in the manuscript by introducing a new paragraph at the end of the model set-up section (lines 371 to 383 on the tracked changes manuscript). We have not been able to find a better title to explain these features. So, we propose to leave the title as it is.*

2- L157- 3.1.1 Model setup: Here, the authors should present, in map view and in cross-section, the initial geological stage inferred from paleogeographical and geological data (before the initiation of crustal shortening and tectonic inversion of the margins). This will help readers fully understand the specific initial geometry and structure of the analog model. Without this crucial information, it is sometimes difficult to understand and follow the description of the complex 3D model characteristics.

***Author's reply:*** *The model set-up is based in the geological map (Fig. 2) and the paleogeographic reconstructions done by López-Gómez et al., 2019. This work is referenced during the description of the set-up.*

3- Comparison Model vs Nature: This part of the manuscript should be improved to better highlight the contributions of the present study. On one hand, some model results look quite different compared to the presented geological cross-sections and, on the other hand, additional comparisons between the models and Nature should/could be done (see more detailed comments hereafter).

***Author's reply:*** *We have added a new cross-section across the eastern part of the Asturian Massif to facilitate the comparison between our experimental results and Nature. We have also done the suggested modifications, such as changing the orientation of the top views of models to facilitate comparison with the geological maps.*

**SPECIFIC COMMENTS:**

Here are some additional comments concerning unclear portions of the main text and suggestions to improve the quality and efficiency of the figures and associated captions:

The Abstract sounds good and I just suggest clarifying a little bit more the following sentence (L19): " The experimental results show …. thin-skinned domains". Explain what are the evoked "oblique structures" and "active structures"?

***Author's reply:*** *We have rephrased this sentence.*

L48- Which type of differences?

***Author's reply:*** *We have clarified the sentence in the new paragraph introduced at the Introduction (lines 57 to 72 on the tracked changes manuscript).*

1-Introduction: L63- I would use "experimental protocol" (or experimental set-up or experimental results when needed) rather than "experimental program".

***Author's reply*:** *Done.*

2- Geological setting: It is well summarized including the open questions which are still debated.

Figure 1: Add the location of the map presented in figure 2.

***Author's reply:*** *Done.*

Caption: Indicate the reference/source of the geological cross-section n°2.

***Author's reply*****:** *Done.*

L117: Detachment inversion -> Could the authors describe or refer to a natural case example?

***Author's reply:*** *We don't understand this comment as we are explaining in this paragraph the Basque-Cantabrian Pyrenees. We don't consider pertinent here to refer to other natural examples. In any case we have slightly modified this sentence and change the term inversion by reactivation.*

Figure 2: The authors used geological cross-sections from Alonso et al., 1996 to present the crustal structure of the Pyrenean range across the Asturian Massif. The single and main thrust fault plane dipping North from the surface up to more than 20 km depth is highly dubious … Especially taking into account the geometry and wavelength of the tectonic structures in the upper 5 km of the cross-section. It is also not really compatible with the geological cross-section in figure 1 (there is a mid-crustal flat décollement level at around 15 km depth). This should be corrected/modified.

***Author's reply:*** *We have added a sentence explaining this (see answer to the first general comment). We have also modified the figure, as there was a drafting error with respect the original cross-section by Alonso et al., 1996. We agree with your comment about the depth of the detachment and the wavelength of the structures at surface, but this is not our section. These authors reconstructed the geometry of the sole detachment by applying a simple fault-bend fold model taking as a reference the geometry of the unconformity at the bottom of the Mesozoic succession. They did 2 sections, one with a proposed detachment at 15km depth, and another with a slightly deeper detachment (the one reproduced in Fig. 2). In any case, both sections are compatible with the regional cross-section of Fig. 1.*

L130: Any comment on this proposed interpretation? Is it fully in accord with available geological observations or there are some discrepancies?

***Author's reply:*** *We prefer no to add any comments on this interpretation as that would require a description of the geological and geophysical data that is not in agreement with this interpretation. This would require a long discussion and hopefully it will be a future article. This is beyond the objectives of this study.*

3- Analogue modeling: L158-L161: I would move this paragraph just before 3.1 Experimental methodology, as a short introduction to the main chapter (3- Analogue modeling).

***Author's reply:*** *Done.*

L186-L192: Please, add the uncertainties on the mechanical parameters (example: 34.6° +/-3° of internal friction).

***Author's reply:*** *Done.*

Table 1: The mentioned viscosity of the natural evaporite ($10^{18}$-$10^{19}$) seems very high to me. Correct if necessary.

***Author's reply:*** *While the viscosity of salt rock in a diapir ranges from $10^{17}$ to $10^{20}$ Pa.s, it can be as low as $10^{15}$ Pa.s in fine-grained extrusive salt. As grain size doubles, the viscosity of salt increases tenfold (Jackson and Hudec, 2017). We have modified the values from salt viscosity to $10^{15}$-$10^{19}$ Pa.s.*

Figure 3: Nice figure. I think adding the names of the main simulated geologic domains would help readers better understand what are the geometric analogies/boundary conditions between the model and Nature. Also, orient all the map views of the model with the North pointing toward the top of the figure/page.

> ***Author's reply:*** *Done.*

L207- Can you explain why it was necessary to wait for 5 hours to "favor the early development of salt structures »?

> ***Author's reply:*** *Done. We have explained that waiting for 5 hours sedimentary loading promotes salt evacuation and consequently the development of salt structures. The final proposed sentence is: "To favour the early development of salt structures by sedimentary loading and salt evacuation, the first syn-extensional sand layer was poured after 5 hours of extension".*

4- Experimental results: The introduction of this chapter is clear. I recommend orienting all the map views with the North pointing upward/top of the page to favor the comparison between the models and Nature. I know that this will require some work but I consider that it's an important point.

> ***Author's reply:*** *Done.*

Figure 4: I don't understand the relations between the arrows showing the direction and amplitude of extension and the indicated cumulated amount of extension. Caption: Indicate that syn-tectonic sedimentation is performed during all the initial extensional phases of the experiments.

> ***Author's reply:*** *We have explained the significance of the arrows and the dashed rectangle of reference in the figure caption. We have also added the explanation concerning the syn-tectonic sedimentation following your suggestion*

Figure 5: Caption: Add a sentence to explain why the whole model is tilted northward by 2.8° during the extensional deformation phase.

> ***Author's reply:*** *Done.*

Figure 6: Ok, here, the evolution of arrow lengths seems more consistent with the indicated cumulated amplitude of shortening. However, I think an arrow is missing for stage a) (21 mm of cumulated shortening).

> ***Author's reply:*** *Done.*

The four pictures show a relatively complex tectonic evolution and I recommend adding/showing the corresponding interpreted structural maps (fault symbols + main tectonic structure names). Caption: Indicate that no syn-tectonic sedimentation is performed during the compressional phase of this experiment.

> ***Author's reply:*** *We already did an annotated version of this figure, but we decide to leave as it is as this version shows better the experiment results without the interpretation. We have added a sentence with the information you refer.*

Figure 7: Caption: Add a sentence to explain why the whole model is tilted northward by +2.1° during the compressional deformation phase.

> ***Author's reply:*** *Done.*

Figure 8: Arrows look good this time. Caption: Indicate that syn-tectonic sedimentation is performed during the compressional phase of this experiment. Same suggestion concerning adding interpreted fault maps of the four presented evolutionary stages.

***Author's reply:*** *Done. See answer above concerning Fig. 6.*

Figure 9: Same suggestion concerning adding a few words to explain the imposed additional northward tilting of the model.

***Author's reply:*** *Done.*

5- Discussion:
I know that this is beyond the scope of this study (and probably hardly achievable through an analog modeling approach alone) but I'd like the authors to comment on the potential role of surface erosion (which is not simulated in authors' analog experiments) on deformation mechanics and kinematics.

***Author's reply:*** *This is an interesting parameter that has a critical impact on the deformation mechanics and kinematics of fold-and-thrust belt. Specific studies concerning this topic by analogue modeling can be found in the literature illustrating how erosion impacts the sequence of thrusts emplacement as well as the height and width of the resulting fold-and-thrust belt (Malavieille, 2010; Konstantinovskaia and Malavieille, 2011; Perrin et al., 2013; Malavieille et al., 2019 to highlight some of them). We consider that, as the reviewer indicate, this is beyond the scope of our study, and that develop this point would make the manuscript longer.*

Figure 10: Interesting figure (and correctly N-S oriented this time). Caption: remove « approximate »

***Author's reply:*** *Done.*

Figure 11: Orient the map view with the North pointing upward as for figure 10. Add fault traces and symbols on the map view.

***Author's reply:*** *Done. We have added the main fault traces.*

Figure 12: This is the last and concluding figure of the manuscript. It is, then, of significant importance. However, if I can see a good analogy between model cross-section e) and its geological counterpart f), I'm less convinced by the comparison between model cross-section c) and its geological counterpart d). Also, I think the comparison between an E-W model cross-section in the transitional domain and the equivalent geological cross-section is missing.

***Author's reply:*** *We have enclosed a new section closer to the modelled area across the eastern termination of the Asturian massif to show the comparison with the model cross-section.*

References are abundant and, as far as I can judge, relevant. However, some recent contributions of potential interest for the present study are not cited (e.g. Labaume et al., 2020; Jourdon et al., 2019, 2020; Angrand et al., 2021; Ford et al., 2022). I think the authors should consider including and commenting on at least some of them.

***Author's reply:*** *We have added references to the papers by Angrand et al., 2021, Jourdon et al., 2020 among other following also the suggestions by other referee.*

Based on all these considerations, my overall evaluation of the manuscript is positive. The authors have done a huge amount of work. However, I think this manuscript needs some corrections on the science and on the form to be fully acceptable for publication.

***Author's reply:*** *Many thanks for the reviews and comments that helped improve the manuscript. In addition to the previous general comments, we have implemented all the* *comments and suggestions that the reviewer has made in the annotated manuscript. All changes can be checked in the manuscript with tracked changes.*

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