Reply to reviewer 2

Text for submission textbox:
We would like to thank reviewer 2 for the helpful comments and suggestions, and especially for stepping in after the original second reviewer could not submit a review.

In order to provide an orderly reply, we have uploaded a PDF with an overview of all comments and our replies to these (see below).

2. Reply to reviewer 2 - anonymous

Reviewer comments:

In this paper the Authors present a set of analogue models designed to compare different set-ups adopted in analogue modeling of extensional tectonics and to discuss the differences/similarities among them. The different experimental series consider four basic different set-ups, which can be grouped in two different approaches: distributed extension (foam, rubber sheet experiments) and localized extension (basal plates, conveyor belt experiments). The Authors additionally analyze parameters such as the presence of a weak seed to localize deformation, variations in rheology (e.g., thickness of the brittle/ductile layers composing the models), velocity of deformation, etc.

Although the paper is potentially interesting I found it is affected by many important problems, which are summarized in the following

In many parts of the paper the Authors are somehow confusing the description of the experimental set-ups (foam, rubber sheet, basal plates, conveyor belt), with boundary conditions of deformation (e.g., vertical rheological layering, velocity of extension) and technical expedients to improve experiments (e.g., lateral confinement of the models). This is for instance the case of the introduction section, where the Authors mix many different things (including the analysis of 4-layer models, which do not seem important for this paper – see below). In the experiments, this makes it is difficult (at least in many places) to isolate the effect of the different set-ups alone. In this respect, it is not very clear why the Authors try for instance experiments with variable velocities, which are to me only complicating the interpretation of the influence of set-ups on the experimental results. In summary, to facilitate the reader, I would simply present the results of the 8 reference set-ups.

- Each experiment is described by experimental set-up (foam base, rubber sheet base, conveyor sheets, basal plates, lateral confinement), initial conditions (rheology of the material and their layering), and boundary conditions (velocity, basal and sidewall friction). We agree that these are many factors. However, the issue to consider is their inter-relationship. Extension velocity impacts viscous strength directly and therefore we need to consider extension velocity and rheology together when assessing the influence of brittle-ductile coupling on extension style. The impact of experimental set-up is directly modified by the rheology of the overlying materials (which in turn is affected by the velocity that the basal conditions apply). This is why we need to report set-up, boundary conditions and initial conditions together.

- In order to avoid confusion between setup and initial conditions, we have rephrased the occurrences of the term “set-up” where set-up incl. initial conditions were meant.

- Beyond comparing model set-ups and their experimental results we hope to create a baseline for future work. We hope to provide insights into the problems and challenges that one may encounter when modelling extension. Failed models are often not published, so that the same mistakes can be made again. This paper may help to avoid such problems and we have added additional clarification in the introduction.

- Four-layer models are relevant as background for our crustal-scale models. We model the mantle through the experimental set-up (Fig. 3). Another approach is to include part of the mantle through appropriate materials and place the bottom boundary further down in low-viscosity regions.

- We provide a summary overview in Fig 10 to delineate the effects of the different set-ups and rheology (brittle-only versus brittle-viscous), including variations in extension rate and layer thickness. This figure is used throughout the discussion and we find that especially the variations in extension rate and layer thickness provide highly relevant insights.

Some of the models are to me quite strange, or –at least- should deserve a more detailed discussion and comparison with previous experimental works. I refer, for instance, to the foam or rubber sheet experiments with no seed, which -in both cases or purely brittle or brittle/ductile systems- are unable to produce significant deformation. This latter seems to be in these cases mostly accommodated at the model boundaries –i.e. by undesired boundary effects- which makes the experiments a sort of failed models. In many other cases, see Ken McClay’s works as examples, the rubber sheet has always produced significant faulting. The difference may be due a slightly different application of the rubber sheet (see lines 236-238, although there is no explanation for this difference) or different thickness of the brittle layer (larger in McClay’s models), but a comparison/discussion of this seems to be lacking. Anyway, as said, the models look like failed models, and this has to be explained/addressed by the Authors; their meaning is really not clear to me.

- The pure brittle foam/rubber base models, as well as the pure brittle-viscous reference models do not “properly” localize deformation. These may in a sense be “failed models”, but show nicely how the set-up affects the experimental results in these cases, showing what can go “wrong” and why one might consider using another method. We must stress here, that the aim of this paper is not to present “good” models. We ran various models with various set-ups to compare and simply show what the results are, including such boundary effects. We consider this information highly valuable for analogue modelers, which is a good reason to add and discuss them in the text.

- Concerning the width of the rubber sheet, the reason why the work by Ken McClay does create nice structures is because the rubber is spanned between two sidewalls and has only a limited width. In a very real sense it is a plate base model that localizes all deformation in a somewhat wide velocity discontinuity with local distributed extension. It is not surprising that such a set-up produces well-developed structures, but we may wonder if this does represent natural conditions. We have added a short discussion to the revised manuscript in Discussion section 4.2 and 4.6.

The Authors show and discuss in many places rheological profiles of a 4-layer lithosphere, or show models reproducing a complete lithosphere/asthenosphere system (e.g., Fig. 1). However, since their models are limited to the crust (or upper crust) this may be –to me- misleading. The lithosphere-scale models are normally very different from 2- or 1-layer models, in terms of both architectures of deformation and evolution (see for instance Brun and Beslier, 1996). Therefore, I suggest the Authors not to go in so much detail in the discussion of rheology at the scale of the whole lithosphere.

- Our model set-ups are indeed aimed at the crustal-scale and include 1 or 2 layers. Even so, each set-up implies assumptions about the sub-crustal mantle. A plate base for example, may infer a brittle upper mantle, whereas a foam base may represent a weak, ductile upper mantle. This is why the sub-crustal mantle is discussed and shown in Fig. 3. We acknowledge of course that multi-layer models may yield different extension styles from simpler one- or two-layer models. As background, and because our set-ups assume a sub-crustal mantle, we discuss lithospheric-scale models in our introduction.

I think there is a problem with the scaling of the experiments. The Authors indicate that for a viscosity of 10(21) Pa s, the system scales down to a velocity of 0,5 mm/y. However, calculations taking values from Table 3 (and the velocities reported in the text) seems to result in a velocity of 5 mm/y for the same viscosity (which is velocity of extension velocity closer to natural ones). I also checked by computing the Ramberg number, which is similar to nature only for velocities of 5 mm/y (assuming a viscosity of 10(21)). A velocity of 0,5 mm/y seems to result from a viscosity of 10(22) Pa s. This has to be carefully checked. Anyway, velocities of ca. 500 mm/h seem to be very high, and more difficult to scale to natural conditions.

- There was indeed a typo in the original text, it should be 5 mm/y for a viscosity of 10(21), which is close to the values for the East African Rift as reported by e.g. Saria et al. (2014) later in the same sentence. We have now listed all scaled velocities in Table 2. Indeed, some velocities are quite high, this is now also mentioned in the scaling section. But here a modeler may have some freedom, since e.g. lower crustal viscosities may vary substantially. When one assumes a lower crustal viscosity of 10^23 Pa*s, even our highest velocities become reasonable. Proper scaling is always a challenge and we can question whether scaling can even be truly correct with the data we have available at the moment. This is another reason to vary, rather than pinpoint, parameters such as velocity so that we understand what their influence may be on the experimental results.

The Authors should try to improve the discussion of the applicability of model results to specific natural settings: an example of such a detailed discussion is illustrated in Morley (1999 JGS), where the Author discusses the relevance of VD, crustal-scale models for the analysis of pre-existing fabrics in the crust. The Authors should at least refer to this relevant paper. Also, aren't some of the limitations of model set-ups already analyzed in Schreurs et al (2006)? This has to be better clarified

- We would like to point out that our paper aims to compare model methods and results and provide an overview for which tectonic settings these may be appropriate, rather than closely reproduce natural examples. We do agree that in the end, models must serve to better understand nature, but a case study for every single set-up cannot be a part of this paper. We provide a list of natural settings (that is lithospheric profiles) that could be represented by our experiments. We however urge the reader to not simply copy our schemes, but to use them as inspiration and make sure that a certain set-up is appropriate for their natural example.

- We include insights from the very interesting paper by Morley (1999) in the methods and discussion and are grateful for bringing it to our attention.

- Indeed, some limitations are already mentioned by Schreurs et al. 2006, in which the authors have run a (in principle) same model in various labs around the world. Their main findings were already mentioned in the introduction and referred to in several places in the text. Only one extension experiment was run in this benchmark study, for which no variations in set-up, rheology or boundary conditions were investigated.

Many of the descriptions of the internal deformation (and evolution) of models with a seed (analysis made with the CT scan) are not very useful to delineate differences among the different setups and may be significantly shortened or (at least in some cases) removed.

- We think that the application of CT images strongly improves the quality of the results, showing certain features with much more clarity (e.g. the conjugate faults in the rubber base models), whereas others might have remained unnoticed or unknown without the CT-scans (the conjugate fault dips of ca. 90˚). We must also stress that the application of CT scans to show internal evolution is unique and a great help for model interpretation. The figures thus show the possibilities and advantages of CT-scanning models and we consider that they should be included here.

Throughout the paper the Authors use the terms 'rift' and 'graben' as synonymous, as it is sometimes done in the literature. However, rifts are normally larger than grabens, and involve deformation of the whole lithosphere (e.g., Sengor and Natalin 2001 GSA Special Paper 352). The Authors should try to highlight this difference and indicate individual 'tectonic troughs' as grabens (as done in the figures), and when they form in a series giving rise to a wider, more complex deformation zone they could be labelled as a rift system or similar.

- We do agree that there is a difference between the terms "rift" and "graben" and that the latter are generally considered to be less significant structures. Since our models do imply significant deformation within the whole lithosphere, we have decided to rephrase the occurrences of the term "graben" with "rift"/"rift basin"/"rift structure". The term "graben" is now only used for minor fault-bounded depressions (e.g. "marginal grabens")

Other comments (numbers refer to lines)

21. 'the' instead of 'our'

- We prefer to change to "their".

29-30. "Brittle-viscous plate base and conveyor base experiments only localize deformation with high brittle-to-viscous thickness ratios that increases brittle-viscous coupling. This effect is further enhanced by higher strain rates."
The effect (localization) or (as I guess) the coupling is enhanced by high strain rates?

- Indeed, we have rephrased to 'Brittle-viscous plate base and conveyor base experiments only localize deformation with high brittle-to-viscous thickness ratios that increases brittle-viscous coupling. Such coupling is further enhanced by higher strain rates.'

32-37. See main comments.

- See reply to main comment on model usefulness.

46. 'The' instead of 'These'

- We think "these" is better here, building a link with the previous phrase.

82 and other parts in this section (but also in the resto of the paper). The Authors discuss the effect of boundary conditions (e.g., velocity) in the style of deformation together with the effect of set-ups etc. This is, as said, confusing and the discussion of the effect of different boundary conditions (velocity, rheology) does not seem to be pertinent to this work. Also, analysis of parameters such as velocity or rheology should require a more detailed review of the numerous previous works which have investigated these processes.

- The issue is that velocity (a boundary condition) and rheology (an initial condition that evolves) are closely related. Variations in velocity impact viscous strength and thus brittle-viscous coupling. This is why we discuss the effects of velocity together with rheological set-up. This is indeed based on many previous works that have pointed to the close relationships between velocity and rheology and their effects on extension style. We use the same materials throughout, only changing their thickness ratios in selected experiments. Brittle-viscous coupling is of course affected by other factors

Section 1.2. Again, is this summary of experimental materials necessary for the aims of this work? Also, the Authors review materials used to reproduce the asthenosphere but this is really not pertinent here.

- We described standard methods and materials in order to provide an overview of the possibilities for modelling of extensional tectonics. This not only sets the background to our modelling experiments, but we also hope that the overview in combination with our experimental results will form an inspiration for future work. Therefore, we consider the description necessary.

156. Suggest to change to 'Aims of this study' 158-162. I suggest to move the first sentence to after 'numerical means'

- Thank you for the suggestion, we have modified the subtitle.

173. Is Dooley and Schreurs pertinent here? Maybe better to refer to some works analyzing crustal rheology?

- Dooley and Schreurs 2012 is a review paper that partially discusses these issues and contains numerous references. We have slightly modified the text.

181-185. See main comments. These are experimental boundary conditions. C4

- See answer to main comment about strict classification of "set-up" and "boundary conditions"

223. I would change to 'Distributed extension set-ups'

- Thank you for the suggestion, we have modified the subtitle accordingly.

225. 'extension' instead of 'deformation'

- It is modified, thank you for the suggestion.

239. These differences should be discussed in much more detail. See above.

- We have added a clarification to the text, but prefer to discuss this in more detail in the Discussion (section 4.2 and 4.6). Please also see our reply regarding the models by McClay above.

249 and following. See main comment above. For instance, a hot lithosphere following thickening is expected to be characterized by a very ductile crust, leading to very different results.

- Our approach was not to directly reproduce nature, but rather to start from often-used model set-ups and link those to what they may represent in nature. We stress that modellers should check the rheological profile of the natural case they would like to reproduce. Our Figure 3 only serves as an indication.

- Concerning the properties of a post-orogenic crust, we agree that it may not be the most elegant example to link with our experimental conditions (set-up and layering). We have replaced it with the example of increased radiogenic heating, which can occur in a normal crust due to anomalous concentrations of radioactive elements, significantly affecting crustal strength (see Mareschal & Jaupart 2013, and our Fig. 12f)

261. 'extension' instead of 'deformation'

- Thank you for the suggestion, we modified the subtitle accordingly.

Section 2.2.2 (294 and following). For the discussion of these experiments I would consider the paper by Morley (1999), as explained above. See also other main comments.

- We have added additional information on the problems of inherited structures raised by Morley (1999) to this section. However, we should keep in mind that these are not necessarily that important for our set-ups since we are 1) not looking at oblique extension and 2) not concerned with pervasive inherited structures. The VDs can simply represent a new fault at the base of an isotropic (upper) crust.

Section 2.3. See discussion about the relevance of the varying boundary conditions (e.g., velocity of deformation). I would remove the experiments, at least those with varying velocity.

- Please see our earlier answer above.

Sections 3.1-3.2. The experiments with no seed are –as explained above- strange and to me they should be considered failed models. The Authors could think about considering the seed experiments as a different set-up, since they use it to localize deformation in the models

- The models without seed could indeed be considered "failed" models, but as they provide useful insights in modelling processes and boundary effects, we would like to share those. These experiments also allow evaluating the impact of distributed extension alone or distributed extension with a seed.

388-393 (but also 408-414, 432-439, etc.). See above comment on details of the CT scan.

- See our earlier reply to the usefulness of the CT data.

423-424. The focusing of deformation at the sidewall is to me an anomalous, undesired boundary effect which makes the model a failed model.

- These are indeed boundary effects, but these cannot be avoided (see above and discussion later in paper in Discussion Section 4.6)

426. What does 'poor lightning conditions' mean?

- It means that the illumination we applied by lamps was not perfectly oriented to visualize these low-offset structures. The text is modified to clarify.

471 and following (and similar effects for the conveyor belt experiments). Again, the non connection of the grabens in the central portion of the model, where deformation is taken up at the boundaries, make to me the model a strange (failed?) model. It is also strange that a reduction of the thickness of the ductile layer did not help to reduce the effect.

- This is actually a very common effect in (our) models (see also Zwaan et al. 2016, 2018 and Zwaan & Schreurs 2017) and seems to be an inherent feature of models with continuous ductile layers. The fact is that in such models, we do not pull the sand, but the silicone. As the viscous layer flows, it leaves gaps at the sidewalls, as the sand is not directly attached to the sidewalls. Analogue models with similar brittle-viscous set-ups, but without such effects, have probably only a limited (narrow or thin) viscous layer or find other (unclear) ways to counteract these problems. When assessing published model results, one can often distinguish such boundary effects, they are just not highlighted. We prefer to be open about the experimental limitations and their effects, hoping that future model projects may be aware of this issue and can hopefully take measures or find solutions.

512 and following. The Authors are introducing here a description of technical expedients to reduce undesired effect. See main comments above.

- We prefer to describe measures taken to reduce boundary effects and their success (or not). Please see our earlier replies above.

518 and following. Too many details introduced and described, which make the experimental analysis difficult to follow. See again main comments.

- We agree that there is a high information density in this section. We have partially rewritten, but do stress that these models are important for the overall message of our paper.

539-549. This is a somehow obvious conclusion, familiar to experimentalists.

- We agree that this may seem obvious. Still the total lack of influence of the set-up due to decoupling was surprising to us, as we did expect to have at least some influence from the VD. This highlights the importance of considering viscous rheology, layer thickness and extension velocity in such set-ups. We also prefer to share our findings so that future researchers do not reproduce the same "failed" models.

I think the organization of section 4 somehow exemplifies the confusion between setups and boundary conditions throughout the paper. In fact, although the paper should be focused on the analysis of the different set-ups, the discussion is organized in sections describing the models in terms of rheological boundary conditions (brittle-only vs brittle/ductile models)

- The decoupling observed in the brittle-viscous reference models was the reason we organized the start of the discussion in a brittle-only part and a brittle-viscous part. This way we do not have to repeat the same obvious comparison several times and can instead focus on the effect of layering, which is more important here.

573 and following. The Authors should better discuss here the differences with McClay's models in terms of brittle deformation (not observed in the current models, well developed in McClay's models).

- We have added details on why McClay's models do develop clear structures that are absent in our experiments in Discussion section 4.2 'It is worth noting that the results from our experiments with a full rubber base (distributed faulting) differ from those obtained with narrow rubber sheets between base plates (localized and well-developed rift basins, e.g. McClay & White 1995 and McClay et al. 2002; Schlische & Withjack 2009). This is because in the latter experiments, deformation is strongly concentrated above the rubber sheets, with the edges of the plates acting as VDs. These models produce well-developed rift structures, but mix two basal boundary conditions (distributed extension and VDs) making it more difficult to identify equivalent natural conditions (Morley 1999, see also 4.6)'

584 and following. See above comments for the discussion of VD (or conveyor belt) experiments

- We have added details on the nature of the VDs and what they may represent in nature to the methods section.

593. Role of sedimentation not clear. Is it because the sediments are expected to load and therefore reduce the topography of the base of the basin? This has to be clarified.

- The sediments may simply fill the basin, restoring the original topography and adding more material that can be deformed. Without sedimentation, the sand slopes on the moving plates will just reach equilibrium and no further deformation will occur in the sand, no matter the amount of extension (displacement of the base plates or sheets).

607 and following. See comments on the failed models.

- See our earlier replies above.

Section 4.4. As explained above, in my view the experiments investigating variations in velocity are not relevant to the aims of the papers. Moreover, they should discuss in some details the scaling of velocities in the models (some of which may be unrealistically high – see above) and compare the results with many previous works investigating similar.

- See our previous comments explaining why variations in extension velocity are an integral part of this work. We agree that some velocities may seem quite unrealistic when scaling with a lower crust viscosity of 10^21 Pa*s. A more viscous lower crust (ca. 5*10^23 Pa*s) would result in lower equivalent natural extension rates. We have added the velocity scaling for each model in Table 2 (based on a lower crustal viscosity of 10^21 Pa*s).

696-698. Sentence not very clear.

- It is rephrased

706. Brun and Beslier?

- Thank you for mentioning this paper, they indeed develop a similar double trough. We have included the reference.

735 and following. But I wonder why the results are so different with a somehow similar set-up. This is not explained and deserves a more detailed analysis.

- We have added some details on differences in rubber base model results to Discussion sections 4.2 and 4.6.

Section 4.7. Is the analysis of strength profiles relevant? Many other papers in the past have shown this (see Burov's or Cloetingh's papers or schematization of strength envelopes in Brun 1999 or the classic paper by Buck 1991 among others). So, I do not feel it is important to re-calculate again strength profiles

- Strength profiles can vary to a substantial degree depending on numerous factors (materials, material layering, deformation rate, presence of water and/or melt, temperature etc). Therefore, we think it appropriate to calculate our own profiles and compare with the experimental profile.

795. As said, this is to me due to failed modelling
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- We are afraid that this is a feature that is quite common in analogue experiments, even if not always fully described. Please see our previous reply on failed models.

812. Maybe in some specific conditions, but I guess it is not so easy to generalize these set-ups

- Unless the brittle upper mantle can continuously rejuvenate as a symmetric (!) conveyer belt system, this is a valid statement. However, it is not when we concern an asymmetric conveyer system, which is (after as shift of reference frame) the same as an asymmetric plate base system (see Appendix A1). We added a clarification to the text that now reads: 'The symmetric conveyor belt extension mechanism may not be well suited to crustal-scale models, as the continuous "upwelling" of the plastic sheets resembles a convection cell system, which could be taken to simulate sub-lithospheric mantle behaviour and would therefore be more appropriate for lithospheric-scale models driven by mantle convection. For crustal-scale wide rift experiments we recommend using an asymmetric plate base or conveyor belt mechanism instead, which are the same after a shift of reference frame (appendix A1)'

815 and following. Again, is this reasoning needed here?

- We would like to suggest some ideas for future work, which is one of the goals of this paper, therefore we feel this reasoning is needed.

Table 3. Indicate the scaling of velocity here

- It was already included. We have now also included the scaling (for the reference viscosity of 10^21 Pa*s) in Table 2.

Figs. 1, 3, 12. See comments above

- See reply above.