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Interactive comment on "A systematic comparison of experimental set-ups for modelling extensional tectonics" *by* Frank Zwaan et al.

Anonymous Referee #2

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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.

- 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 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.

- 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.

- 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

- 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.

- 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.

Other comments (numbers refer to lines)

21. 'the' instead of 'our'

29-30. The effect (localization) or (as I guess) the coupling is enhanced by high strain rates?

32-37. See main comments.

46. 'The' instead of 'These'

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.

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.

156. Suggest to change to 'Aims of this study'

158-162. I suggest to move the first sentence to after 'numerical means'

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

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

223. I would change to ‘Distributed extension set-ups’

225. ‘extension’ instead of ‘deformation’

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

261. ‘extension’ instead of ‘deformation’

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.

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.

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

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

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

426. What does ‘poor lightning conditions’ mean?

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.

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

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

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

I think the organization of section 4 somehow exemplifies the confusion between set-ups 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)

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)

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

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.

607 and following. See comments on the failed models.

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.

696-698. Sentence not very clear.

706. Brun and Beslier?

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.

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

795. As said, this is to me due to failed modelling.

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

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

Table 3. Indicate the scaling of velocity here

Figs. 1, 3, 12. See comments above

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