Comment on egusphere-2022-1203

Title manuscript

Tectonic interactions during rift linkage: Insights from analog and numerical experiments

Referee's comments in black
Authors answers in blue

Referee #1 Guillaume Duclaux

This manuscript investigates the causes of faults deflection during early rift segments propagation. Numerous observations of curved fault systems are reported from early continental rift settings, yet the cause(s) of such deflections remain to be understood. Here the authors use crustal-scale analog and numerical models to investigate rift propagation and strain localisation in early rifting stages when isolated continental rift segments interact. The comparison between nature, analog and numerical models is elegant. Thanks to the high-resolution numerical modelling results the authors demonstrate the importance of transient stress rotations at the surface of early rift systems for controlling propagation of rift arms. Although this work is original and focused on basin scale tectonic evolution of propagating continental rift basins it confirms earlier findings proposed from larger scale numerical models (see point 1 below).

The paper briefly reviews published work on rift basin propagation and linkage, then introduces a set of analog and numerical models' setups designed to investigate rift propagation dynamics in early rifting when separated rift branches interact. The different models explore a wide range of rift arms geometries (imposed using a weak seed heterogeneity at the base of the brittle upper crust) and test how these branches propagate in a sub-pristine environment before joining. Modelling results are presented in great details and highlight transient rift segments deflection prior to propagation. The numerical model's analysis explains this behavior with surface stress rotations and conclude that faults bounding rift segments do not necessary align with the regional stress field. This contribution seems well suited for EGU Solid Earth and will be of interest to the tectonics community in general. Overall, the manuscript is original, very well written, well organized, and beautifully illustrated. I would recommend accepting this manuscript after minor to very moderate revisions.

I present below some key points (mostly related to the numerical models and conclusions) for which I have some concern followed by a list of minor comments and suggestions.

1) My first comment relates to the main conclusion of the manuscript that stress re- orientations occur and change over time and with progressive deformation and the call for caution about paleo-stress measurements @ lines 131-134 & lines 735-739. I couldn't agree more! Yet, I believe that these statements are in essence what we wrote in Duclaux et al. (2020): *"Our models, however, show that progressive deformation during Phase 1 extension results in rotation of the extensional shear zones to become orthogonal to the plate motion direction and control the structural style during oblique rifting. Although the stress around the active extensional shear zones rotates (Fig. 3), the progressive rotation of Phase 1 extensional shear zones during widening (Fig. 5) forces a discrepancy between σ2 direction and the strike of the structures that must be accommodated by a minor component of strike slip. Early rift structures are thus critical in controlling the final architecture of oblique-rifted margins, but because of potential rotations they must be used with caution when interpreting the tectonic evolution of passive margins."*

I hope I'm not biased but I believe a reference to our work here (as the paper is already cited in the MS) would be legitimate, as well as a reference to Gapais et al. (2000) paper. I do understand this original work focus on a smaller scale than ours, but the findings seem to match rather closely.

(Full ref: Gapais, D., Cobbold, P.R., Bourgeois, O., Rouby, D., de Urreiztieta, M., (2000). Tectonic significance of fault-slip data. J. Struct. Geol. 22, 881–888.)

Thank you for pointing this out and the additionally suggested literature. We agree that, albeit on a smaller scale, our conclusions well agree with those in Duclaux et al. (2020) and Gapais et al. (2000) that highlight the issues that arise when interpreting passive margin evolution based on local stress and strain data. We adjusted the abovementioned section in the conclusion and included the suggested references there.

“Albeit on a smaller scale, implications from our observations are in agreement with conclusions from previous studies (Duclaux et al., 2020; Gapais et al., 2000). Locally, stress and strain can largely deviate from a regional pattern and merely represent local problems of deformation interferences. In addition, the observed stress re-orientations change over time indicating that stresses measured in natural examples may depict transient stages that change with progressive deformation due to subsequent changes in material strengths. This implication must be considered when processing local fault-slip data when interpreting the evolution of rifts at any scale.”

2) Frictional softening is of primary importance to control fault localisation and propagation in the numerical models and I think a few more words should be added about it in the numerical model setup section. Lines 266-268: I understand that grid resolution varies vertically in the top part of the upper crust of the numerical models (Fig 4). Is there a normalization procedure in place for the softening function to account for weakening with different grid sizes (like in Lavier et al., 2000)? If not cells just below the surface will weaken faster than those deeper. That might have rather negligeable effect on the results, but it should be presented/discussed at least briefly.

(Full ref: Lavier, L. L., Buck, W. R., & Poliakov, A. N. (2000). Factors controlling normal fault offset in an ideal brittle layer. Journal of Geophysical Research: Solid Earth, 105(B10), 23431-23442.)

Thank you for pointing this out. We do not use a normalization scheme for frictional softening. As suggested, we added this information in the pertinent section for clarity.

"… Note that we apply frictional softening as a function of strain within each cell and for simplicity, we do not include normalization accounting for cell size (e.g., Lavier et al., 2000) nor viscoplastic regularization techniques (Duretz et al., 2019; Jacquey and Cacace 2020). …"

3) Line 328: "the fault segments deflect and turn away from each other": Don't they just tend to form at this angle to strike orthogonal to the extension direction rather than "away from each other" as stated for the analog results (line 192). This brings me to the next point which seems worth discussing further in your work.

This is a good point. We find that, while strain rates tend to strike rather orthogonal to the extension (Fig.5 c), the resultant finite deformation expressed by the topography (Fig. 5b) shows rather curved rift segments that deflect away from each other. To our understanding, this is not in contradiction with results from the analogue model. Fig.3 b and c show the resulting finite deformation (i.e., similar to Fig. 5b) where rift segments deflect from an initially oblique orientation and rotate into an inverted oblique direction (with respect to the extension direction). With this respect we find that analogue and numerical results are rather identical where the rift segments deflect away from each other.

4) Line 504: "dip slip faults are favored over oblique-slip faults with a strike-slip component" - According to Brune et al. (2012) analytical and numerical modelling work oblique extension should be favored. I believe this finding should be discussed in more details as it seems to contradict previous work on the subject. Is this because of the rheologies, the boundary conditions? I find this very interesting.

Thank you for pointing this out. In Brune et al., (2012) rift arms of different obliquity compete with each other and the more oblique one wins (and thus all its secondary features, such as oblique-slip faults). This does not mean that oblique-slip should be favored over dip-slip faulting. The striking difference in the model setup of Brune et al., (2012) and our setup is that ours comprises two sub-parallel rift segments (competing for linkage with an opposingly propagating segment; y- and v-seed configuration) with identical obliquity. Hence, the degree of obliquity should not control which rift segment is favored in our models. Moreover, Brune et al., (2012) and our study involve two different scales (i.e., lithospheric scale vs crustal scale). In our models, the favoring of dip-slip over oblique faults does occur throughout the entire model run but is most prominent at early stages when strain rates are symmetrically distributed and the system is controlled by the competition of the sub-parallel propagating rifts. We therefore conclude that the occurrence of dip-slip fault is largely due to the initial conditions (i.e., the presence of the symmetric seeds in the v- and y-seed configurations).

We agree that this needs some clarification and needs to be discussed in the pertinent section. We adjusted these lines accordingly in section 3.7.3.

"… The early symmetric stress distribution in the y-seed configuration model is unarguably due to the symmetric seed configuration (see also Fig. 8a-e). At this stage, dip-slip faulting along the competing sub-parallel rift segments is favored over oblique slip faults identical to the v-seed configuration. It is only after 1.2 million years, when fault activity along the right rear segment ceases that deformation localizes along the left rear and frontal segments and linkage intensifies (Fig. 8m). Successively, localization and linkage occur coevally with a switch from a symmetric to an asymmetric stress distribution and resembles more the stress distribution in the i-seed configuration model (Fig. 8f-j). The model state after 1.2 million years (Fig. 8m) also marks the switch from a symmetric to an asymmetric stress distribution that was formerly dominated by the competing rear rift segments with dip-slip faulting favored along the two competing rift segments (see also v-seed configuration; Fig.8 a-e). After 1.2 million years the system is dominated by the linkage of two obliquely oriented segments (i.e., i-seed configuration). Note that after 1.2 million years dip-slip faulting mostly occurs along the competing rift segment that links with the opposingly propagating segment whereas dominantly oblique slip faults occur along the abandoned rift segment where activity ceases.
…"

5) I find Figure 8 very informative and pretty well designed. It allows visualizing stress deflection at the surface of the models and the surface stress regime at once. There must be an interpolation method used for the stress vectors representation as not all stress markers (one per cell cell) are depicted. Could you comment on this and how does it smooth the signal out?

Thank you! The stress vectors are indeed resampled. For this, we defined an equidistant grid plane in Paraview with the desired grid resolution and resampled the existing unstructured stress data on that structured grid plane.

More importantly, I have some trouble with the location marked with "rotation jump" in Fig 8i. It seems that some of the stress markers are not resolved (non-defined in the caption), so I assume S_{Hmax} could be as depicted or be orthogonal?? How can a jump be argued in this context? I'm not arguing it doesn't take place, only that the marked region chose to highlight it is not the most suitable one... It seems to me that the overall "rotation jump" is related to the transition from compressional to extensional regions, while the gradual rotation relates to region with a transition from strike-slip to extensional. Is that correct?

The choice where to show a rotation jump is unfortunate from our side. At its current state, it appears as if “rotation jump” relates to the flip of E-W striking S_{Hmax} (compressional regime/blue) to the N-S striking S_{Hmax} (non-defined regime/purple). However, we refer to the rapid switch from compressional to extensional regions that is associated with rift boundary faults that prevail tectonic activity over long period (i.e., no rift-inward migration) in contrast to zones where this re-orientation occurs gradually via faults with an oblique-slip in a strike-slip regime, as assumed correctly. We adjusted the position of the label and marker in Fig.8 accordingly.

6) The comparison of rift arms propagation, symmetry, and timing between the different model geometries in the discussion would benefit some additional words related to the consequences of differential frictional softening rates resulting from the different seeds geometries. Rather than comparing time between models, you could maybe compare the amount of extension accommodated at one seed tip?

+ Lines 557-563: Because strain is distributed in the 2 arms in the early stage of the v- and y-models, this difference with the i-model is to be expected. Indeed, the models don’t have comparable strain rates, and frictional softening isn’t as effective in the y- and v- models.

+ Lines 575-576: In the i-model case, frictional strain softening rate is more effective too.

We agree that differences between models are also due to different frictional softening rates in particular branches. But since these differences arise from localisation and competition, we feel that it would not add significant information to compare rift tips at similar total strains.

Minor comments

+ Figure 1: a location map for c and d in the context of the EARS would be a nice addition.

We adjusted Fig. 1 and added an overview map of the EARS. This overview replaces the example of the Turkana Rift system since this example of a natural y-configuration has a more complex evolution than we originally described.

+ line 113: for the multiphase extension I would recommend adding a citation to Duffy et al. (2015) whose work seems relevant in this context.

(Full ref: Duffy, O.B., Bell, R.E., Jackson, C.A.-L., Whipp, P.S. & Gawthorpe, R.L. 2015. Fault growth and interactions in a multiphase rift fault network: Horda Platform, Norwegian North Sea. Journal of Structural Geology, 80, 99–119. DOI: 10.1016/j.jsg.2015.08.015)

Thank you for the hint. The study's conclusions are indeed similar to those of Bellahsen et al. (2005) and demonstrate that observations from the laboratory are in agreement with results from natural examples. We have changed this passage accordingly.

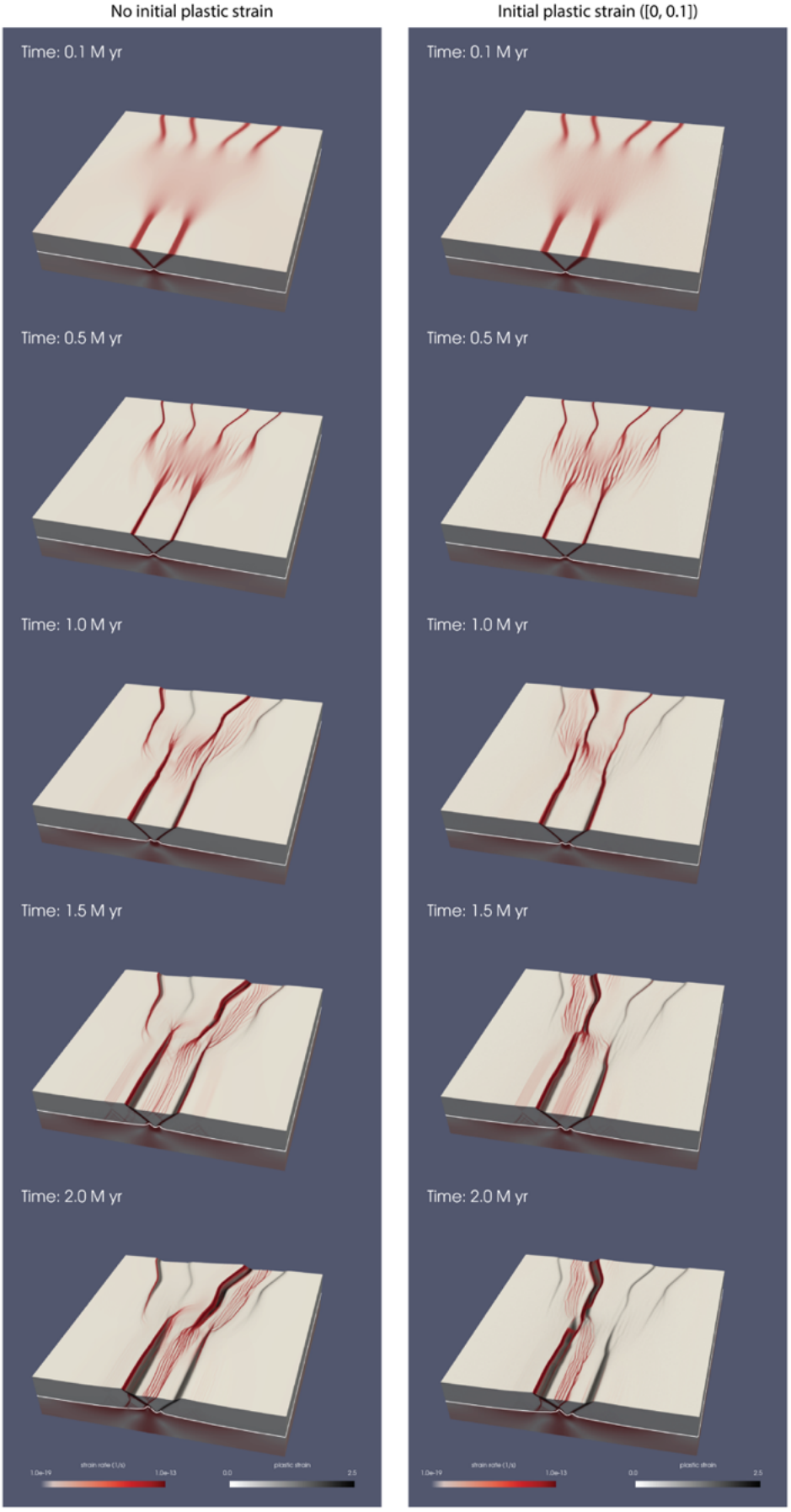
"They suggested that pre-existing faults may disturb the local stress field and impede linkage of newly forming faults which also occurs in natural examples of multiphase extension (Duffy et al., 2015)."

+ line 213: "[...] propagated minimalLY [...]" - missing the LY

Thank you for pointing this out. We changed it accordingly.

+ Figure 5: Just a question: did you try a model without the random seeds to check whether surface ruptures remain symmetrical? I understand the random noise distribution will promote dissymmetry; this is out of curiosity.

Yes, during the testing phase of the model setup we switched the initial plastic strain distribution on/off. In both configurations, eventually linkage of one of the competing rift segments with the opposingly propagating rift segment is favored. However, we find that the presence of initial plastic strain yields more natural (i.e., distinct) fault zones. This is particularly the case in early phases (i.e., up to 0.5 M years; see image below).


No initial plastic strain
Initial plastic strain ([0, 0.1])
Time: 0.1 M yr
Time: 0.5 M yr
Time: 1.0 M yr
Time: 1.5 M yr
Time: 2.0 M yr
strain rate (1/s)
1.0e-19
1.0e-13
plastic strain
0.0
2.5

+ Figure 6: While I can understand the "Curved faulting" contour line for i- and y- geometries, I struggle with v- geometries... there are plenty of faults outside the curved faulting region... and the faults within the regions do not seems to be very curved either.

We agree that “curved faulting” is not an appropriate term for the v-seed configuration. Rather, faults deflect in a fan-shape fashion and successively rotate into an orthogonal orientation (with respect to the extension direction) towards the model margin. We therefore use the term “deflection” in the revised version for the v-seed models and adjusted the text where needed. We also adapted this change in Fig. 6 and the pertinent caption.

+ Figure 9: In the models, main border faults are facing each other’s creating a strong asymmetry of the segments at time of propagation/linkage. On the other hand, in the natural example LT is marked as a hemi-graben with east dipping western border fault, but SV is super narrow graben and doesn’t display apparent asymmetry. Can you please comment on this significant difference?

As commented earlier, we removed the comparison with the Turkana Rift from the revised manuscript since it has undergone a more complex evolution compared to our models. However, we agree that the asymmetry (i.e., half graben vs narrow graben) in models with a y-seed configuration is an interesting aspect that needs further discussion. This feature also occurs at times in models with an i-seed configuration, where most of the strain is accommodated along one prominent boundary fault with a polarity switch across the interaction zone. We implemented this point in the pertinent sections in the discussion.

+ Line 131, 581: I would recommend using the term "heterogeneity" or "structure" rather than "fabrics" throughout the manuscript, but this is just semantic, and I will let the authors decide whether this is the correct terminology. To me a "fabric" relates to a preferred orientation or configuration of all the elements that make up a rock. In the context of this study there is no initial fabric in this sense, but a pre-existing weak structure at the base of the brittle upper crust. A "fabric" would relate to the initial noise distribution within the upper crust region.

Thank you for elaborating this fine difference. We agree and replaced “fabrics” by “structures”, where appropriate. Note that, where related literature is cited, we still use “fabrics” according to the literature.

+ Line 621: I'm a little confused... how can a "discrete zone" be "broad"? Maybe the broad zone could be described as "distributed"?

We agree that this reads confusingly. The broad zone (according to Kolawole et al., 2018) describes a wider zone where faulting occurs clustered (rather than along a single discrete fault) and thus, creates a “wider zone” where faulting occurs distributed. We rephrased for clarity:

“Prominent strain accommodation localized onto a discrete and narrow zone along large rift boundary faults (Style-1; sensu Kolawole et al., 2018) and faulting distributed over a broader zone, where fault clusters may reflect pre-conditioning of the material (Style-2; sensu Kolawole et al., 2018).”