Dear Editors, dear Referees,

We are grateful for the careful reading of our manuscript by both referees. We appreciate their insightful and constructive comments which highlighted some of the manuscripts weaknesses and allowed us to address them.

Below, we reply to individual points raised in the referee reports. In our responses, we outline what changes were made to the manuscript and provide clarifications on the questions and concerns raised. The original text of the report is in black, our response in green

On behalf of the authors

Filip Havlíček

Referee report 2 (Anonymous reviewer)

Review of the paper “Modelling the interactions of discrete inherited structures, pervasive fabrics and extension direction during rifting: application to the Cenozoic Eger Graben” by Havlícek et al

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The papers use different series of analogue models to analyse the importance of inherited discrete and pervasive fabrics during rifting, in conditions of uniform or two-phase extension. The models are run both with the use of a discrete VD and basal foam to reproduce extension, and investigate different geometries of the fabrics. Results are used to analyze the influence of tectonic inheritance in the deformation architecture of the Egen Graben.

In general, the experiments are nice and well illustrated, but I found many different problems in this work –listed below- and I think that major revisions are needed before the manuscript can be considered further.

My main concern is related to the fact that the Authors put together many series of different experiments, with many different starting boundary conditions (e.g. experimental expedients to reproduce fabrics, different model domains, etc.) and different scaling relations which make the paper and the results not easy to follow. My feeling is that putting so (too) many things together may somehow reduce the impact of this paper, as some interesting results may be dispersed among many other less interesting ones. For instance, one could have split this work in two different papers: one (with the VD setup) dealing with the kinematics of deformation, geometry of the VD and resulting fault pattern, comparing this with nature at a

basin scale; another one with the many experiment dealing with the different orientation, distribution, etc of inherited fabrics, comparing them with the characteristics of single faults or fault arrays in nature. This latter could have dealt with many aspects related to fault reactivation (e.g., pattern of fault growth and displacement/length profiles in case of reactivated fabric, kinematics of reactivated faults – see annotated pdf) which are currently not so much investigated. However, at this point, it is rather difficult to rearrange things this way.

Response:

We thank the referee for highlighting some of the deeper, conceptual problems which potential readers might perceive while reading our manuscript. In light of their comments, our revision broadly focused on sharpening the distinction between the two model setups and on clearly formulating why the two complementary modelling approaches were necessary. More specific concerns are addressed below.

Some of the sections of the paper are too long and could be maybe shortened. I refer for instance to section 2.1 and the too much information reported for the pre-Cenozoic history of the Eger Graben, which is not always strictly pertinent to the modelling. So, this section should be summarized and shortened, with only the information pertinent to the experiments left

Response:

We agree that, in its initial form, this section is too long and unnecessarily complicated. Consequently, we noticeably shortened section 2.1 so that it discusses only those aspects of the pre-Cenozoic geodynamic history, which we regard as necessary to familiarize the reader with the regional setting of the study and which are relevant for the modelling work.

Per suggestions in the annotated PDF, figures 1, 2, 3 and 5 were modified and streamlined while figure 4 was removed entirely, since we reevaluated it as complicated and unnecessary for the main topic of the paper.

Another important problem is that the Authors should provide a clearer definition of discrete vs pervasive fabrics: for instance, they can make use of Morley's 1999 Journal of Structural Geology paper to clearly define them and refer to it. Also, a more detailed discussion of how the analogue fabrics transfer to natural conditions is to me important. For instance, as the Authors note, all these inherited fabrics do not strictly represent anisotropies in the brittle layer, as one could expect (and has been modelled in many other papers – see below); they simply reproduce spatial variation in integrated strength starting from lateral thickness variations or from the presence of a VD. How does this compare to nature? More localized deformation is expected when the fabric is reproduced within the brittle layer by –for instance- pre-cutting it and creating a strong internal anisotropy (see below).

Response:

We acknowledge that the precise definitions of “discrete” and “pervasive” inherited fabrics were indeed missing in the initial manuscripts version and the referee is right to point this out. To address that, we have rewritten and expanded the introduction to include paragraphs defining those concepts, mainly as outlined by Morley (1999), per the reviewer’s suggestion. There, we also discuss the importance of scale-dependence of these concepts, both in nature and in how they are typically implemented in analogue models.

Regarding the second part of the comment, the referee is right to point out that our method for introducing “fabrics” relies solely on producing lateral strength variations in the brittle layer (which we acknowledge in the manuscript) but does not produce a true pervasive anisotropy in the analogue material. To avoid any confusion on this, we use the term “distributed fabric” to describe our model setups of series T and A from the introduction onward in the revised manuscript. Together with that, we revise the introduction and methods sections to state that our “distributed” fabric of regular grooves serves just as an approximation of the role of pervasive fabrics in nature and is NOT a proper scaled analogue to a foliated block of crust.

The resulting fault pattern in the models which incorporate these fabrics (model series T and A) is comparable to the effect of pervasive fabrics on normal faulting in nature, as we show in section 5.5. Admittedly, this similarity is rather superficial which we acknowledge in the revised version.

Also, and very importantly, the use of a VD to simulate a discrete fabric may be appropriate, but the experimental series involving it investigates the kinematics of polyphase deformation not strictly the role of the fabric itself. Indeed, the series adopt a similar VD geometry and ‘simply’ changes the extension direction, not the characteristics of the VD. This is classic (‘old style’, I would say) approach in modelling, which has basically anything to do with the analysis of the characteristics of the discrete fabric/basal VD.

Response:

We agree that in the initial manuscript, the description of what setup 1 (E-series) actually tests may be confusing. Therefore, in the revised introduction (Chapter 1) and methods (Chapter 3) sections, we emphasize a sharper distinction between the two model setups and we clearly state that the main tested variable of that experimental series is the imposed kinematics rather than the VD geometry. The VD geometry is a prescribed boundary condition, identical for all the 4 models of E-series. Nonetheless, we still consider the segmented VD geometry very important when applying the models to the Eger Graben setting. There, it serves as a representation of the inherited Late Paleozoic fault zones whose possible reactivation is one of the main topics of the paper.

Moreover, placing a layer of viscous material on top of the VD surely complicates things, as this layer decouples (as the Author correctly indicate) the base of the model and the ‘discrete’ fabric from the uppermost brittle crust. As such, the viscous layer is redistributing deformation, which also depends on the applied strain rate and –according to this- may be

localized or diffused. So, describing such a set-up in terms of discrete vs pervasive fabrics may be misleading and surely not straightforward.

Moreover, this initial boundary condition (i.e., presence of the decoupling viscous layer) is really confusing as the Authors admit that this layer is not present in nature. So, why has it been introduced? – I guess not to have a very localized deformation above the VD, but this has to be discussed more. Not really clear this point.

Response:

As outlined in our response above, the revised version aims to clearly state that the main tested variable in model series E is the imposed kinematic history while the discrete inherited VD geometry is a predefined boundary condition, albeit an important one.

We very much agree with the referee that currently the presence of the basal PDMS layer in setup 1 can seem confusing and not entirely justified. Therefore, we have rephrased parts of Chapter 3, particularly around lines 321-331 to clearly state that the PDMS layer serves primarily as a technical expedient to distribute strain due to the use of a VD setup, which itself should be considered a limitation. In the natural prototype, the PDMS layer thus approximates a “general”, largely hypothetical, weak layer within the rheological profile rather than a defined rock layer. We keep the scaling of this layer to salt, since that would represent the most realistic natural analogue, while explicitly acknowledging its absence in the Eger Graben.

Limitations divided for the two series is again somehow confusing and go back to the previous point related to the split of the two different approaches/experiments/results. For the ductile layer (section 5.3) the Authors provide again a lot of details, which are at least in parts confusing: in general, the rheology does not seem to match that of the natural example, and this is the point. In one of these limitation sections (5.5) the Authors state “Due to the mechanically isotropic nature of granular materials such as quartz sand, modelling the effects of pervasive fabrics in commonly used rift sandbox setups has long been considered difficult to achieve and thus only a limited number of analogue studies exists (Chattopadhyay and Chakra, 2013; Morley, 1999; Samsu et al., 2021).”. This is actually incorrect as many papers (Bellahsen and Daniel, 2005 JSG; Corti et al., 2007; Zhou et al 2024 Tectonics, among others) have for instance used pre-cuts to model brittle inherited fabrics. As an alternative to cut, other papers (Wang et al., 2021) “pre-create” faults during a first deformation phase and analyse in detail their reactivation during a successive, non collinear phase of extension. Note that a comparison with the main findings from these previous papers is lacking in 5.4 and should be improved.

Response:

We have unified both the “limitations” sections (5.3 and 5.5) of the discussion into one which addresses the technical limitations of both model setups together. Additionally, section 5.3 was shortened and merged with 5.2, since it discusses the limitations of the

applicability of our models to the Eger Graben (EG) rather than technical aspects of the modelling approach.

The contents of (former) section 5.3 were modified to 1) state that the rheology of the model does not precisely match the natural example, as highlighted by the referee while 2) emphasize that the main role of the PDMS layer model series E is as a technical expedient which allows for distributed deformation above the VD. However, we still argue that if some level of decoupling within the Cretaceous layer beneath the EG is assumed, our models still may be applicable to the EG setting, with due approximation.

We fully agree with the referees reservations about section 5.5 which, in its initial form, was oversimplified and did not reflect the published literature on the topic fairly. Therefore we have included the comparison of our approach for modelling inherited fabrics with the results of previously published studies highlighted by the referee in section 5.4 and in the new “limitations” section; especially those featuring “pre-cut” and “pre-created” faults (e.g. Zou et al., 2024; Maestrelli et al., 2020; Wang et al., 2024 and others) which were, regrettably, omitted in the initial version.

The comparison with nature is –at least for the experiments with VD- to me not convincing. Experiment E8, in its central part, is dominated by long, linear E-W faults; there are at least 5-6, 40cm-long E-W faults. But this is certainly not the case in nature, where the central parts is characterized by the presence of a few E-W fault segments (and there is only ‘major’ E-W highly segmented fault). So this pattern is to me more similar to experiment E13, where the central part is more segmented, although still characterized by E-W fault. Note that this latter also presents some WNW-ESE or NW-SE-trending minor fault segments, which may be somehow comparable to the similar fault set in nature. So, to me, the kinematics of experiment E13 more resembles nature than that of model E8. In any case the comparison is somehow confusing, highlighting portions of the natural rifts which compares to different portions of the models with difference kinematics, other portions which do not well resemble natural features, etc. So, the comparison is not straightforward at all.

And also the conclusions of the paper leave the reader with a sort of doubt and a sense of a lack of a main summary and integration between the two series (with or without VD) of experiments, see for instance lines 730-736 or 756-759. Has the direction of extension changed over time and are the VD experiments applicable and relevant? This is not very clear and somehow disappointing.

Response:

We rework section 5.2 with the aim of clarifying the perceived deficiencies. In the revised version, we focus more on comparing corresponding segments of individual models and the natural prototype. While the comparison is admittedly imperfect, we argue that the for the outer segments (W and E) experiments starting with SE directed extension are more similar to corresponding segments in the natural prototype while the center segment shares

some aspects of both model categories (based on the initial extension direction). We conclude that there is not enough definitive evidence for either of the tested kinematic histories. Instead, we argue that many of the characteristics of the EG fault pattern and its segmentation largely arise from the influence of inherited features in structurally distinct basement domains. Furthermore, we argue that fault sets of different orientations could have been active coevally due to simultaneous reactivation of inherited fabrics, rather than sequentially as postulated by Rajchl et al. (2009) or Špičáková et al. (2000).

18 figures are to me too many – this is anyway related to the (too) many experiments and experimental approaches introduced in the paper, as described above

Response:

To partially address this concern, we have removed figure 4 from Chapter 2, which we deemed unnecessary upon revision.