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Interactive comment on "Influence of inherited structural domains and their particular strain distributions on the Roer Valley Graben evolution from inversion to extension" *by* Jef Deckers et al.

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Dear Editor/Authors,

Thanks for giving me the opportunity to review manuscript se-2020-23 ('Influence of inherited structural domains and their particular strain distributions on the Roer Valley Graben evolution from inversion to extension'). The general focus of the paper should

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be of great interest to the general readership of Solid Earth, as well as this Special Issue on ‘Inversion Tectonics’. The numbers below refer to specific numbers in the manuscript.

1. Rather than starting with the specifics of the study area, the Abstract might benefit from a more general sentence (or two) on the generic issues to be tackled in the paper (e.g. strain partitioning during inversion). By doing this, the paper may more immediately appeal to a broader, more general audience; e.g. the reader may not be particularly expert or interested in the Roer Valley, but may be concerned with the far wider, more general topic of basin inversion. 2. I do not follow this section of text, especially the last sentence in the paragraph; i.e. how does the similar strain distributions show the importance of inherited structural domains? Please be more specific. Also, given the rifting width is narrow in the south than the north, and that the magnitude of extension and contraction was the same between the two domains, does this mean that there were: (i) fewer, larger displacement normal faults; and (ii) a greater amount of reverse reactivation per fault, in the south? 3. It is not clear why segmentation is mentioned at this point in the Abstract. It might work better earlier in the Abstract, when you describe the overall (present) structural style of the study, and before you discuss the kinematics (i.e. before the last few sentences in the first paragraph). 4. The last sentence of the Abstract does not really make any clear statements about the inversion aspect of the study; instead, it principally focuses on rifting. This is surprisingly, given the Special Issue is about inversion tectonics. 5. Like the Abstract, the start of the Introduction is rather focused on NW Europe in general, and the Roer Valley in particular. It might help to make some broader, more generic statements about the repeated reactivation (in extension and contraction) of basin-bounding faults. For example, the rationale-style statements in the last two sentences in the first paragraph of the Introduction might be brought to the start of this section. 6. On L59-61, where you mention “stratigraphic distributions”, it might also be worth mentioning “isopachs” (i.e. thickness maps), given this is, I think, what you are referring to. 7. L87-105 – Despite being syn-rift, the uppermost Oligocene to Recent strata appears to be rather widespread

and tabular in the stratigraphic column presented in Fig. 2. Why are these units not more locally developed within the Roer Valley Graben, in a similar way to the Jurassic units? Or are these syn-rift units present on the basin flanks, but substantially thinner and/or punctuated by unconformities related to rift-flank uplift/non-deposition? 8. L109 – you here mention the Paleogene-to-Neogene extension direction, but what was the shortening direction associated with the sub-Hercynian compressional phase? You do not mention this near L68-71 in the preceding paragraph. This is very important, given this will ultimately influence whether and how certain faults were reverse reactivated. 9. L110-128 – This text would greatly benefit if some structure maps (e.g. Fig. 5) and/or cross-sections (Figs 7 and 8) were cited. It is presently very difficult to visualise the described relationships in the absence of any graphical support. I sense many such maps and sections have been generated as part of previous studies (e.g. Decker et al., 2019) and have been included in earlier publications, but some of them may benefit from being included again here. For example, a regional, NE-trending cross-section would a very useful accompaniment to Fig. 1. 10. L158-169 - I think it is important to show at least some seismic profiles. If you do not, then the reader has to solely rely on the geoseismic (i.e. interpreted) sections presented in Fig. 7; in my view, this is not sufficient to really convince the reader of your structural and stratigraphic (i.e. thickness) descriptions, and subsequent interpretations and conclusions. I again argue that, although some of these raw data may have been presented in earlier papers, they need showing again here, especially to help the reader visualise some of the interpretation challenges mentioned in, for example, L213-225. 11. L182-196 – Related to comment (10), this section would benefit from one or two simple stratigraphic correlations (e.g. one from the southern and one from the northern domain), perhaps presented next to or below spatially coincident seismic profiles, showing how the main syn-inversion and syn-rift strata change in thickness across some of the key structures. The gridded data in Fig. 6 are useful, as are the cross-sections in Fig. 7, but some hard-data, in the form of a correlation with stratigraphic/formation tops clearly indicated, would strongly support the inversion-to-extension argument being presented. 12. As a general aside,

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I recommend the authors read Freeman et al. (2010) - Using empirical geological rules to reduce structural uncertainty in seismic interpretation of faults. J. Struct. Geol. 32, 1668-1676. This is an excellent paper, showing how simple displacement-length (D-L) plots and displacement 'strike-projections' can be used to help reduce interpretation uncertainty in areas of modest-quality (and quantity) seismic reflection data. In any case, the authors are to be congratulated on a very open, honest discussion of the uncertainty in their structural model. 13. L248-251 – I think this text needs modifying, given that structure maps alone say nothing about kinematics; i.e. they tell you about present-day basin structure, but not about the motion history (i.e. kinematics) of those structures. Kinematics are best revealed by isopach maps, stratigraphic correlations (see comment (11)), cross-sections (e.g. Fig. 7), etc, both of which tell you about the timing of structural movement as recorded in the related uplift and subsidence. 14. L263-269 (and elsewhere) – I suggest you use the cross-sections in Fig. 7 to help support your structural descriptions. 15. As stated above, the cross-sections in Fig. 7 would greatly benefit from the addition of the location of boreholes. This would help make the interpretations more convincing; i.e. at the moment, the reader of this particular paper has to simply trust that the geometries, depths, etc, presented in Fig. 7 are true, without supporting data. 16. The title of sub-section 4.1 may benefit from modifying, given you provide a description of the present, rift-related structural style (e.g. fault throw, spacing, length, etc), but not the kinematics of rifting. 17. L346 – change "doesn't" to "does not". 18. L345-348 – The fact that strain is the same but more diffuse in the northern domain than the southern domain is a very important, which is currently described in a rather qualitative manner. One option would be to actually quantify this relationship by measuring and summing throws (or heaves) along a series of broadly fault-normal (i.e. NE-trending) profiles in the northern and southern domains (e.g. Wilson, P., Elliott, G.M., Gawthorpe, R.L., Jackson, C.A-L., Michelsen, L., & Sharp, I.R. (2013). Geometry and segmentation of an evaporite-detached normal fault array: 3D seismic analysis of the southern Bremstein Fault Complex, offshore mid-Norway. Journal of Structural Geology, 51, 74-91). This would be a powerful addition to the paper, and make the segmentation argument, which is currently only really supported by three cross-sections, even more compelling. 19. L352-355 - What data indicate that the Chalk Group is missing in the axis of the Roer Valley Graben? It is shown as being absent in the cross-sections in Fig. 7, but is this directly constrained by deep boreholes in this location? For example, is the Cenozoic pre-rift early in demonstrably direct contact with Pre-Cretaceous strata in the rift axis? This query relates back to my earlier suggestion that far more stratigraphic and seismic data need to be shown to support Fig. 7. 20. L359-360 – A key issue relates to the argument that only few faults presently have reverse throws. From what I can see in Fig. 7, all faults are still in net-extension; even the Bree, Dilsen and Rotem faults, for example, all appear to be in net-extension, despite reverse slip vector arrows being drawn at deeper depths. So I again ask, "what data are constraining the interpretations presented in Fig. 7?". Furthermore, classic inversion-related structures, like so-called 'harpoon structures' (i.e. hangingwall anticlines) are absent. As it stands, I see little solid evidence for inversion in the data as it is currently presented. 21. L378-382 – I do not follow the argument that the Bree and Dilsen faults formed only during the sub-Hercynian inversion event, and are not pre-existing, rift-related normal faults that were subsequently inverted (although see my comment (20) regarding the present lack of evidence for inversion). Why do you think this is the case? It is completely implausible that they are Late Jurassic structures? 22. L392-393 – I cannot see reverse throw of 200 m at the stratigraphic level of the Chalk Group in Fig. 7B. The GBF appears to be in net-extension along its entire dip extent. This comment also applies to the start of section 5 (Discussion and Conclusion), where you argue for the magnitude of reverse throw along the various faults in the northern and southern domains. 23. L414-415 - Related to comment (22), this is a critical statement, which is presently not strongly supported by the presented data. I also strongly recommend the authors read Reilly et al. (2017) - https://sp.lyellcollection.org/content/439/1/447, who come to a similar conclusion, but via the presentation of much more quantitative data. 24. L415 – Mora et al. (2008) is not in the reference list. Please check all references. 25. L426-428

– Why would footwall shortcut faults be less prone to being reactivation (in extension) than other faults? Is it because they have gentler dips, thus are essentially 'locked' due to the normal stress exceeded the imposed (extensional) shear stress? 26. L447-454 – I agree there is a change in structural style between the two domains across the GBF, but why does this happen? More specifically, why are more fault required in the northern domain than the southern domain to accommodate the same extensional strain?

In summary, this is an very interesting piece of work that, as stated above, will be of interest to the readership of the 'Inversion Tectonics' Special Issue of Solid Earth. I am keen to see these data published, given the lack of case studies explicitly focused on the role of fault size in controlling the structural style and partitioning of inversion tectonics. However, as I hope is clear from my comments above, I believe additional work and modifications will help improve the paper. For example, more focus on the fault size issue, as shown so clearly by Reilly et al. (2017), would make this a very strong contribution. In general, the English and grammar are good; there are, however, many places where these could be improved. Note that I started to edit a hard-copy of the manuscript, but this was taking a considerable amount of time; I thus encourage the authors (and editorial office) to very closely read future versions of the manuscript.

I am more-than-happy for the authors to contact me to discuss any of the issues raised in my review.

Yours sincerely

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