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Solid Earth
Discussions

Interactive comment on "Mechanisms of destructing translational domains in passive margin salt basins: Insights from analogue modelling" *by* Zhiyuan Ge et al.

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In the submitted manuscript Ge et al. present a series analogue models that aim to explore different potential factors responsible for the apparent absence of the translational domain in passive margin salt tectonic systems, which contradict the current conceptual models of such systems. The model results are of high quality and would make a valuable contribution to our understanding of salt tectonics.

Please find my general comments and suggestions below. Additional comments are uploaded in the form of an annotated PDF of the manuscript.

Printer-friendly version

Discussion paper

I hope these will be helpful, please feel free to contact me for clarification.

Kind regards,

Frank Zwaan

General comments

Text: The work is well written, easy to understand, concise and a pleasure to read. However, I feel it is sometimes a bit too general with opportunities to explore and explain certain topics a bit more. Therefore I would suggest to add extra description and quantification of results, background, discussion and references/comparisons with previous work at various places. These include details on sedimentation, scaling and migration of deformation. I have added specific comment below, as well as notes in the annotated manuscript.

Presentation/order of model results and choice of parameters: A total of 6 "basins" (models) are presented, which are divided in three "experiments", labeled 1a, 1b, 2a etc. I understand that the models were run in pairs, hence the labeling, but I found it rather confusing ("experiment" is singular, whereas there are 2 models, 1a, 2a, etc. looks like figure references, why not just call them models A-F or A1, A2, B1, B2, C1 and C2 or so?). Also, I would suggest to consider reordering things a bit as the current organization seems a bit random; there is no obvious logical change of parameters from model to model it seems? For instance, current 2a and 2b differ in more than one factor, so presenting them together as a pair may provide a direct comparison challenging. Similarly, it may also be difficult to directly compare other models? Was this done intentionally? Are there any additional models available to bridge the gaps? (e.g. between current 2a and 3a, which have both different sedimentation patterns and different pre-kinematic layer thicknesses). Were specific models rerun to test reproducibility?

Specific comments

Scaling: could you add the equations used to obtain the scaling values in table A1, either in section 2.2 or with a bit more background in the/an Appendix? Why the distinction between subareal and submarine salt basins? These models represent submarine salt basins I assume?

At some points in the methods part, the authors mention only model dimensions, where it may be helpful to add the associated natural dimensions. See also annotated PDF.

Please add more details on the set-up (e.g. type of confinement, basal friction)

I suggest adding more details on how sedimentation was applied: how is the wedge-scape sieved and how precise is its shape? How are the minibasin deposits applied?

The “uniform” sedimentation in these models is characterized by aggradation rather than by progradation. I believe the latter is supposed to be more common (at least in models?). What is the reason to use an aggradational sedimentation pattern? And does it influence the results? (could you compare with previous works?)

Could you add some details on minibasin formation and spacing? This is quite interesting and important I think, yet only shortly mentioned in the methods by means of a reference to other work. Is it realistic to have minibasins all over a passive margin salt basin? Widespread minibasin formation may be something more typical for e.g. the North Sea, where post-salt rifting in the Triassic caused the creation of such a setting.

In the discussion, please include the work by Brun and Fort (2004) in Tectonophysics (https://doi.org/10.1016/j.tecto.2003.11.014), as well as Fort et al. (2004) in MPG (https://doi.org/10.1016/j.marpetgeo.2004.09.006), who also describe migration of deformation, even when using a thick pre-kinematic layer, which is in contrast to the results presented in this manuscript? Please make sure to cover all relevant literature → e.g. the book chapter by Warren (2016) may prove useful (https://link.springer.com/chapter/10.1007/978-3-319-13512-0_6)

Also, please make sure to fully describe the migration of deformation in the models. It seems that only the migration of the compressional domain is addressed, whereas that of the extensional domain received little attention?

I would suggest adding some more annotation to the top view figures, especially 4, 6 and 8 in order to help the reader distinguish important details. Please consider giving every sub-image its own label (a, b, c, etc.) that can be used for referencing in the text. I sometimes had some trouble finding in the images what was described in the text.

Please check further comments on Figures in the annotated PDF

The link to the supplementary material does not seem to work, so I was not able to check that

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-2/se-2019-2-RC1-supplement.pdf

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-2, 2019.