

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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Dear Dr. Frank Zwaan,

We thank you for reading and commenting on our manuscript and being so positive on it. We have prepared detailed replies to the comments as follows. Please find the revised text and all the figures in the attached file.

The following is a point to point reply to the overall remarks.

1. General comments Text: The work is well written, easy to understand, con-

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cise 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.

AUTHORS: Thanks! We have addressed all aspects the reviewer commented on, including rearranging the model descriptions and improving background, discussion as well as methodology. Please find them in the revised text.

2. 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, 2aetc. 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?

AUTHORS: The other reviewer also raised a similar issue regarding the order of presenting the model results. So we have rearranged all the models to group them into categories of testing the sedimentary cover thickness and minibasin loading to provide a more systematic comparison of different models. When designing the

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model, Basin 1a (or Model A in the revised text) serves as the main baseline for comparison. For example, Basin 2a (now Model E) has the same sedimentation rate as Basin 1a but with differential loading pattern. Similarly, comparing to Basin 1a (Model A), Basin 3b (Model C) has halved cover thickness at any given stage. And Basin 3a (Model F) has the same cover thickness as Basin 3b but with a differential loading pattern. Moreover, we also ran other models to validate some of the thoughts presented here but with a different boundary condition, such as tilting, running time etc., therefore we only presented three experiment runs in the manuscript.

Specific comments

3. 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?

AUTHORS: We have reworked the scaling part including the addition of scaling relations and factors in the table now more rigorously. The reviewer is correct that the prototype we modelled is salt-bearing basins in a submarine setting, which is typical for most passive margin salt basins. The submarine environment has an impact on the scaling because the extra water column reduces the deformation rate. As our experiments are conducted in sub-aerial environment, this results in a modified time scaling compared to systems modelled in a sub-marine environment, which is now described more rigorously.

4. 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.

AUTHORS: We now have reported more corresponding natural dimensions to model

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dimensions when describing the models.

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

AUTHORS: We have provided more details in the set-up. The silicone is confined by the sand walls and base. We have to be honest that we have no constraints on the strength of the interface between sand and silicone. The basal friction (between sand and plastic base) is not relevant here since it is high enough to prevent any slip across this interface. Please see the point to point answer 11 for more details.

6. I suggest adding more details on how sedimentation was applied: how is the wedgescape sieved and how precise is its shape? How are the minibasin deposits applied?

AUTHORS: We have added more details on sedimentation as how the pre-kinematic layer, syn-kinematic wedges and minibasins are created. The pre-kinematic layers and minibasins are relatively precise in shape. However, the sieving of wedges is loosely confined as more sediments will be put into the area with deformation and topographic low, such as extensional grabens/faults. Now we have added the relevant information in model design for clarification.

7. 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?)

AUTHORS: We did this sedimentation pattern to exclude major influences from sediment progradation, which has been shown to have an effect on translational domain evolution previously. We point to this mechanism in the discussion (strong sediment

progradation along can modify the translational domain by forward shifting of extension on the previous contractional domain, McClay et al.(1998).

With the sedimentary wedge shape, we indeed put more sediments in the upslope area. However, it is worth mentioning that during actual sieving, when deformation localization occurs, such as extensional grabens, more sediments are necessary to add to those areas as they are topographic lows. Now we have added that information in the description of model design to clarify the sieving procedure. Furthermore, based on the evidence of Model D, where no differential loading was applied, the basin evolution of Model D is similar to other basins, we argue the differential sediment input has a negligible impact up domain partition and kinematic evolution in our experiments (A, B etc.).

8. 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.

AUTHORS: We have added more information regarding the minibasins formation and change of sieving pattern in the relevant section. Minibasins indeed occur throughout salt basins in the passive margin, but their driving mechanism can be quite different. For example, minibasins (or growth synclines) in the contractional domain usually associate with contraction rather than sediment loading. However, in our modelling, minibasins were created (by sediment loading) to observe minibasin influences on strain transfer between upslope extension and downslope contraction rather than to reproduce the initial development of minibasins, such as those in the Central North Sea.

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9. 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 the 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)

AUTHORS: We have added more references in the relevant section within the discussion. Regarding the work from Brun and Fort 2004 and Fort et al., 2004, the former focuses on contractional structures and the latter discusses differential loading, therefore the two references are included in two different sections of discussion.

10. 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?

AUTHORS: We have added more description of extensional domains in the result part.

11. 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 reference 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

AUTHORS: We have revised relevant figs presenting model results as well as giving more annotations to top view maps in the text.

12. The link to the supplementary material does not seem to work, so I was not able to

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check that

AUTHORS: During the review process the supplementary material is available from a temporal preview* link. The doi will be issued once the paper is accepted.

*<http://pmd.gfz-potsdam.de/panmetaworks/review/36980d98249d861dabcd19b8331a044b327cf5efe25f553613dc4ef2a92>

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The following is a point to point answer to comments on the attached PDF file. Minor corrections in accordance with reviewer's suggestions such as choices of word are not reported unless we choose another word or phrase. Suggestions to images are all implemented and can be found in the new images. Note the page and line number (P?L?) is based on the commented (submitted) version of PDF.

1. P1L1: Although it may be correct, the title feels a bit off I think. Maybe consider: "Mechanisms of translational domain destruction in passive margin salt basins: ..."?

AUTHORS: The other reviewer also had a similar comment, we have used the word "overprinting" instead of "destructing" in the new title and other places throughout the manuscript.

2. P1L10 Maybe specify "theoretical models"? (vs. the observations from nature mentioned in the next sentence)

AUTHORS: We now have reworded it as 'conceptual models of gravitational tectonics'

3. P2L11 "supposed to be characterized"? (contrast with observations from nature)

AUTHORS: We have reworded 'characterized by' as 'typically depicted as a' (suggested by another reviewer).

4. P3L12 "originates and evolves and ascertain" rephrase.

AUTHORS: We have reworded 'originates and evolves and ascertain' as 'originates and evolves and investigate'.

5. P3L20 Please specify why such (Withjack and Callaway, 2000) materials are proper

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analogues for modelling salt tectonics

AUTHORS: We have added the sentence ‘Quartz sand is suitable to model the supra-salt cover sediment due to its brittle nature. Similarly, silicone oil and salt both behave in a viscous manner in model and nature, respectively.’

6. P4L5 Please This however depends on the thickness of the cover (Jackson and Talbot, 1986). see Fig. 2. here: <http://cires1.colorado.edu/people/jones.craig/WUStectonics/SaltTectonics/index.html>

AUTHORS: We agree that the density ratio between cover and salt is depth dependent. However, as demonstrated by Allen and Beaumont, 2012, the development of structures is more sensitive to overestimated buoyancy than underestimated one, therefore, a low density ratio is suggested. Also, it is a generally accepted approach to mix sand and light material to achieve a light density ratio (e.g. Dooley et al., 2017).

7. P4L15 please add equations/calculations here or in appendix

AUTHORS: We have reworked the scaling section and now give the most important equations. Scaling relations and factors are now also reported more rigorously in the Table.

8. P4L20 This is not very clearly explained. Where is the Ramberg number used?

AUTHORS: We have reworked the scaling section and make clear the role of the Ramberg number “

9. P4L23 what is "t*sm"? please specify sm

AUTHORS: Here we use ‘sm’ as short for submarine. In the revised version we use

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only use t* to avoid any confusion.

10. P4L30 I believe there should be more works that can be referred to (e.g. Brun and Fort 2004)

AUTHORS: We have added the suggested reference.

11. P5L1 which represents xxx km in nature? Is this relevant? It is twice the same basin? Does the sand have an effect? Is the model confined by sand on all sides? What is the friction at the base?

AUTHORS: We have completely rewritten the two sentences to make it clear.

‘Two basins of 35 cm (35 km in nature) wide and 90 cm long (90 km in nature) are built on the wedges separated by a 4 cm wide sand wall in between and bounded by two 3 cm wide sand walls on the outside boundaries (Fig. 2a). The silicone is put into the basins and confined by the sand walls (Fig. 2a).’

‘No frictional deformation occurs in the base sand wedges.’

12. P5L6 how much is this per Ma (in nature)?

AUTHORS: We have added ‘(0.17°/Ma).’ in the main text.

13. P5L10 Why 3.5EŽ? How much Ma does this represent? Is this static position something that we observe in nature? Is there a specific reason for the 36 hours?

AUTHORS: We have added ‘(three and half days or 21 Ma in nature).’ to make it clear. The margin tilting is generally up to a few degrees, please see Brun and Fort 2011 for more details. The static position is simply a result of ceased tilting. And the 36 hours is just a time window to observe what happen after tilting stops (e.g. the basin wide strain rate decreases). We have also added this point in the text for clarity.

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14. P5L18 I would propose to call it 3 pair of models (see general comments)?

AUTHORS: We have added word the sentences as the reviewer suggested.

15. P5L23 Here and in the following it would be useful to have more specific labeling instead of generally referring to "Fig. 3"?

AUTHORS: We have added A-F in the Fig. 3 with reorganized order.

16. P5L28 How is this done? A bit more info would help the reader. Why is this? Is this transition something that is common in nature? Was sieving always done at the same location with respect to the set-up (external reference frame) or with respect to the downdip moving minibasins (sedimentation in the depocenters)

AUTHORS: Regarding the comments above on minibasin development, we have rewritten the part of the model setup to give more details on why and how we built the minibasins. Essentially, we created the minibasins by sieving an extra layer of cover material along strike, on a string in the silicon basin to create minibasin downbuilding. We did such differential sieving in the first three rounds during which the extra sediments were put on the exact location of the minibasin. However, afterwards, we shifted to more regular sieving of wedge shape sedimentation as the differential loading pattern had been established.

17. P6L5 Two parameters that differ with respect to previous models, difficult to compare with previous models...!

AUTHORS: We have reorganized the order of the models as mentioned above.

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18. P6L20 Is it possible to be more accurate than the pixel size?

AUTHORS: We have used 0.1 mm instead of 0.1 px to give a more straightforward evaluation of the resolution.

19. P6L23 Can V_x and V_z be used to quantify and discuss deformation in the models? I believe for now it is only used for map view pictures? Why not use it?

AUTHORS: We agree it is an important question as different dataset highlight different characteristics of the model. In general, V_x is useful to highlight the translational domain and V_z is good at showing the salt outflow and inflow. However, they are not as good as strain in showing the structural and kinematic evolution of the silicon basin. Therefore, we only show V_x and V_z as map view in the results. Now we have added some of the information mentioned above into the main text.

20. P7L6 Ma in nature?

AUTHORS: We have revised the text as '25–36 hours (7–9 Ma in nature), 61–72 hours (16–18 Ma in nature) and 109–120 hours (28–30 Ma in nature)' to show the corresponding time in nature.

21. P7L26 Please add more details on the extensional domain and its evolution.

AUTHORS: We have added 'Afterwards, the extensional domain continues to expand to the end of the experiment, reaching to over 20 cm wide (Figs 4b, c and 5a).' in the revised text to describe the extension evolution of Model A (previous Basin 1a). We also added more details of the extension of Model B, C and F (previous Basin 1b, 3b and 3a) in the text as well.

22. P8L6 and P8L10 Can you indicate this on the image? Please specify. I assume

the diapirism causes the extension seen between the passive minibasins? Please annotate in the Figure

AUTHORS: We have indicated the diapirs in Fig. 9a and b.

23. P8L21 quantify??

AUTHORS: We have replaced the phrase 'late stage' with more precise time 'after 72 hours'.

24. P8L33 Where can we see this??

AUTHORS: We have added annotation on Fig. 9b to highlight the impact of differential loading.

25. P9L25 Please add references here

AUTHORS: We have added relevant references in the sentence.

26. P10L7 Yet Brun and Fort have very different results!?

AUTHORS: In Brun and Fort 2011, in their dominant gliding model (Fig. 15), there is a clear translational domain between upslope extension and downslope contraction. Although it is less obvious in the model of Fig. 10.

27. P10L15 Please compare with Brun and Fort 2004 and potential other works?

AUTHORS: We have compared the characters of thick cover strata to other experiments, such as Brun and Fort 2004, where the translational domain gets preserved.

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28. P10L20 So that we should expect prograding systems? How well does this fit with the sedimentation applied in this work?

AUTHORS: As we have mentioned above, our sedimentation pattern is not entirely aggradation. Structures with topographic lows receive more sediments during sieving. Moreover, Model D has negligible sedimentation shows the basin evolution is similar to those with wedge shaped syn-kinematic sedimentation.

29. P10L23 Please check Brun and Fort 2004?

AUTHORS: We have commented the reference in point 24, indicating that upslope migration with thick cover can preserve the translational domain instead of overprinting it completely (with thin cover layer).

30. P11L1 This would be information on minibasin formation and shape that should be mentioned before as well?

AUTHORS: We have used the variation of sedimentary systems to justify the creation of minibasins in the model. However, we did not model the minibasins specifically tied to any sedimentary systems or their geometries, such as models showed by Banham and Mountney 2013 on Sedimentary Research where fluvial distributary system dominates minibasin formation. In other words, differential loading in nature may be more irregular in shape than the generic geometry we used in the model. Therefore, we feel it is more reasonable to put the information in discussion rather than in the model design of minibasin creation.

31. P11L7 This may be, but is it realistic to have such large minibasin deposits over hundreds of km (the typical width of a passive margin salt basin). Minibasins may be more dominant in for instance the North Sea, which is not a typical passive margin

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setting. Here minibasins are formed during post-salt rifting in the Triassic. Maybe differential loading is more likely in the case of a prograding sediment wedge (e.g. Gulf of Mexico?) This should be discussed?

AUTHORS: As we mentioned in the reply to overall remarks, the minibasins are created as differential loading to observe their behaviour during the experiments, we did not try to model a specific case as the North Sea. And even in the North Sea, there is also some evidence within the minibasins suggesting gravity gliding (see Karol et al, 2014 in Interpretation).

31. P11L24 I believe also Oriol Ferrer in Barcelona has done similar work that could be referred to?

AUTHORS: Thanks. We have added Oriol's Interpretation paper into the references.

32. P12L14 please specify in which part of the system where the loading is most significant

AUTHORS: We have now pointed out that early loading in the mid-slope is important to deform the translational domain.

Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2019-2/se-2019-2-AC1-supplement.pdf>

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

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