Reviewer #3

Dear Peter B.E. Sandersen,

We thank you for reviewing our manuscript and for the constructive comments you gave. We appreciate your efforts a lot, as they helped us to improve our work. The following tables list our responses to your comments.

Kind regards,

Jacob Hardt et al.

General comments:

Reviewer	**Authors' response**
In the Introduction it is mentioned (lines 56 to 59) that in areas affected by GIA fault zones can be reactivated ('glacially induced faults'). However, this is the only place in the paper where this is mentioned. Obviously, it is not in the scope of the laboratory modelling to include the effect on the upper mantle, but I suggest that a sentence or two are added to the Discussion, where it is discussed whether/to which extent this mechanism could have affected the area chosen as study area.	We agree that ice-sheet induced salt movements and glacially induced faults (GIF) at one point deserve an integrative consideration. By mentioning the GIFs in the introduction, we want to transport the message to readers unfamiliar with the region that the ice advances are capable of triggering deep movements and that the salt movements that we investigate are one possible additional component triggered by the ice. While thinking about your comment, we did not find an appropriate spot in the discussion where to briefly discuss our results in the context of GIFs, especially as this was out of the scope of our experiments. We believe this requires more work in a dedicated study.
The 'Experimental methodology and setup' section starts with 'Remarks on the selection of model parameters' followed by 'Modeling materials and data capture' and finally 'Model design'. I suggest that the 'Remarks' section be moved to the end because the reader cannot necessarily relate to remarks on the individual stages of the modelling before the model has been described. Also, consider moving the sentences of lines 185 to 193 to the end of the 'Remarks' section as it, in my opinion, fits better here.	Thank you! We agree and followed your advice!
In the 'Results' section you mention the crestal grabens that form above the modelled salt structures. As the figures generally are small, please refer more specifically to where on the figures the crestal grabens can be seen (i.e. with arrows).	Done! We've added white arrows to the respective images and added a note on that in the text and in the figure captions. (see below)

As the timescales of salt flow and ice flow are very different, I agree that it is obvious that the loading in the modelling can only be stationary. It is also understandable that the modelling cannot be weighted, and that the ice load in the model setup has to be exaggerated. However, when specifically evaluating the effect of a lobate ice margin, I feel - due to the factors mentioned – that the uncertainties on the model results here must be quite large. I suggest more elaboration on this in the discussion, for instance as a separate part of the discussion dealing with uncertainties.	This is an issue that was also pointed out by the other reviewers. As you suggested, we added a whole new section "remarks on the scaling of our models" to increase the transparency regarding the scaling.
It is mentioned as a result, that 'the reversed vertical displacement after the unloading, caused by the flow reversal of the salt system accounts for only up to roughly 50 % of the vertical displacement that occurred during the loading stage' (sentence from the Conclusions). But there are no suggestions as to why this is happening. Please elaborate on why the system does not return to the pre-loading situation but instead establishes a new equilibrium, and to which extent is it believed that the chosen model setup can be responsible for some of the observed differences (the static load, a non-weighted model, the extrusion etc.)?	Thank you, this is a very interesting remark. We don't think that the system had quite reached a new equilibrium after the end of the unloading stage. Although we made sure that the unloading stage was long enough and we didn't record any significant movements before we finished the models, we do believe that very slow processes of reequilibration would have continued for some time to come. This is an effect of the high body forces applied during the loading stages versus the low body forces during the unloading stages. Also, the back flow was favored by the high connectivity within the pillows, whereas the load resulted in a thinning of the source layer, which decreased the flow reversal capacity outside the structures. This is a complex issue, which will require further work. We added a few lines on that in the new discussion section ("remarks on the scaling of our models"): "*Although we witnessed a flow reversal during the unloading stages, the vertical displacement rates during the unloading stages only accounted for roughly 50% compared to those from the loading stages. This is most likely an effect of the very different body forces involved, which were high during the load stage and low during the unloading stage. In addition, the back flow was favored within the pillows, where the salt is thick and connectivity is high. Outside the pillows the source-layer salt was thinned during loading by expulsion into the pillows and diapirs, and thus the flow resistance through these thinner conduits increased for the unloading stage. This thinning and increased flow resistance impacted the process of reequilibration driven solely by gravity,*

	which we would expect would to have continued very slowly for some time to come. The process of decreasing salt flow in thinning salt layers is well known from research into salt welds and has to do with specific salt viscosities and internal impurities within the salts (e.g., Wagner & Jackson, 2011; Jackson and Hudec, 2017a)."

Specific comments:

Reviewer	**Authors' response**
Line 57-59: Please re-think this sentence and the argument it contains: The orientation of faults parallel to the Pleistocene ice margins does not document a link between neotectonic activity and ice sheet loading.	This was also mentioned by Reviewer #1 and the passage was rewritten accordingly: *"In northern Central Europe, postglacial seismic activity has been identified at several preexisting faults (Brandes et al., 2015; Müller et al., 2021)."*
Line 71: Delete 'at the surface'.	OK.
Line 90: The loading/unloading processes are unrelated to the size/shape of the salt structure. Consider writing 'during the loading and unloading processes'.	Thanks, now it reads: *"This requires an investigation of different shapes and sizes of the salt structures during loading- and unloading processes."*
Line 91: I would prefer to write 'explore the relation between'.	This whole part was rewritten following the suggestions of Reviewer #1.
Line 94: Consider 'Northern Germany constitute an ideal study area, as it is….' instead of 'Northern Germany provides the ideal model region for our study, as it is….'	OK: *"Northern Germany constitutes an ideal study area, as it is rich in various types of subsurface salt structures, was repeatedly glaciated during the Pleistocene, and provides several areas where geomorphological landforms point to a salt tectonic influence."*
Line 103: With 'low-lying', do you mean 'deep-seated'?	Thanks, "deep-seated" is indeed the better term. *"The Mesozoic and Cenozoic overburden on the Zechstein salt varies in thickness in the region between more than 3000 m above deep-seated pillows, to only few hundred m above the highest salt domes (Stackebrandt and Beer, 2015) – some domes in northern Germany even pierce to the land surface (Künze et al., 2013; Sirocko et al., 2002; Stackebrandt, 2005)."*
Line 107: Consider using 'terrain surface' instead of 'free surface'	The whole sentence was rewritten: *"The so-called surface cracks are interpreted as expansion ruptures due to salt flow triggered by loading- und unloading effects of the SIS, which*

	eventually resulted in upwards movement of pillows and domes."
Line 136: Write 'silicone flow' rather than 'salt flow'.	OK: *"Powdered pigments were mixed with the silicone and added as passive markers to several locations in the source layer in order to track the silicone flow."*
Line 159: Please explain what you mean with: 'The GS and KH have a heterogeneous geometry with several peaks'.	We clarified it and changed it to: *"The GS and KH salt pillows have an undulating topography with several peaks."*
Line 162: Should the sentence '…stimulating the debate of the relationship between salt structures and ice sheet extent' be moved to the discussion? In my opinion it is irrelevant here.	Agreed, we moved this passage to the discussion (section "can these models help us…") and slightly modified it: *"Interestingly, the spatial correlation between salt pillows and the W2 ice marginal position has initially led to the development of the theory of a dynamic relationship between salt structures and the ice extent (Gripp, 1952; Schirrmeister, 1998) and our results revealed the largest deformations in comparable settings."*
Line 166. Consider deleting '…thus providing a promising modeling scenario', because it is a subjective evaluation at this stage. If what you mean is that it would be interesting to model a scenario like this because the salt structures were partly transgressed, please rephrase.	Agreed, we rephrased it to: *"[…] which provides a setting that corresponds with the focus of our research questions."*
Line 179: 'Front edge' instead of 'leading edge'?	OK. *"In the later runs, a metal plate with an undulated front edge was used to simulate the lobate nature of ice margins (Fig. 5)."*
Line 212: Consider reducing 'covered by the glacial load during the loading stage' to simply 'loaded'.	OK.
Line 282: 'Here, we will…….attempt to discuss…'. Delete 'attempt to'.	Agreed!
Line 325: 'Keeping the ice dynamics of the two different Weichselian ice advances……in mind,….' What is meant here apart from the spatial extent of the ice advance?	Thank you! I have deleted the term "ice dynamics" and only relate to the different ice extents now. Ice dynamics were not in the scope of this paper and shall be addressed in a different study. *"Keeping the spatial extents of the two different Weichselian ice advances in mind (Fig. 1), the distribution of the surface cracks may be explained on basis of the results gained from our physical models."*

Line 355: '….the advancing ice sheet would push an intrasalt 'bowwave' in front of it, giving rise to the structures in front of it': As modelling does not include the dynamics of the ice sheet and given the large differences in time scales of salt movement and ice-sheet movement, I find that this conclusion is difficult to make based on the modelling.	OK, we deleted this interpretation. It shall be addressed in future projects.

Revised Figures

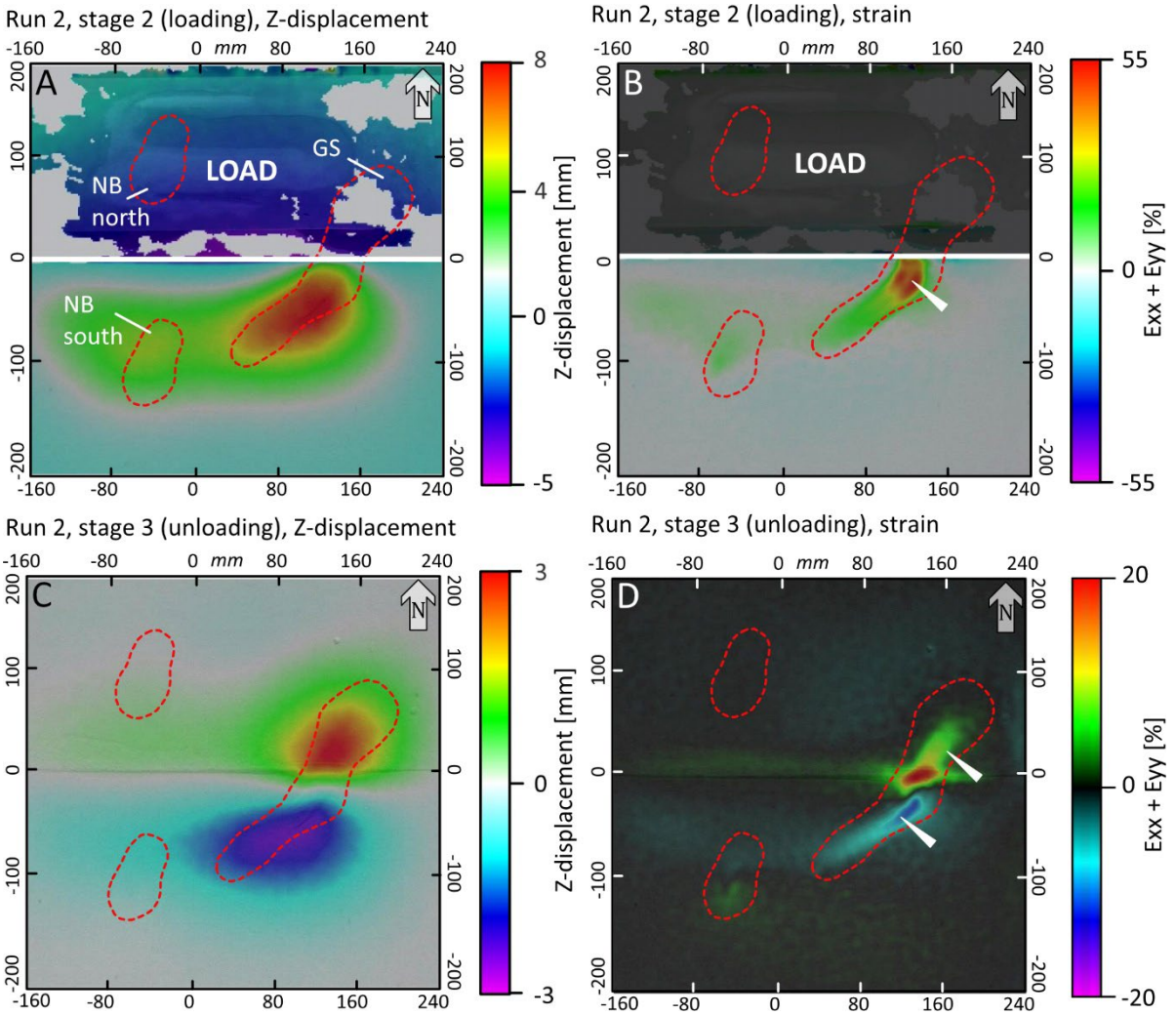

Figure 1: Summarizing DIC imagery of run 2. In stage 2, the load was applied to the north of the horizontal white line. A: Z-map showing total vertical displacement in mm of stage 2. The grey colors in the upper half of the figure are "no data" areas. B: Strain [%] map of the total strain of stage 2. C: Z-map showing total vertical displacement in mm of stage 3. D: Strain [%] map of the total strain of stage 3. Red dashed outlines depict approximate position of salt structures. White arrows indicate position of crestal graben structure.

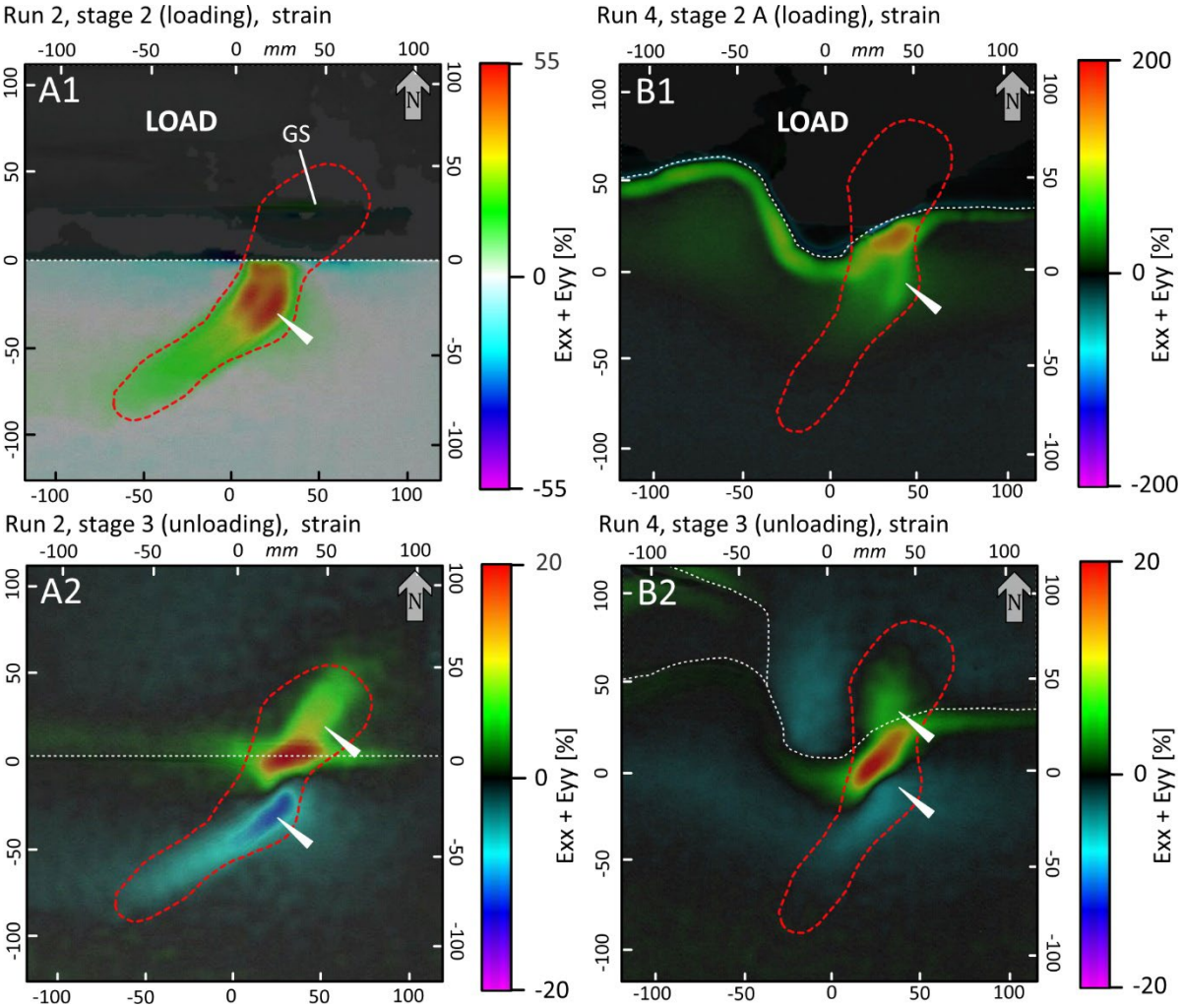

Figure 2: Comparison of strain patterns above GS pillow using different load geometries (white dashed line): Left column - straight load margin; right column: lobate load margin. Red dashed outlines depict approximate position of salt structures. White arrows indicate position of crestal graben structures.