Author's response

Melchior Schuh-Senlis

We would like to thank the reviewers and editor for their review and kind remarks. We will answer here to their review and state the changes made accordingly in the paper. As a general remark, reading the various comments in the review made clear various issues. When writing the manuscript, the wording was often too prompt to put the results as an improvement from previous geomechanical restoration schemes, without really comparing them. The purpose of the article was not entirely clear at this point, and the introduction did not help in that matter. In order to clarify the purpose and results of the article, its goal was re-thought, the title changed and introduction re-written. The idea is to focus on the papers main results, and why they are scientifically interesting. Various changes in all the manuscript were also done to account for the reviewers comments and this clarification of purpose. Hereafter, the reviewer's comment will be shown in blue and the author's response in black.

1 Answer to Peter Lovely

"Application of the creeping flow restoration method to an analogue model," by Melchior Schuh-Senlis, et al. leverages a laboratory scale structural analogue model, which has been imaged through its evolution by a time-series of X-ray tomography images, to validate a novel approach to structural restoration. The restoration method assumes that deformation may be explained by a viscous rheology modeled using Stokes flow equations, and applies reverse time-stepping to reverse deformation of the structure(s). Is see this line of research as a critical next step in the development and demonstration of Stokes flow restoration methods.

However, I have significant concerns about the results that are presented. Most importantly, the analysis of restoration results and comparison with time-series imaging of the analogue model focuses almost entirely (with the exception of Figure 6) on the first restoration step, corresponding to the final stage (18 minutes?) of the forward model. This is only a small amount of the total deformation of the model. The misfit between restoration and forward model at this step is difficult to discern (this is a good thing) but I am sure it becomes greater as the restoration goes further back in time. The authors do not explain their rationale for focusing only on this final step of the forward model, and it leaves me concerned that an incomplete picture of the study is portrayed in the manuscript.

The focus was made on the first restoration step, corresponding to the final stage of deformation, to reduce computation time for the simulations. This part of the simulation shows all the deformation of the top layer of the model, and finishes with a flat top surface, which was considered enough for testing purposes. It allowed, rather than testing the ability of the method to restore the model as well as other methods, to study the impact of the boundary conditions and the material properties on the restoration results.

Additionally, the pressure (Neumann) boundary condition applied to the right hand side, which is suggested as an enhancement to the model relative to kinematic boundary condition, is not sufficiently justified, from my perspective. In viscous flow models, deviatoric stress is related to viscosity and strain rate. How does Poisson's ratio help to calculate an optimal horizontal traction in this case, and, thus, what makes this boundary condition more physical than the previously applied kinematic condition?

On the right hand side of the model, the velocity is not known, and can only be estimated from the deformation of the model. This neglects the vertical variations of velocity, which have a large impact on the overall model deformation. Having kinematic conditions on all boundaries also puts an over-parameterization on the simulation system. The presented Neumann conditions, while simplifying the physics by using Poisson's ratio, both remove this over-parameterization, and are based on known properties inside the model. Moreover, using a traction on the right hand side of the model removes the enforcement of the velocity at this boundary, which was necessary for the material properties analysis.

Perhaps it is beyond the scope of this study, but the final results left me feeling somewhat unsatisfied, with no benchmark how the Stokes flow restoration compares to other restoration methods. I believe that Chauvin, et al. (2018) benchmarked an elastic restoration using the same numerical analog model. Some comparison of the results could enhance the impact of the manuscript without a lot more work.

As stated, the article lost itself in trying to compare the results with previous geomechanical schemes, without it being the purpose of the study. This comparison may be interesting, however, in a continuation of this study.

Finally, there are many opportunities to improve the organization of the manuscript, as well as clarity and precision of the writing. I have attempted to identify and assist with examples in the comments below and in the attached, annotated version of the manuscript.

Most of the specific comments in the annotated version of the manuscript were taken into account (see the track-change file for precise changes in the manuscript).

For these reasons, I recommend major revisions before accepting this manuscript for publication.

Specific comments:

The abstract is difficult to follow. My perspective is that too much focus is placed on general background related to restoration, and too little focus on the background of stokes flow-based restoration. The last few sentences, focused on the contribution of this work, are vague and worded in such a manner the results and implications are difficult to understand.

The abstract was re-written.

The introduction provides a relatively detailed, and helpful, overview of the history of restoration and various methods. However,

- The detail is imbalanced. Mechanical restoration methods receive quite a lot of focus, whereas kinematic restoration methods are described only very briefly

- Validation of geologic interpretation is an important application of restoration methods, but is never addressed

- The direction that the manuscript is going to take could be made clear earlier in the introduction. The review of restoration methods is good, but left me wondering until many paragraphs into the introduction where the authors are going. Perhaps the introduction could be shortened and a separate section or subsection added to review kinematic and geomechanical restoration methods.

- The paragraph introducing analogue modeling methods comes out of nowhere. The purpose of the analogue model for this study needs to be introduced first.

- The final paragraphs in which the direction of this manuscript is laid out might be explained more clearly (see annotations)

The introduction was almost entirely re-written, except for the paragraph on analogue modeling, which was better put in context.
Section 2.1 (Creeping Flow Restoration) is a good overview of the method. Though published previously, it is sufficiently important to reiterate in this manuscript. A few minor suggestions include:

- Describe somewhere the viscosity model you use. I believe you assume a linear viscous fluid with constant viscosity, but this should be clarified
- A brief introduction to the section on what is stokes flow and why it is used here would be helpful.

Indeed, the viscosity model is linear viscous fluids, this was added in this subsection, as well as a brief introduction to Stokes flow and its use here.
Section 2.2 (The FAIStokes code) is summarized significantly and references Schuh-Senlis et al (2020) appropriately.

- Because the numerical method is not the focus of this publication, I wonder if this section could be abbreviated further. Is Figure 2 (flowchart showing numerical implementation) necessary?
- How is the free surface is implemented and (how) is the material above it is addressed in the numerical scheme.

Figure 2 was removed, and more explanations on the implementation of the free surface was added.
In Section 3 (Presentation of the Analogue model), I have made quite a few suggestions and posed several questions to the authors. To summarize,

- The model could be explained better. Specifically, what is the right-hand-side boundary of the model? How are the layers deposited through time? How do the material properties of the silicon, sand, and pyrex scale in both space and time to geologic materials and structures? See annotations for additional detail.
- The description of X-ray tomography and how it is used may not be clear to an unfamiliar reader. Need to specify that it is an imaging method.
- The header title "Available data" is vague and unclear. I would suggest merging this section with the analogue model description in the prior paragraph and relabeling the section something like "Analogue Model Description."
- Section 3.2 (Creation of the Numerical Model): Please clarify what is meant by "numerical model." This section refers specifically to the geometry to be restored and its discretization on the mesh. Stokes flow and the implementation in the FAIStokes code is another numerical model not addressed here.
- In section 3.2, please address the mesh resolution and particle density per cell, in addition to the swarm.

- The final paragraph addressing the use of X-ray tomography images to determine the height of the topography after the deposition of each layer seems out of place. Boundary conditions are not otherwise addressed in this section, and the time-series of tomography images has not been adequately introduced.

The various suggestions were taken into account when possible (data was not always available concerning the analogue experiment). To summarize, the right-hand side boundary was a wall, the layers were deposited at a fixed time, and the specific scaling was not specified. The analogue model description and data sections were merged. Details on the geometry and numerical models were added as suggested.

Section 4 (Boundary condition analysis)

- It might help to put this section in the context of how boundary conditions have been dealt with in previous applications of Stokes flow restoration, especially the 2020 paper by the same authors. I tried to read this manuscript in the context of that prior work and was left wondering because I think that this study begins with a different approach (this may be fine, but I found it confusing).
- I have made several suggestions to clarify wording in the annotated manuscript

Indeed, the first version of the manuscript was not clear about the approach. I hope the various changes made will remove this confusion. The annotations were taken into account in the manuscript.

Section 4.1 (Restoration using kinematic boundary conditions)

- Please specify if gravity is applied in this model
- The paragraph beginning line 223 seems primarily relevant to the numerical implementation, and I'm concerned it will be a diversion to the average geologist reading the manuscript (it took me some effort to understand). Perhaps it could be moved to the numerical implementation section, or explained in a more intuitive manner?
- Figure 6 is a nice time series of the restoration model, but only (b) is compared to the tomography reference image. Why? Also, why is this the only time-series of restoration results? I would like to see a similar time series and comparison to tomography images for the preferred model described in section 5 also. The absence of this rigorous comparison leaves me feeling like the analysis presented in this manuscript is incomplete.
- I have made several suggestions to clarify the captions of figures 7 and 8
- In figure 8, is d_reference(x) always positive? Or does it represent an absolute value? I cannot see in figure

Gravity is always applied in the simulations, this was specified in the relevant paragraph, which was re-written to try and make it more intuitive. Indeed, the analysis was incomplete, as the complete restoration of the model was outside the scope of the study. This Figure was simplified to show only the first stage of restoration and clarify the goal of the article. d_reference is indeed an absolute value, this was added to the manuscript.

Section 4.2 (Upgrading the kinematic conditions to natural boundaries)

- At the start of section 4.2.1, please explain more completely the restoration model to be performed. In particular, what is the boundary condition applied to the top of the model in this case? I had to read on some way for this to be clear.

- It is unclear to me how the pressure boundary condition applied to the right side of the model relates to the forward analogue model. In viscous deformation, deviatoric stress relates to strain rate. Why is Poisson's ratio used here? What makes this boundary condition more physically realistic than the kinematic boundary condition applied previously?
- Building on this, it is unclear to me (and not addressed sufficiently in the paragraph beginning line 273) what might be the implications for model precision.
- In figure 11, it would be helpful to include the topography before restoration as an additional reference, beside the expected flat topography.
- The legend of figure 11 is unclear, because (a) and (b) refer to the right hand side boundary condition, but this is not specified. Reading the text, it says that the top surface is treated as free in fig. 11.
- The caption of Table 6 could be shortened by referencing Table 5 (similar to what is done with Figures 8 & 10
- The last sentence of this section makes a broad statement that is, so far, unjustified. This should be presented as a hypothesis or it should be specified that this will be demonstrated in the next section.

Precisions on the top surface boundary was added. I was confused when writing the article, between the Poisson ratio ν and its common use α in the definition of such a traction specific relation:

$$\alpha = \frac{\nu}{1-\nu} \tag{1}$$

This was clarified, and references were added to justify this relation. The other remarks were taken into account in the manuscript as well.
Section 5 (Model parameters analysis)

- The section header I believe refers specifically to material properties... please say so.
- In line 305 a statement is made regarding the ratio of viscosity between overburden and silicone. Please explain this better. Where does this ratio come from?
- I like figure 13! It's a good visual display of the DOE study.
- In line 325, I do not follow what are the "computational instabilities" shown in Section 4.2.2.
- In the sequence of figures 14-16, the authors focus on results at the final time step of each simulation. However, as a geologist, I am most interested in the optimal time step (i.e. the results when each curve in Fig. 14 is minimized). Could the same analysis be completed on the optimal time step (i.e. show it in Fig. 15) or could an explanation be provided why the final time step is better?
- Line 345 presents a hypothesis which has thus far not been demonstrated as fact. Please present it as a hypothesis or with the caveat that this will be demonstrated in the following section.
- At the start of section 5.2, please specify (in addition to the parameters) which experiment number (from prior section) is selected as optimal.

- Line 353 says a range of multipliers from 0.75 to 3 are used, but Table 7 shows multipliers ranging from 0.75 to 7. Please reconcile.
- Does figure 17 need to be its own figure? It's just adding one curve to figure 14.
- Table 8 includes the exact same information as Tables 5 & 6, plus a new row. Please consider if tables 5 & 6 need to be included as stand-alone tables.
- In figure 18, the difference between restored geometry and reference target (image) is difficult to discern. As discussed previously, please add restoration steps with more deformation or address why only the first restoration step (in which deviation is presumably smallest) is chosen for comparison.

the viscosity ratio was chosen so that the viscosity of the sand and pyrex layers would be greater than that of the silicone (the silicone being a viscous fluid which creeps more easily), and so that the numerical experiments time and errors would be tractable (both increasing with higher viscosity ratios between layers in the model). As the idea was to find the effective material viscosity inside the model, the "optimal time step" would not have given as much information on the parameters that introduced over/under-estimation of the necessary deformation. The tables were reconciled, and the various other remarks were taken into account in the manuscript.
Section 6: discussion

- Table 8, row 3, column 1: the use of "Kinematic restoration" confuses kinematic restoration methods with kinematic boundary conditions in a mechanical (stokes flow) restoration. Please clarify.
- Line 377: I believe this references silicone viscosity, not actually salt.
- Please clarify the caption for Table 8 as requested in annotated manuscript.
- Line 379: please specify what you mean by times scales ranging from seconds to hours. The experiment was 4 hours long.
- The paragraph beginning line 384 overlooks the fact that the last case (optimized material properties) produced the best restoration results. This is key to conclusions and should be highlighted here.
- Paragraph beginning line 390 seems a bit of an aside to the point of the manuscript. It's relevant, but maybe could be moved later in the discussion.
- Line 401: The sentence beginning "It poses. . . " touches on questions around the magnitude of traction applied to the right boundary. I would like to see this addressed more thoroughly (in relation to comment on section 4.2)
- The numerical improvement from the first case to the last is relatively small (see total of first two rows in Table 8). I would like to see more discussion of if it's worth the (large) additional effort of to optimize the model.

All remarks were taken into account in the manuscript, see the track-change file for precise changes.
Throughout the manuscript, wording is frequently awkward and/or imprecise. I have tried to identify and assist with the more significant examples in the attached, annotated version of the manuscript.

Thank you for taking the time to send me this annotated version of the manuscript. Most of the commentaries were taken into account in the final version of the manuscript.

2 Answer to Anonymous Referee

Schuh-Senlis et al. present a meticulous study aimed at determining the optimal method for restoring an analogue experiment that represents a geological event. This is achieved through the application of an inversion scheme utilising Stokes' equations, with velocities inverted to facilitate backward stepping in time (restoration).

The study draws attention to the impact of velocity and pressure boundary conditions on the success rate of inverting a numerical model. The numerical model, derived from the initial conditions of a late stage analogue experiment, demonstrates that while pressure boundary conditions, despite their greater realism, do not yield significantly superior results, they underscore the importance of material properties, such as viscosities along individual faults.

As a reader, the expectation was to find a robust solution to the problem. Towards the end, however, there is a sense of frustration at the need to delve into material properties individually through a parameterisation study. Nonetheless, this meticulous approach is considered positive, as it necessitates a detailed exploration to establish a constitutive relationship capable of inverting a broad spectrum of geological events. Yet, achieving such a relationship appears incomplete, demanding further studies that may involve more sophisticated tools, such as machine learning. Conversely, the authors could opt for a simpler model, if the analogue part exists, involving one or two faults with varying properties. Then, the authors would increase the complexity of the problem in subsequent chapters.

Before proceeding with publication, I advise considering the following points:

1. In your inversion model, it is noted that the heat transport equation is not solved. Could you elaborate on the potential impact of this factor on your results?

Additional content on this point was added to the manuscript in subsections 2.2.2 and 3.3. To sum it up here: In the analogue experiment which was studied, the temperature was considered constant during the experiment, so the viscosity of the materials would not have varied had the heat transport equation been solved.

2. On line 35, clarification is sought regarding why the aforementioned methods are particularly limited in capturing the dynamics of salt basins. Is it related to layering or other characteristics of salt basins?

The limitations of previous mechanical restoration methods to restore salt basins are simply due to the fact that elastic deformation alone (and the physics behind) cannot be used to compute the creeping flow of salt layers in time. As the paper was reworked to better put forward the main results, this specific comment in the introduction was removed because it seemed out of scope in this light.

3. Have you assessed whether the resolution of your model may significantly impact the results?

Indeed, the resolution of the computation grid has an impact on the results, and specifically on the behaviour of the faults, which is why specific care was put to having an adaptively refined grid. The amount of the impact itself was not studied, but could be an interesting perspective for future work.

4. Would you consider exploring periodic boundary conditions on the side walls? It is presumed that faults may restore as the model equilibrates.

Periodic boundary conditions were not considered for the analogue model studied here, given the experiment layout. On other models, however, they could indeed be considered. It could then be interesting to see how they affect the simulations and the effective material properties compared to the boundary conditions presented in the manuscript.

3 Answer to Patrice Rey

This paper is not ready. It needs major revisions before it is considered for publication in Solid Earth. Please carefully consider the reviewers comments, as well as the following.
The paper tackles an interesting subject. However, writing lacks rigor, e.g.:

- Line 70 "The idea is to choose materials that present the same deformations as those observed in geological models,. . . " You probably mean "similar rheology".
- Line 158: "These point swarms are denser than the material particle swarm and are one dimension lower (i.e. lines in our 2D cases)." One dimension lower? More simply: . . . the surface of our 2D numerical model is tracked using a set of passive tracers with an initial horizontal spacing of X m.
- Line 202: What do you mean by flattening? Do you mean removing any topography to make the surface horizontal? What does "... tying the fault lines ..." mean?
- Analog or analogue, choose one and be consistent.

The comments were taken into account in the manuscript, see the track-change file for precise changes.

The introduction section is largely borrowed from a previous paper from the same authors, please rewrite it.

The introduction was almost entirely re-written.

The analysis focuses on the comparison of the first increment of restoration with the last increment of extension. Yet, X-ray tomography at 2-minute intervals allows for a more thorough comparison between the restoration and the analog model.

Indeed, and this could be interesting in order to check the restoration in a step-by-step way. It was not done here for several reasons: first, the tomography image resolution did not permit a very accurate numerisation, so without enough deformation the difference between restoration error and numerisation errors would be hard to see. Second, the numerisation process, because of the lack of accuracy in the tomography images, took a long time. This more thorough comparison, however, could be interesting when introducing an inverse problem on the restoration process to find the effective material properties.

In Figure 6, panel h shows that the top surface of the basal viscous layer doesn't seem to return to its initial horizontal position as restoration proceeds. This suggests some serious limitations of the method. Have you considered that the viscous/viscous interface in the numerical model may not capture the physics of the granular/viscous interface in the analog model?

Indeed, Figure 6 showed that the restoration of the lower layers cannot be achieved properly with these boundary conditions and material properties. This is one of the reasons which lead to the following sections trying to assess their impact on the results, to improve the restoration process. The viscous/viscous interface could indeed be part of the problem, although the difference in viscosity should be able to mimic the granular/viscous interface. What could also be problematic is that the numerical solution and advection of the particles follow the grid discretization and not the interface itself, which may overly simplify the deformation at this interface.

The purpose of the paper was lost when it became a game of tweaking the viscosities, following a painful trial and error approach, to match the tempo of the analog model. Have you considered to what extent the tempo of a 3D model involving a basal viscous layer can be compared to that of 2D model?

What the analysis of the material properties showed, is that the main factor changing the results is the viscosity of the faults. The main difference between the 2D and 3D cases would then be, in my sense, the horizontal variation of the fault throws. A paragraph discussing this issue was addded in the discussion section of the manuscript.

The description of the reference analog model needs more details:

- What are the dimensions of the model in x, y and z?
- Also give the scaling and the dimensions in kilometer.
- Give an idea of velocity of the right end of the model.
- Were the walls lubricated to approach free slip?
- How was sedimentation implemented, continuous or in stage?
- How was the right boundary condition implemented?
- What was the final total amount of extension?

The analogue experiment was done outside the scope of the study, so all the informations on it was not available, but I added those requested and available in the manuscript.

Many claims are made that dynamic boundary conditions are more "natural", "less unphysical", or "more realistic". Fair enough, but can you explain why?

The dynamic conditions are considered more "natural" here in comparison to kinematic conditions because they do not impose a deformation on points where the deformation is not known, and try instead to build on known physical parameters or behaviour. This has been added in the manuscript.

The heat equation is not included. Given the dependence between viscosity and temperature, please justify this choice.

There is indeed a dependency between viscosity and temperature, particularly when doing simulations at very large scales (mantle, subduction,...). Here, the temperature during the laboratory experiment was considered to be mostly constant and stable, and as such have no impact on the experiment itself. Moreover, the scaling of the model aims to represent basin models, where the temperature should not have large variations at the studied time scales. To simplify the problem and be able to assess the impact of other parameters, the variation of temperature was then neglected. In future studies, it could still be interesting to see its impact on simulations. This discussion was added in the manuscript as well.

Add the dimension to all parameters in Table 1 and 4.

The units were added to these tables.

In the conclusion, it is stated that "... conclusive results can be obtained while changing the consideration of salt layers and faults to a more physical behavior, compared to previous geomechanical restoration schemes using elastic behavior". Yet no such a comparison is made in the paper. In the conclusion, it is stated that "... the creeping flow restoration of this analogue experiment model showed that this restoration scheme can be applied to complex real-case structural models, ... ". I do not think that this is demonstrated in this paper.

Indeed, these two points were not shown in the manuscript. I removed these statements and rewrote the conclusion to mirror the organization changes I did in the manuscript.

Kind regards, Patrice