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Interactive comment on "Towards the application of Stokes flow equations to structural restoration simulations" *by* Melchior Schuh-Senlis et al.

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"Towards the application of Stokes flow equations to structural restoration simulations" presents a novel approach to structural restoration based upon principles of Stokes flow and deformation of Newtonian viscous fluids. The manuscript is well written and organized. The authors clearly explain the new approach and its implementation, provide clear and sound justification for the scientific principles, and demonstrate its potential value with three simple synthetic examples. While the current implementation and demonstration is limited to 2D, the potential extension to 3D is made clear. The manuscript is clearly worthy of publication, but I would first provide several comments and recommendations.

First off, it is my opinion that the authors do not adequately address their assumption of a linear (Newtonian) viscosity model in sections 1 & 2. The authors explain at some length in the introduction the limitations of elastic geomechanical restoration techniques to capture inelastic (nonlinear) processes. They also provide references to justify the representation of rock deformation as viscous flow. However, there is only brief mention in the discussion (line 297) of their simplification to assume Newtonian fluids. At the least, this assumption, and that most of the preferred representations of rock deformation as viscous fluids assume non-Newtonian (e.g. power law) models, needs additional (and earlier) acknowledgement and discussion. The first two examples of the new restoration technique use forward models that also assume Newtonian fluids. These are insightful; however, ideally, I would like to see a restoration of a forward model that uses more realistic rheology for the forward model.

Second, I believe that the third example (Section 4.3) requires additional explanation and discussion.

1. It's not clear how the geometry was constructed. To what extent is this "image" an interpretation of real data vs. based on a model? How was it generated? The general reader should not have to read the reference to understand this model which is critical to this manuscript.

2. Also, why use a stochastically generated diapir rather than a previously published interpretation of a real structure? As a geologist, I would be more comfortable with an example that used a real subsurface structure than a stochastic model.

Further, the results of this section are very interesting, and probably warrant additional discussion.

1. It makes sense that the system tends toward a state that is in mechanical equilibrium (thus a flat salt-sediment interface). It would be nice to know that the restoration path is valid, too.

2. I'm having trouble understanding what are the geologic implications of the restored images in which synkinematic sediments are not removed. What is or is not representative of past state? What parts of the restored images should I focus on (and what should I not focus on)? There are significant differences between the models in the shallower section, but perhaps the authors do not discuss because they consider it geologically irrelevant.

3. It is important to note that the loading of shallower (younger) sediment is not removed and thus the stress state driving restoration in the past is incorrect.

4. A video (or several key frames) of the preferred restoration as it progresses back in time might add value in addition to showing only the final state of each.

The authors discuss the ability of this method to discuss faulted structures, and it seems the numerical implementation is ready. It would be nice to include an example.

The discussion of the numerical implementation (Section 3) is lengthy, and this detracts from the focus on structural restoration. Further, there are many prior implementations of Stokes flow using particle-in-cell methods. I recognize that the numerical implementation was much of the effort, but consider if it would be appropriate to condense this section and move the details to the Appendices (along with the validation examples). This could provide space in the manuscript for additional examples and discussion.

Finally, following are a few more technically specific comments. Figure 1: Verify that the velocity fields (B&C) correspond to this sketch (A). I think that these velocity fields represent a single wavelength perturbation of the material contrast in the horizontal dimension, but the sketch shows two wavelengths perturbation. In other words, for this sketch, there should be four convection cells, not two, and material should be flowing up at the side boundaries in the forward sense.

Paragraph beginning line 279: Use of the term "weld" in the sense of restoration is

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confusing. The diapir is restored, and sediment is juxtaposed against sediment where there was originally no salt. This is not a weld in the geologic sense. To avoid confusion, I would recommend finding an alternate description of this feature of restoration.

Paragraph beginning line 321: The authors provide two solutions to the rock-air (or -water) interface problem: sticky air or the free surface. They go on to explain the issues with a free surface in some detail, but do not offer further discussion of the sticky-air solution. If it is a viable solution, why not demonstrate it?

Reference to Medwedeff., et al.2016 (abstract) is now available in peer reviewed paper (Lovely, Jayr & Medwedeff, AAPG Bulletin, 2018)

Lines 65-67: I don't understand why large deformation and potential remeshing may limit the value for interpretation validation. Would remeshing not be OK, so long as key structural elements (e.g. faults and horizons) are preserved?

Line 111: Should a reference be provided for CFL condition?

Lines 136-139: Another reason not to solve the thermal equations is that diffusion may be important at geologic time scales, and it is not reversible.

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