Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-89-SC3, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

Solid Earth Discussions

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

Alik Ismail-Zadeh

alik.ismail-zadeh@kit.edu

Received and published: 3 July 2020

I have read the manuscript submitted to the journal with an interest refreshing my memory of research done some 20+ years ago and to see what new is done in this area. Unfortunately, I did not find any novelty of the work, except an implementation of different numerical solution approach to compute a viscous flow. I respect the work done, and believe that the authors did not know about the work previously done by others (my colleague Boris Kaus mentioned a number of earlier works on this topic). Meanwhile without assessment of the previous work versus of the work presented in this manuscript, the value of the paper will be little, if any. More detailed comments follow.

1. The authors should discuss the novelty point of the work. The statements as "We have therefore developed a new approach for restoration based on considering ... (in the abstract)", "we investigate a new method to address these challenges ... (in the Introduction)", "We have presented a new scheme that exploits ... (in the conclusion)", and elsewhere in the manuscript must be deleted as the approach is NOT new.

The idea of the reconstruction method was presented at the EGS General Assembly in Hague in 1999 and published in 2000 in the Russian book series "Computational Seismology and Geodynamics" and later peer-reviewed and published by AGU https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/9781118669853.ch4

2. Careful reading of the manuscript has revealed that the only novelty is the use of different numerical method to calculate the viscous flow (the use of FAIStokes code based on ALE implementation). The authors can highlight this, but not the novel idea of restoration.

Please note that you refer wrongly (line 151) to the technique, which was used in Ismail-Zadeh et al. (2004): the Eulerian FEM was used with tricubic splines to approximate basis functions (see Appendices A-E in Ismail-Zadeh et al., 2004;).

Also, note (lines 147-148) that the phrase "... neither of them is specifically adapted in the case of large displacements over time" needs clarification. Please see Naimark and Ismail-Zadeh (GJI, 1995) and Naimark, Ismail-Zadeh and Jacoby (GJI, 1998) for the techniques on how to track the interface between the layers and to get a higher accuracy at larger displacements.

3. The Stokes equations.

- Lines 79-80. "(isothermal)" should be deleted as unrelated to the Stokes equations. Also, the Stokes equations are not reversible (see next comments) but the Navier-Stokes equations, which are not used here.

- Lines 89-103. Your creeping flow equations do not depend on time and describe a

steady-state flow. You cannot reverse time in Eqs. (1)-(6) as time does not exist in the equations. To avoid the problem you need to advect density and viscosity with the flow using d\rho/dt=0 and d\eta/dt (d is the full derivative). The fact that FAIStokes treats with the issue using a particle swarm is not enough to ignore the discussion of why the steady-state equations (1)-(6) allow for a non-stationary flow. You must clarify this.

- Lines 105-119. Suddenly you introduce time using the Euler scheme, which is proven to be computationally unstable, and the use of higher-order methods should be encouraged unless your time step is small enough (see e.g., Ismail-Zadeh and Tackley, 2010. Computational Methods for Geodynamics, Cambridge Univ. Press). BTW, later it is mentioned that the second-order RK-method is used (better the 4th-order methods to see a difference in computations).

4. Unstable simulations (lines 127-9). Instabilities in this case reflect inaccuracy of numerical methods used and are likely to be related to overshoots and undershoots of the viscosity, which lead to computation of erroneous velocities controlling the advection of interfaces between layers (including a free surface, where the viscosity ratio is significant). For more detail, please see Naimark, Ismail-Zadeh and Jacoby (1998) - fig. 3 and description in section "Efficiency of the method" (http://www.mitp.ru/∼aismail/papers/GJI1998.pdf)

5. All models of salt diapirism considered in the manuscript are related to upbuilding (when a lower density salt penetrated into already formed overburden due to RT instability). Actually, in salt tectonics upbuilding is a rare process, and essentially salt structures are formed due to downbuilding (when salt starts to move due to different loading of sedimentary overburden). See examples of upbuilt and downbuit diapir's dynamic restorations in Ismail-Zadeh et al. (2001; http://www.mitp.ru/∼aismail/papers/restore.pdf). This should be discussed in the paper and especially in the light of how the method used can treat the case of downbuilding. A model of downbuilt diapir dynamic restoration using the FAIStokes will enhance the paper.

Hope the comments would help in revising the manuscript.

Alik Ismail-Zadeh

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-89, 2020.