Response to referees

Ian Rose, Bruce Buffett, and Timo Heister

We thank referees Fabio Crameri and Dave May for their thoughtful and constructive reviews, which are appended to this document for convenience.

Response to reviewer 1

Fabio Crameri provided a number of helpful comments and suggestions for clarification, which we summarize here:

- We have identified **I** as the identity tensor.
- We have itentified **n** as the unit normal.
- We have clarified lines 154-155, 174 and 460-463.
- We have improved the caption for Figure 5.

Response to reviewer 2

Dave May gave a very thorough review with substantial suggestions for improvements and additions to the manuscript, which we address in detail here.

Summary

May's review has two main criticisms:

1. That we do not provide sufficient non-trivial examples of the usage of our new time integratior, especially making detailed comparisons with the existing quasi implicit (QI) time integrator of Kaus et. al. (2010), including a discussion of the relative advantages and disadvantages of the scheme.
2. That we have not provided enough information of how to determine the stabilization timescale (τ^*) which is required to use the nonstandard finite difference (NSFD) time integrator, including a discussion of the consequences of making a poor choice for τ^*.

In order to address these we have made two major additions to the manuscript:

First, we have added a more lengthy discussion of how to numerically determine the minimum relaxation time of a given geodynamic simulation, which is the best choice for τ^*. Our favored method is using power iteration, which finds the dominant eigenvector/eigenvalue pair of the system. Each iteration involves a solution of a Stokes system, and we find good approximation of the minimum relaxation time in 5-20 iterations, depending on how good the initial guess is.

This estimate of the minimum relaxation time is not only useful for the NSFD scheme, as it can also be used to determine appropriate timesteps for quasi-implicit time integration, or even forward Euler time integration. We provide several numerical examples of using power iteration, including to confirm our analysis of the effect of the quasi-implicit stabilization term on the spectrum of the system (Equation 29). The results of this test are shown in Figure 2.

Our second major addition is an investigation of the Rayleigh-Taylor instability with a free surface which was considered in Kaus et. al. (2010). We found that the QI and NSFD schemes are comparably accurate at the same adaptive timestepping (with a slight advantage to QI). However, we have found that the NSFD scheme is able to take larger stable timesteps (though at the cost of lower accuracy).

We have also used the Rayleigh-Taylor benchmark to investigate the effect of different choices of the stabilization parameter τ^*. We considered four cases:

1. recomputing the minimum relaxation timescale every 50 timesteps and setting τ^* to that value (this allows for changes in the viscosity and density structure with time). This is used as the reference case.

2. Using the initial value of the minimum relaxation time for the whole simulation.

3. Using a value that is about a factor of two too large.

4. Using a value that is about a facator of two too small.

The first two cases are almost identical, while the third and fourth have significant differences with the reference case.

We also have specific responses to the "General Comments" and "Detailed Corrections" sections May's review, detailed below.

General comments

1. The manuscript is no numbered.

2. We have made equation referencing consistent.

3. Equations are now punctuated.

4. Section headings now all use sentence case.

5. "timestep" is now used uniformly.

6. The point about the title is well-taken, though we would argue that our analysis of the stability of the system and the effect of QI stabilization on the spectrum are at least as important as our new time integrator. We have retitled the manuscript "Stability and accuracy of free surface time integration in viscous flows."

7. We have added a detailed look at the time dependence of τ^*. In many cases its time variation is probably not large enough to merit recomputing it during the course of a simulation.

8. We have provided new numerical experiments investigating the numerical determination of τ^*, and have taken a detailed look at a Rayleigh-Taylor benchmark. In the Rayleigh-Taylor case we have QI and NSFD schemes, investigated the time dependence of τ_{min}, and demonstrated the effects of a poor choice for τ_{min}. We ran experiments for the Schmeling et. al. (2008) benchmark, but have ultimately not included them because (1) they did not provide new insight over the Rayleigh-Taylor benchmark, and (2) this particular benchmark has not proved to be particularly useful due to its extremely sensitive dependence on model parameters and viscosity averaging schemes, making the results difficult to interpret.

9. We have added a treatment of using power iteration to calculate the minimum relaxation time, which is useful not just for determining τ^*, but can also be of use for analysis of timestepping with other schemes.

10. We have added some discussion of direct comparision of the QI and NSFD schemes. We agree that the benefits of spatial adapdivity are less connected with the preceding discussion on timestepping, however we still felt that demonstrating the savings of combining AMR with a free surface had some value.

Detailed corrections

1. We have reworded the abstract to emphasize time integration of the free surface.

2. We have cited Jarvis and Peltier (1982) for an early dynamic topography calculation.

3. We have given an additional reference for the sticky air method.

4. The surface of Earth is not truly stress free, since there are stresses due to oceans and atmopheres. We would argue that "more closely matches" is appropriate.

5. We have cited Popov and Sobolev (2008) for another implicit approach to free surfaces.

6. We have explicitly given the conditions for free-slip, free surface, and Dirichlet boundary conditions.

7. We agree that discussion of the discrete mesh is more appropriate in Section 7, and have reorganized accordingly.

8. This is a good suggestion. We have reorganized Section 3 to put all the information dealing with the time evolution of $\Omega(t)$ in one place.

9. We have introduced a new notation $\hat{\Omega}$ for when we are dealing with a spatially discretized domain. However, for most of the analysis of Sections 2-5 the equations are agnostic to whether we are using a discretized or continuous domain, so we have retained the continuous notation when appropriate. We have also tried to clarify what is meant by the prediction of the shape at a later time $\Delta\Omega$ as an approximate integration of the domain evolution equation (Eq. 7).

10. We have changed line 178 to "become unstable".

11. We have clarified the approximations under which the QI stabilization term is identical to the M matrix in the homogeneous problem. Specifically, they are approximately the same when the slope is small, and density variations are small compared to the reference value.

12. We have added a note to this effect.

13. It is true that our ALE formulation involves assembly of a surface mass matrix, as does the solution of the generalized eigenvalue problem. However, not every implementation need have the same approach. That being said, it is a good point that the modifications to the Stokes system matrix are overall not very difficult to apply.

14. We have rephrased this point.

15. x and y axes are now italicized.

16. We have clarified this point.

17. This has been fixed.

18. We mean Equation (16), which can be reorganized to Equation (18), so either are appropriate here.

19. We have clarified this point in the text, as well as explored by how much $\tau_{\min}$ might change.

20. Equation (19) involves the inverse of τ_i, so finding the largest eigenvalue of this system is equivalent to finding the minimum relaxation time.

21. Boundary conditions are now all defined mathematically.

22. Absolutely right, this is now rephrased.

23. This is now clarified to an "L_2 projection of $u \cdot n$ onto Γ_F".

24. Changed to say "cheap".

25. Line 267 now says "acceleration due to gravity".

26. L2 $\rightarrow$ L_2.

27. We now use the symbol E for the error.

28. Line 472-473 has been rephrased.

29. We now give more information about the parameters used to run this simulation, including refinement schemes, maximum spatial resolution, and timestepping schemes.

30. It is true that 500 years is a very small timestep, and that forward Euler timestepping would be stable for this. We use this timestep because Crameri et. al. (2012) found that a timestep of several hundred years was needed for the solution to fully converge. The purpose of this test is less to investigate the time integration schemes than to demonstrate the effectiveness of combining adaptive mesh refinement with a free surface treatment. To a certain extent we agree that this is not the primary point of the manuscript. However, we still think it is valuable to demonstrate that our implementation, which is open source and freely available, has this capability.

31. QI is now defined.

32. The caption now reads "significantly fewer unknowns".