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The Cryosphere Discussions

Interactive comment on "Reformulating the full-Stokes ice sheet model for a more efficient computational solution" *by* J. K. Dukowicz

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This paper investigates an integro-differential formulation for the non-Newtonian Stokes problem appearing in glaciology. The approach uses an integral in the vertical direction to eliminate vertical velocity and pressure in exchange for dense coupling of unknowns within columns. The author asserts that this formulation is more computationally efficient because it produces a uniformly elliptic operator, resulting in a symmetric positive definite matrix. Although the title of the manuscript indicates that it addresses computational efficiency, there is no computation, efficiency, or even discretization in this paper. The paper contains several related unsubstantiated claims. This is not acceptable, so if the present scope is to remain, the title and exposition would need to change to reflect what is actually shown rather than what the author believes may eventually

work out. However, I think the contributions in the present scope are likely to be of limited value without demonstrating that they are relevant to computation, so I would encourage the author to address these issues.

My primary concerns were raised in my comment TCD-5-C781-2011 and the author replied in TCD-5-C882-2011. Rather than reproduce that dialogue in full, I only include a brief summary and reply again.

1 Stable approximation spaces

In the original manuscript, the author states that the inf-sup condition makes discretizations for the velocity-pressure form of the Stokes problem more technical. While this is true, the present method only moves this concern to a different place (compatibility of an integration method). I stated this in my first comment and the author responded by suggesting that a Galerkin method could be used to discretize the variational form. This is natural, but does not answer my question. The standard Stokes formulation is often solved using Galerkin methods (corresponding to energy minimization over the space of divergence-free fields), but applying a Galerkin discretization to the author's reformulation only moves the inf-sup concerns to a different place. If the author intends to say anything about discretization (which I think is necessary), he should state the integral form of the inf-sup condition and propose at least one stable approximation space. (It is not difficult to state and prove these stability conditions, but they are an important technical requirement that I think are no less awkward in practice than the conventional inf-sup and Korn's inequality needed for the standard form of the Stokes problem.)

2 Discrete conservation

I raised concerns about what discrete conservation statements are available. The author's reply named Galerkin methods and the action, but did not provide any specific statements about conservation. Not all stable discretizations for the velocity-pressure form of the Stokes problem possess any local conservation properties. This is especially concerning in the presence of reentrant corners because the exact solutions of the continuum equations have singularities in these regions and some methods commit systematic mass loss or gain near those corners. Among inf-sup stable Galerkin discretizations, a common choice is to make pressure discontinuous between elements, in which case the divergence of the velocity field integrated over any element is zero (at least up to iterative solver tolerances). These choices have analogues for the integro-differential form. Again, it is not difficult or deep, but it's important for practitioners to have these issues documented.

3 Conditioning

I expressed concerns about conditioning; the author responds by stating that there are inexpensive models that could be used as "physically-based preconditioners". Although this idea of using low-fidelity physical models to precondition higher-fidelity models is attractive, it has not been demonstrated to work for similar problems and there is an outstanding mathematical issue that should be addressed before this claim can be substantiated. The mathematical problem is that the lower-fidelity methods do not have any natural way to act on the whole residual, they only act on a subspace (and are identity on the co-space). For stiff waves in hyperbolic systems (the birthplace of "physics-based preconditioning"), the preconditioners also only act on a subspace, but the subspace rigorously contains the long-range interactions. For these transient problems, the preconditioner should act on all "stiff" parts of the system; the remaining

non-stiff terms have Green's functions that decay exponentially, causing the iterative solver to converge rapidly without a preconditioner acting on the co-space. For elliptic problems such as Stokes (reformulated or not), the operator is still elliptic in the co-space, so the Green's function decays only algebraically (instead of exponentially). Unless this can be overcome, the resulting method cannot improve asymptotics over unpreconditioned (or locally preconditioned) methods, it can only improve constants.

The author asserts that the action is positive definite, but it would be useful to actually prove coercivity because the proof would provide some clarity about what geometric factors affect the appropriateness of this formulation (perhaps through conditioning of the discrete system).

4 Solution methods

I explained some challenges in solving the discrete equations resulting from this formulation. The author replied by stating that the "Jacobian-free Newton-Krylov" method (JFNK) could be used. The JFNK approach is quite general and could be used here, but it is not a silver bullet and requires careful thought for effective preconditioning (as with any methodology). My original comment and also the discussion in the last paragraph explains some of the technical issues that will need to be addressed to build a scalable JFNK method. I pointed out that you can numerically transform between the conventional and integro-differential form of the Stokes problem, such that an effective solver for either one implies an effective solver for the other. Elaborating on that topic, you can also work with the standard Stokes form, but add a preconditioner that only corrects vertical velocity and pressure by performing an integral. This preconditioned system is identical to the one produced by the reformulation (thus symmetric positive definite, etc) except that it is augmented by identity to act on a larger space. Since the identity part is inconsequential to Krylov methods, anyone with a standard Stokes formulation and a compatible integration rule can choose for their Krylov iteration to be carried out in the same space as with the reformulated alternative.

5 Implementation

I raised concerns that the manuscript has no implementation, yet claims that the method offers computational efficiency. The author replied that an implementation was beyond the scope of the paper and that the main contribution was a formulation based on a minimization principle. I still think that an implementation is necessary, especially given the current title, and would also like to point out that all common forms of the Stokes problem have a minimization principle. One can choose to work with a differential form in which minimization occurs over a somewhat awkward set or a somewhat awkward integro-differential form in which minimization occurs over a nice set. The author should make a convincing argument for why the present form should be preferred to the standard form, but this involves several steps (discussed in my earlier comment and this review) and preferably concludes with an implementation (especially given the title). Note that it is possible to prove efficiency without implementing, e.g. local Fourier analysis for multigrid methods (a method known to deliver very sharp bounds on convergence rates), but doing so requires more rigor than is easily accessible in the present context and I suspect it would be significantly more work than an implementation of the algorithm.

Interactive comment on The Cryosphere Discuss., 5, 1749, 2011.