Review of: Albany/FELIX: a parallel, scalable and robust, finite element, first-order Stokes approximation ice sheet solver built for advanced analysis

December 20, 2014

1 General comments

The paper provides an outline of a new – apparently modular built – ice-sheet code deploying the so-called Blatter/Pattyn or first order (FO) approximation to the Stokes equations. As many other new generation ice-sheet codes, the linear algebra part is left to state-of-the-art HPC suites for solution of sparse matrices, in this case, Trilinos. Also the Albany code-base seems to be a well established platform providing a good interoperability with external libraries and built-in functionality for data assimilation. This appears to be a good example of high level computational science being deployed in the field of applied sciences - in this case numerical Glaciology. FO models themselves are not a novelty, neither are inverse methods/data assimilation nor high performance computing in the field. Nevertheless, Albany/FELIX combines those aspects in a new way. In that sense, this article is well suited for publication in GMD.

My general impression of the article is, that it is well structured and consistent. The language – for me as a non-native speaker – is without errors. I have one or two scientific concerns (or perhaps issues that demand deeper explanations), which I will discuss in detail in the following. If these points are addressed, I would recommend publication.

2 Main points of criticism

My main concern is about the **manufactured solutions** you present for testing a 2-D FO model. Your manufactured solutions, which you claim to be tailored for testing FO codes, apply on two-dimensions, so I would have expected to see flow-line problems. The essential feature of Blatter/Pattyn (a.k.a. FO) is the hydrostatic assumption (Greve and Blatter, 2009). This eliminates the pressure out of the equations and – as you correctly state – releases you from the pain to solve a saddle point problem. It also further eliminates the third velocity component and the longitudinal vertical stresses out of the system, thus rendering the problem to be 2-D-ish. Nevertheless, there would be still vertical, z-derivatives due to the shear stress components in the equation system. And that exactly raises suspicion from my side that you are not really testing the 2-D FO problem with your setup (as you claim to do). From your equation (22) I conclude that you basically reduce your 3-D problem (2) to the horizontal plane, i.e., setting all vertical derivatives (i.e., the ones in z-direction) that still would exist in $\dot{\epsilon}_{1,2}$ to zero. You actually never explicitly define the two-dimensional effective shear rate, $\dot{\epsilon}_{e,2-D}$, but from the term in brackets in equation (23) I conclude that it is the original 3-D version stripped of all z-dependencies, which would underline my suspicion. To cut a long story short: your 2-D problem setup does not reflect a flow-line FO problem (which from your words I would expect) but rather a single layer horizontal solution with no vertical shear, which reminds me rather to a Shallow Shelf (SSA) and not a FO problem. But perhaps it is your intention to proceed like that and I missed the point. What I would ask to get from you is a good argument that these cases are still a proper verification of the FO code in 2-D, as you claim in the text, or else some additional paragraph that sheds light on why you construct your solutions like that.

My second point is about that you actually never provide information on whether Albany/FELIX is capable of doing **prognostic runs**, which I think is essential for an ice-sheet model, in particular if one wants – as you claim – couple it to Earth System Models (ESM). That, in consequence, would include a discussion of the thickness evolution equation, which can be numerically quite tricky. Further it would impose the difficulty of dealing with in time changing meshes. If you could shed some light on the prognostic capabilities of your platform, this would be valuable information for the reader.

Finally, if I correctly recall the recommendations of GMD – and also in view of perhaps your own interest that the code rises attention in the wider community – you should include **information on the license(s)** the code and its components are published under and - if so - how it is accessible for other scientists. This might be clear for the separate components (like Trilinos), but even if parts of codes are open source, there can be combination of licenses (even open source flavours, like GPL, MIT, BSD) that might impose issues. And gathering this information in this paper is an asset.

3 Technicalities (sorted by their occurrence)

line numbers refer to the printer-friendly version of the text downloaded from `http://www.geosci-model-dev-discuss.net/7/8079/2014/gmdd-7-8079-2014-print.pdf`

> page 8080, line 15: "…discretized using structured and unstructured meshes."
> The wording *structured* might be misleading, as in FD/FV community

this is synonymous to i,j,k indexed points/cells. FEM inherently is unstructured by its method (or are you taking advantage of the structure in your assembly of matrices?). I would – and this occurs a few times in the text – rather use the terminus *layered* mesh.

page page 8081, line 7: opinions on what model is part of the "new generation" club are somewhat subjective and naturally have a tendency to not include those models from scientists that are in lesser proximity to oneself. But for instance (and there might be even other examples) SICOPOLIS, despite being around for some while, has been used as an adjoint model (Heimbach and Bugnion, 2009) as well as been coupled to climate models (Vizcaíno et al., 2009) and – similar to PISM – also has a hybrid approach between SIA and SSA (Sato and Greve, 2012) and has a community around it (it is open source with multiple contributors).

page 8081, line 14: You might include the early work of Pattyn (2008) in this list.

page page 8081, line 10: the abbreviation HPC (High Performance Computing) is not explicitly given - minor thing, but readers not familiar with it might wonder.

page 8082, line 1: Gille-Chaulet → Gillet-Chaulet

page 8082, line 1: missing references: (Jay-Allemand et al., 2011) and (Favier et al., 2014) (see in References)

page 8082, line 16: minor technicality - "HPC computing platforms" would read as "High Performance Computing computing platforms"; so, perhaps change to "HPC platforms"

page 8084, line 17: "...and the assumption that the normal vectors to the ice sheet's upper and lower surfaces, $\boldsymbol{n} \in \boldsymbol{R}^3$, are nearly vertical:"
I would say that this is not an additional assumption but rather a consequence of your initial shallowness assumption, as the gradients of your surfaces scale with $\mathcal{O}(\delta)$, as you correctly state in equation (1) in combination with the fact that $\boldsymbol{n}$ occurs in terms of $\mathcal{O}(\delta)$ or larger.

page 8085, line 5-14: I would suggest to introduce the strain-rate tensor by name.

page 8086, line 5 and 7: It is rather the temperature relative to pressure melting point than the normal ice temperature that enters the Arrhenius factor, as you describe it. In ice sheets this is not negligible, as we are talking of a shift of about 0.87 K per kilometre ice thickness.

page 8087, equation (16) and (17): Why are there curly brackets in front of these equations?

page 8087, equation (17): if your z-coordinate is negative for values below sea level ($z = 0$), then the right-hand side should rather read as: $\rho_{\mathrm{w}}\, g \min(z, 0)\boldsymbol{n}$. If it needs a positive or negative sign depends on the orientation of your surface.

page 8089, line 11: "Note that in our weak formulation Eq. (19), the source terms in Eq. (2) have not been integrated by parts." I do not get the point of this statement, as I would not see how the divergence theorem would apply to single directional derivatives, like $\partial s/\partial x$.

page 8090, line 15: "..., then splitting each prism into three tetrahedra (Fig. 17)." Out of curiosity: why do you not use wedge-type prisms but rather split them and by this give away the possibility to keep a low aspect ratio of the element?

page 8092, line 9: You let the continuation parameter, α, pop up in the middle of this sentence, but the only place where it occurs else is the table showing the algorithm. This is a little bit confusing.

page 8093: missing reference in the discussion of multi-grid methods in ice-sheet modeling: (Jouvet and Gräser, 2013)

page 8098, line 4, equation (23): As mentioned in my main points of critics, I do not think that the expression in brackets represents the 2-D version of the effective strain-rate for a FO problem.

page 8100, line 2: I have issues understanding how there can be an x in the $3\pi\, x$ term which should be a derivative of (25) – please verify.

page 8116, line 7: "In general, glaciers and ice sheets are modeled as an incompressible fluid in a low Reynolds number flow with a power-law viscous rheology, as described by the Stokes flow equations." In general you are right. Nevertheless (also in order to end up with an expansion of the equations with respect to the aspect ratio), in the context of creeping shallow flows one introduces scales for the typical stresses not in terms of a viscosity, but rather scaling with the hydrostatic pressure. Consequently, the resulting equations in ice sheet flow usually are presented as the double limit of a small aspect ration and Froude numbers (see Greve and Blatter, 2009).

page 8138, Fig. 8: I would say that the figure in that form is not very informative and as well can be skipped.

page 8139, Fig. 9: Out of curiosity: Can you explain me, why there is a small, yet asymmetric deviation between two solvers solving a symmetric problem (mesh, partitioning, algorithm).

page 8143, Fig. 13: I would skip that figure. The explanation in the text suffices.

page 8145, Fig. 15: Out of curiosity: Can you perhaps explain why the "full Newton" method does not diverge immediately (as I am used to see from my applications with full Stokes solutions), but starts to go out of the window only after a few iterations?

page 8148, Fig 18: I think it would be more informative to include the error between observation and the (I guess based on inversed basal friction coefficients) computed solution and perhaps explain the deviations (which there seem to be in the fast flowing areas).

References

Greve, R., and Blatter, H., *Dynamics of Ice Sheets and Glaciers*, Advances in Geophysical and Environmental Mechanics and Mathematics, Springer, 2009

Heimbach, P. and V. Bugnion., *Greenland ice-sheet volume sensitivity to basal, surface and initial conditions derived from an adjoint model*, Ann. Glaciol. 50 (52), 67-80, 2009

Vizcaíno, M., U. Mikolajewicz, M. Gröger, E. Maier-Reimer, G. Schurgers and A. M. E. Winguth, *Long-term ice sheet-climate interactions under anthropogenic greenhouse forcing simulated with a complex Earth System Model*, Clim. Dyn. 31 (6), 665-690, 2008

Sato, T. and R. Greve, *Sensitivity experiments for the Antarctic ice sheet with varied sub-ice-shelf melting rates*, Ann. Glaciol. 53 (60), 221-228, 2012

Pattyn, F.: Investigating the stability of subglacial lakes with a full Stokes ice-sheet model, J. Glaciol., 54, 353 - 361, doi:10.3189/002214308784886171, 2008

Jay-Allemand M., F. Gillet-Chaulet, O. Gagliardini and M. Nodet, *Investigating changes in basal conditions of Variegated Glacier prior to and during its 1982–1983 surge*, The Cryosphere, 5, p. 659-672, 2011, doi:10.5194/tc-5-659-2011

Favier, L., G. Durand, S. L. Cornford, G. H. Gudmundsson, O. Gagliardini, F. Gillet-Chaulet, T. Zwinger, A. J. Payne and A. M. Le Brocq, *Retreat of Pine Island Glacier controlled by marine ice-sheet instability*, Nature Climate Change, 2014, doi:10.1038/nclimate2094

Jouvet, G., C. Gräser, *An adaptive Newton multigrid method for a model of marine ice sheets*, Journal of Computational Physics, Vol. 252, 2013