Response to Reviewer #1 (T. Zwinger)

March 13, 2015

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.

We thank the reviewer for his thoughtful feedback, which has been addressed in the revised manuscript, and has helped us to improve our paper. All changes to the original manuscript are marked in blue in the revision. You will find also our responses to your comments below, in blue.

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 1,2 to zero. You actually never explicitly define the two-dimensional effective shear rate, $\dot{\epsilon}_{2-D}$, 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.

The reviewer is correct in how it is defined. We have made the notation more clear in the revision.

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.

The reviewer is correct that our MMS solutions were not to the 3D FO Stokes equations and lacked the vertical shear term. The equations were obtained by neglecting the $\partial/\partial z$ terms from the FO Stokes equations. The test cases were not intended to verify the 3D FO Stokes equations: rather, they were intended to be used as part of a multi-stage code verification that includes also verification of the full 3D FO Stokes equations using code-to-code comparisons and mesh convergence studies on realistic geometries (Sections 5 and 6 in the paper). We have attempted to make this more clear

in the paper, and also have made it clear that our MMS problems are for a 2D version of the FO Stokes equations, not the full 3D equations.

We feel despite being simplified, our MMS test cases are nonetheless useful. The task of deriving source terms for an MMS study for the 3D FO Stokes equations is cumbersome, if not intractable. In contrast, our MMS problems are simple enough to be implemented by anyone simply by referring to the expressions in our paper.

We do agree with the reviewer that a test case based on equations which include vertical shear would be worthwhile. To address this point, we have derived an MMS test case for the FO Stokes equations in the x–z plane (obtained by neglecting the v and $\partial/\partial y$ terms in the equations), and showed some mesh convergence results for our code on this test case. Please see Section 4.2 of the revised manuscript.

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, to 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.

How we plan to couple to ESMs and do prognostic solves is a good question. The reviewer is correct that *Albany/FELIX* can only be used for the diagnostic stress-velocity solve, and does not discretize the ice temperature and thickness evolution equations. To do prognostic runs and couple our code to ESMs, we have interfaced *Albany/FELIX* to two other land ice codes: the *CISM* (Community Ice Sheet Model) and *MPAS-LI* (Model for Prediction Across Scales - Land Ice) codes. In the resulting dynamic cores (dycores), termed *CISM-Albany* and *MPAS-Albany* respectively, the steady state stress velocity solve occurs in *Albany/FELIX* and the ice sheet evolution (including updating the ice extent, if the ice has advanced or retreated) is calculated in *CISM* or *MPAS*. The details of these dycores will be presented in a subsequent paper, so we had intentionally omitted the discussion of *CISM-Albany* and *MPAS-Albany* from this paper. We agree with the reviewer that some mention of these dycores is worthwhile so the reader can see how we will do prognostic solves and couple to ESMs. It has been added to the "Conclusions" section of the paper. If the reviewer would like to learn more about *CISM-Albany* and *MPAS-Albany* and see some results from prognostic runs

produced by the code, there are some presentations on the subject available at: `http://www.scidac.gov/PISCEES/presentations.html`.

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.

We have added a "Code Availability" section to the paper that describes the availability of the codes. Both *Trilinos* and *Albany* have BSD-type licenses and can be cloned from their `git` repositories by anyone interested in looking at the code. The *Albany/FELIX* solver described in this paper is not publicly available at the present time. A public release of the code as part of *Albany* is planned for 2015.

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`.

1. 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.

   The reviewer is correct that the FEM is inherently unstructured. No, we are not taking advantage of the structure in the matrix assembly. We do take some advantage of structure in the z–direction, e.g., in creating the decomposition for a parallel run (described in Section 6.1). We have replaced "unstructured and structured" with "tetrahedral and hexahedral" where this terminology appeared in the paper to prevent confusion. We did not change to the term "layered mesh" as all the meshes we consider (both structured and unstructured) are structured (layered) in the z–direction.

2. page 8081, line 7: opinions on what model is part of the "new genera-tion" 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 (Vizcano 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).

   We thank the reviewer for pointing us to these additional references. We have added some of these to the bibliography, but not all. Our list of references was not meant to be exhaustive or all inclusive, but merely representative. In an attempt to clarify this, we have added "e.g." at the beginning of the list of references.

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

   We have added this reference.

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

   We agree that the abbreviation HPC should be defined prior to its first use. This has been done in the revision of the paper.

5. page 8082, line 1: Gille-Chaulet $\rightarrow$ Gillet-Chaulet

   This was a typographical error, which we have remedied.

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

   As we stated in our response to 2., our list of references was meant to be representative, rather than all inclusive. To clarify this, we have added "e.g." at the beginning of the list of references. We feel our list of references is sufficiently representative.

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

8. page 8084, line 17: ". . . and the assumption that the normal vectors to the ice sheets upper and lower surfaces, $\mathbf{n} \in \mathbb{R}^3$, 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 $\mathbf{n}$ occurs in terms of $\mathcal{O}(\delta)$ or larger.

   If a *shallow* domain is defined as a domain obtained by a δ-scaling of the z–coordinates of a "regular" domain, then we agree with the referee that the gradient of its top/lower surface will scale with $O(\delta)$. However, in the paper we are simply considering a domain with small aspect ratio H/L, i.e. a thin domain, that in principle can have very rough surfaces' topographies with large slope gradients, so an additional assumption on the surfaces' slopes is needed. We refer to Dukowicz et al, *J. of Glaciology 2010*, where it is stated that FO model "is valid only when both the low-aspect-ratio and small-basal-slope assumptions are valid". Since an ice-sheet lower surface can be pretty rough we prefer to point out the assumption on $\mathbf{n}$ as it may not be always satisfied.

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

   We have added the definition of the strain-rate tensor.

10. 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.

    We agree and have added this detail to the text.

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

    We agree with the reviewer that the curly brackets were unnecessary. They have been removed.

12. 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:

$\rho_w g \min(z, 0)\mathbf{n}$. If it needs a positive or negative sign depends on the orientation of your surface.

Thank you for pointing out this typographical error. We have fixed it in the revised manuscript.

13. 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 $\partial s/\partial x$.

In principle, one could replace $\int_\Omega \rho g \frac{\partial s}{\partial x} \phi_1 d\Omega$ with

$$- \int_\Omega \rho g s \frac{\partial \phi_1}{\partial x} d\Omega + \int_{\partial\Omega} s\phi_1 \mathbf{n}_1,$$

using (a corollary of) divergence theorem. However, we agree with the referee that the sentence is misleading and not needed, therefore we removed it.

14. 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?

Mostly for historical reasons. We wanted to compare our results with another code (*LifeV*) that uses tetrahedra, and so we used the same elements/meshes. We are now exploring the possibility of using prisms, but we did not try yet with realistic geometries. As a side comment, we remark that a nice consequence of using linear FE on tetrahedra is that a one-point quadrature rule is enough to get exact integrals for the diffusive term (if the flow factor is constant on each element). We added a sentence in the paper mentioning the possibility of using wedge elements on prisms.

15. 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.

To prevent confusion, we have added the phrase "(defined in Algorithm 1)" following the first appearance of α in the text.

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

We have added this reference.

17. 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.

Our x–y 2D FO Stokes equations are intended to be the mathematical version of the 3D FO Stokes equations obtained by neglecting the $\partial/\partial y$ terms in the full equations. We agree that the physical interpretation of the terms becomes blurred with this definition. We have removed references to $\dot{\epsilon}_{e,2D}$ as the effective strain rate.

18. 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.

The reviewer is correct: there was a typographical error in equation (25). The $3\pi x$ term should be 3π. The error has been corrected in the revised manuscript. We thank the reviewer for calling the mistake to our attention.

19. 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).

While we appreciate this insight from the reviewer, we've decided to leave this level of detail out of the current paper, which is why we currently refer to Dukowicz et al. (2010) and Schoof and Hindmarsh (2010) for additional discussion on how the first-order approximation is derived from the Stokes equations. If the editor thinks it necessary and appropriate, we would be happy to add an additional reference to Greve and Blatter (2009).

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

   We agree, and have removed the figure from the paper.

21. 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).

   The error is on the order of machine precision so truncation errors are probably the leading errors here. Part of the asymmetry could be generated by the fact that the algorithm used to split prisms into tetrahedra does not preserve symmetry. We realize that this figure may be misleading, therefore we prefer to remove it and just mention that with the two codes we get the same result up to machine precision.

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

   We agree, and have removed the figure from the paper.

23. 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?

   Our answer is speculative, and should not be added to the publication. Our intuition is that the full Newton algorithm sees a nearly-infinite viscosity, which leads to no practical progress in the nonlinear solver, but also does not lead to divergence. In a full Stokes code, the deviatoric pressure is probably what diverges, trying to compensate for stresses weighed by the nearly-infinite viscosity, while our hydrostatic formulation does not have this term.

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

   The deviations are likely due to the fact that in the optimization we also try to be in equilibrium with a given surface mass balance, which can be in competition with matching the surface velocity observations.

The inversion details and related discussion are in the referenced JGR paper by Perego, Price and Stadler. The focus of this section is mainly on demonstrating the capability of *Albany/FELIX* to efficiently solve ice-sheets steady problems and that is the reason why we used a "reasonably" realistic basal friction coefficient. The validation and discussion of the results of the inversion is out of the scope of the paper, therefore we prefer not to add additional details on this as it would distract the reader from the main focus of the paper. We added a sentence in the paper where we acknowledge the deviations and refer to the JGR paper for discussion.

4 References

We have added many but not all of these references. We feel the addition of all these references is not necessary. In our referencing, we tried to be representative, not exhaustive.

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