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Interactive
Comment

Interactive comment on "Benchmark experiments for higher-order and full Stokes ice sheet models (ISMIP-HOM)[1]" *by* F. Pattyn et al.

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General comments

This paper describes the first intercomparison of a large new class of models for ice flow. These models include those stress components which are assumed to be significant for grounded ice in nonshallow circumstances. Namely, "higher order" and "full Stokes" models. In many cases, clearly, participants developed new numerical models. In addition, several participants have been involved for the first time in ice flow problems through this intercomparison. The intercomparison is a clear success in these

[1] Ice Sheet Model Intercomparison Project for Higher-Order Models; http://homepages.ulb.ac.be/~fpattyn/ismip

important senses.

The intercomparison shows evidence that higher order and full Stokes models produce different surface velocity fields and elevation changes from the shallow ice approximation (SIA). Furthermore the higher order and full Stokes models produce different answers from each other, especially at high aspect ratios, as expected. Interesting and insightful analysis of the reasons for these different qualitative results is given. For all the models, sliding is still a problem, even in the limited senses of producing results which look good qualitatively or producing results which grossly agree.

Some aspects of a report on an intercomparison exercise, like the one under review, are intrinsically hard to criticise and/or improve at the publication stage. Are the final authors of such papers expected to tell all $N \gg 1$ participants to do it again just because a reviewer has complaints? Probably not.

My concern is with the writing of the paper under review, however, not with the underlying comparison exercise. Too often where there could be precise use of the words "accurate", "accuracy", "convergence", "disparity", "valid", "benchmark", and so on, there is instead vagueness. Too frequently, the authors use these terms to "spin" results. They seem to seek words which sound more convincing than saying the results "look good", or were important first results of frankly unknown quality, when that would be an appropriate description. This aspect of the paper, along with a critical but technical concern about the "analytical solution" putatively found in (Gudmundsson 2003), needs to be fixed.

What should the reader take away from the paper under review? Certainly it is news that so many researchers are now focussed on models with longitudinal/membrane stresses. The simplified boundary value problems described in ISMIP-HOM are likely to become standard (benchmark) experiments, even if the solutions presented in the paper are not themselves benchmark results. Model to model comparisons are reasonably clearly analyzed here, but such comparisons have also appeared elsewhere in

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rich detail. For instance, the reader might consult (Leysinger Vieli and Gudmundsson, 2004) for comparisons between full Stokes and SIA or (Hindmarsh, 2004) for comparisons among flavors of higher order ice flow models.

Specific comments

In summary, the intercomparison consisted of several experiments (A, B, C, D) which were simplified in particular ways: isothermal, periodic boundary conditions, no surface evolution ("diagnostic"). Another experiment (F) has surface movement ("prognostic") but is otherwise simplified in the same way. Finally a diagnostic experiment (E) is performed based on flowline geometry data (surface and bedrock elevation) for an Alpine glacier. Two of the experiments were three dimensional (A, C), while the others had a direction of symmetry which allowed them to be performed by flowline models (B, D, E, F).

As noted, the terms "accurate", "valid", "benchmark", and so on, are used without precision. The paper becomes more convincing if the reader believes these terms are used carefully.

At most points when such terms are used, especially in the conclusion, which of these precise questions is being addressed is not clear:

(*i*) how far are the numerical results from each other?

(*ii*) how far are the "higher order" results from the "full Stokes" results?

(*iii*) how far are the numerical results from the authors' understanding of nature?

(*iv*) how far are the numerical results from the exact solutions, even if those exact solutions are not known, of the continuum model(s) in use?

Not all of these questions can be addressed at this last stage by the authors of this paper, but some certainly can be. There is an effort to address (*i*) through Table 4,

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but instead of distances between results we just get the statistics of the maximum values of the results, a hard-to-interpret function of the numerical model. Question (*ii*) could be addressed by presenting slightly more statistical evidence; I would love to see a histogram figure which shows a "double hump" with one cluster being the Stokes results and one being the higher order results. Question (*iii*) could be addressed by the inclusion of additional (presumably!) known data; the lack of a comparison to observed surface velocity for Haut Glacier d Arolla is remarkable and hard to explain with generosity. (Said another way: why the Little Ice Age geometry and not the modern geometry of this or some other alpine glacier with modern surface velocity measurements?) The remaining question (*iv*) could have been addressed by a more careful search of the literature for useful special cases or a consultation with mathematicians who specialize in such things, I believe. (See technical comments below.)

Note that the continuum model in use, that is to say the partial differential equations which are being approximated by the participants, varies among the participants. That is, it is FS, L1L1, L1L2, etc. This makes it all the more important to distinguish the spread of all the results from the spread of the numerical results for a particular continuum model. The authors make efforts in this direction, but this aspect of the paper is certainly one that could be improved by careful rewriting. (See technical comments below.)

There is no figure which directly shows an answer to any of the above quantitative questions. That is, there is no figure which shows the distances (norm of the differences, presumably) between results. Instead we have lots of pictures of the results themselves. Of course we can "eyeball the spread". But the spread itself is not measured. When the authors make an attempt to describe the meaning of the spread, it is frequently not clear which of the above questions is being addressed.

Regarding Table 4, two models could produce radically different solutions which happen to have the same maximum surface velocity. It would be nice to have metrics which were less crude.

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The phrase “analytical solution” is used for certain formulas which appear in (Gudmundsson 2003). They are, presumably, formulas (55)–(58) and (60) in that source, with associated boundary condition-determined constants, but there is no specific mention of equation numbers from (Gudmundsson 2003). (This makes an important aspect of this review speculative; it is the authors’ fault not mine.) On the one hand, concretely, the formula for the “analytic” surface velocity “solution” for the steady-state no slip case of experiment F, which is pictured in Figure 16, is nowhere stated in the paper under review. Instead this “solution” is treated as one of the models even though it is called an “analytic solution” in the abstract and the conclusion, and elsewhere in the paper. But, again specifically though speculatively because the paper under review is not specific, formula (56) in (Gudmundsson 2003) solves differential equation (53) in (Gudmundsson 2003). But equation (53) is explicitly just the description of ϵ^1 and β^1 term in an asymptotic expansion. I think there is no claim in (Gudmundsson 2003) that formula (56) solves the Stokes problem with the bump in the bed boundary condition; it would be a surprising and interesting claim, but I can’t find it.

In fact the result from (Gudmundsson 2003) is appropriately just one of the approximate models in the intercomparison, and *not* a solution to any of the continuum models under consideration. It is not a formula which can be substituted into the Stokes equations, to give exact balance, or into any of the higher order models. Calling it an “analytical solution” in the context of this intercomparison exercise is a land mine for readers of this paper seeking benchmarks. It *is* the result of a deep and insightful asymptotic analysis, an expansion in the small magnitude of the gaussian bump in the bed topography (Gudmundsson 2003). But I would hope that the numerical solutions to the particular Stokes boundary value problem in experiment E actually come *closer* than this “analytical solution” to the exact solution of the Stokes problem. Concretely, the Gudmundsson result should be called an “asymptotic analysis result”, or something like that, and not an “analytical solution”.

The paper under review risks following an unfortunate precedent set by a previous intercomparison, wherein suitable exact solutions were present in the literature but overlooked. Here a search of the literature for exact solutions to the shear-thinning power law flow line Stokes equations is not mentioned; was such a search performed?

Such a search might come up with nothing. In that case, at least an "experiment 0", done before the others, would have been helpful. Namely an experiment based on an exact solution of the flow line, linear, constant viscosity Stokes problem for some boundary conditions like those of the simpler ISMIP-HOM experiments. An exact solution technique in that case, at least, is completely addressed by "potential fluids" (biharmonic) methods in well-known and classical sources within fluid dynamics (e.g. Ladyzhenskaya, 1963). Indeed I would expect that the existing Stokes solvers used by a number of the participants—see the Conclusion for mention of this—were verified with exact solutions. It is too late to do this, I know . . .

Exact solutions in the best case are easy enough to write down that one practical "benchmark result" improvement is immediate: checking future results against a one-line formula is a lot easier than checking future model results against a bunch of data in a "supplemental" file in a *Cryosphere Discussions* website. Indeed, with regard to the "benchmark" claim, *how* is it intended that future ice sheet models using higher order or Stokes will compare to these results? Table 4 only shows maximum surface velocity, while it seems like any of the models could be tuned to hit a maximum velocity number target. It is better to have a more rigid set of numbers to compare to, perhaps merely a maximum and minimum velocity bracket.

The results with sliding illustrate that just having a higher order or Stokes model may represent little progress toward modeling a glacier or ice sheet with complicated regions of no-slip and sliding base. This point is more-or-less acknowledged, but I have the impression that the authors would like the reader to be converted away from zeroth order theories regardless.

The point made in (Leysinger Vieli and Gudmundsson, 2004) has not been addressed

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by the experiments here, namely whether in nonsliding cases the SIA is an adequate model for some major glacier modeling questions like the time evolution of the glacier margin/toe. Time *is* spent beating up the SIA, even as it is claimed that the experiments were designed so that SIA would not apply, so I think it would be nice to have such an acknowledgement.

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Technical corrections/comments

See separate comment

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