

## ***Interactive comment on “Benchmark experiments for higher-order and full Stokes ice sheet models (ISMIP-HOM)<sup>1</sup>” by F. Pattyn et al.***

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### **Technical corrections/comments**

- *page 113 line 2: (Abstract)* The word “validated” has a standard technical meaning in computational fluid dynamics (Roache 1998; Wesseling 2001), namely the comparison of model results to trusted physical observations. These benchmark experiments here are not validation in this sense. Even experiment E (Haut Glacier d Arolla) involved no comparison of model outputs to data. I believe that “validated” is not appropriate here.

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

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- *page 133 line 4*: As noted above in the “Specific Comments”, no analytical solutions are mentioned, which are analytical solutions to the models involved in the intercomparison. As noted, “...of which one is modeled by an asymptotic analysis ...” would be accurate.
- *page 113 line 6*: “Convergence” also has a precise meaning, in too many mathematical references to cite here (though Morton and Mayers (2005) is an example). It is not used with that meaning here. In any case “a good convergence” should say “good convergence”.
- *page 113 line 10*: “hardly influenced by the used numerics”: Clearly this is a matter of judgment. But it would be a matter of *quantitative* judgment if there were quantitative measurements of either numerical errors (differences relative to exact solutions) or differences between model results; there are neither. So the judgment becomes merely an evaluation of whether the reader thinks the graphs look good.
- *page 113 line 26: (Introduction)* “...degrees of approximations ...,” probably.
- *page 114 line 1*: “...so-called higher-order models as analytical solutions are not always available.”: Indeed they are not. This point would be more convincing if references to the best, but still inadequate, exact solutions were given. Otherwise the reader might wonder: Did anyone bother to look?
- *page 114 line 5*: “validated” again, but this time it’s circular. Are the benchmark experiments “validating” the model results, or are the model results “validating” the benchmark experiments?
- *page 114 line 6*: “...paper also allowed for distinguishing ...” should be “...paper also allow distinguishing ...”, probably.

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- *page 114 line 15*: “linear”? This is at least confusing. Presumably the Stokes models, as well as the higher-order models, all used the non-Newtonian rheology in equation (5) with  $n = 3$  as in Table 1, except in Exp F where  $n = 1$ .
- *page 114 line 19–21*: The idea of this sentence could be clearly communicated more simply: “The experiments are described as well-posed continuum problems, so they are independent of numerical methodology.” At least I think this is what is being claimed. (If the goal is to list the numerical methods which might apply to these problems, spectral methods should be added. Spectral methods are the most promising for producing benchmark quality solutions to experiments A, B, C, D, at least. And they were used in the intercomparison.)
- *page 114 line 25*: Presumably there are *two* reasons why there is a switch from non-Newtonian rheology ( $n = 3$ ) to constant viscosity for Exp F. First, there is an existing asymptotic analysis in the linear viscosity case. Second, most (all?) the models are unable to do time-stepping in the non-Newtonian case, I think. If the second reason is not true then it begs the question of why prognostic non-Newtonian experiments were skipped. If the second reason is true, it should be stated.
- *page 114 line 26*: As noted in the “Specific Comments”, this sentence is not true as stated. The formulas in (Gudmundsson, 2003), in my understanding, give an analytical solution to an asymptotic approximation to the Stokes model in experiment F, not to the model itself, or to any of the higher order models considered in ISMIP-HOM.
- *page 115 line 3*: **(General model setup)** “further” is not very clear here. Presumably “higher-order” means “including effects not present in SIA (Hutter, 1983) for grounded ice and not present in SSA (Weis and others 1999) for floating ice” or something like that. Perhaps this ISMIP-HOM paper is a place to clearly de-

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- fine the meaning of “higher-order” even if the resulting definition is slightly more restrictive than is used in casual discussion.
- *page 115 line 11*: “implies” is not true. Perhaps: “Generally, acceleration terms in Eq. (2) are neglected. Ice incompressibility is more easily described if the stress tensor is split into . . .”, or something like that.
  - *page 115 line 21*: Researchers new to the modeling of ice may not realize that this is the point at which the isothermal assumption (stated in the introduction) gets applied.
  - *page 116 line 5*: This definition of the pressure should be moved to the vicinity of equation (3).
  - *page 118 line 11*: This comment is a little odd except for those who have fully absorbed the culture of glacier modeling. For others it may be confusing. Probably just not necessary to say here at all.
  - *page 118 line 20*: Is “oscillations” more appropriate than “bumps”?
  - *page 118-119 line Experiments C and D generally*: A comment “Note that in experiments C and D the basal sliding coefficient goes to zero within the domain”, or something like that, might help prepare the reader for issues that recur later in the paper. There are consequences of this fact for all models of ice flow, not just the SIA (which crashes and burns . . .)
  - *page 119 line 19–22*: I have the sense that here an opportunity has been missed. Namely, for the *linear* 2D Stokes problem, which has a substantial literature, with this kind of switch from a Dirichlet condition ( $v_b = 0$ ) to a stress/Neumann condition ( $\beta^2 = 0$ ), what kind of singularity exists at the boundary? Are the velocities finite? (I think so.) Are the strain rates finite? (Not sure.) Are the strain rates differentiable (i.e. do the velocities have bounded second spatial derivative?

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(Perhaps not, and if not that has profound consequences on all schemes: FE, FD, FV, ...). The mathematical literature of 2D linear Stokes must address these questions, and their must be mathematicians who can serve as guides into the scary literature. It is esoteric literature, until you try to solve these problems numerically, at which point you are either in trouble and don't know it, or in trouble for partly understood reasons; the latter is enough better than the former to make it worthwhile. At the very least some educated speculation on this issue at this point might explain the (common to all models, perhaps?) bad behavior for stress at the basal boundary in Fig 14. From the point of view of "... hardly influenced by the used numerics," an opportunity has been missed to ask and attempt to answer a mathematical/numerical question highly relevant to the reported results.

Note that on pages 124–125 the problems with the SIA in  $\beta^2 \rightarrow 0$  circumstances are appropriately mentioned. There is no hint that the numerical solution of the Stokes equations might not be very nice, though. Do the authors believe that the continuum solutions to full Stokes problem in Experiments C and D are sufficiently regular so that there is no serious effect on the accuracy of numerical schemes, or the choice among them? We are led to believe this is so.

- *page 120 line 16*: "... is taken to be  $100H^{(0)}$  ..."
- *page 121 line 6–12*: After the third reading I realized that the slip ratio  $c$  *had* been defined in these lines. Perhaps that could be made more apparent, though perhaps it is apparent to everyone but me. Said another way, equation (10) already states the content of equation (26). The goal of these several sentences seems to be to say that (Gudmundsson, 2003) defines " $c = (\beta^2 AH^{(0)})^2$ ", that  $c = 0$  corresponds to  $\beta^2 = \infty$  (the no slip case), and finally that  $U_b^{(0)} = cU^{(0)}$  follows from (10) and the definition of  $c$  and the condition (25) on the zeroth order term in the Gudmundsson expansion.
- *page 121 line 17*: Here we have it right: (Gudmundsson, 2003) is an analytical

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- model, not a solution.
- *page 121 line 19–20*: In my understanding of the large taxonomy of shallow approximations of the Stokes equations, “The different Stokes approximants all in some way start from the ... (SIA) ...” does not quite make sense. The word “start” could refer to mathematical small-parameter derivation, in which case it isn’t true. More likely a cultural meaning is intended. Perhaps: “The different Stokes approximants extend the SIA in various ways ...”?
  - *page 122 line 1*: I think Hindmarsh has also adopted the term “membrane” stresses, which may be helpful to those who start their thinking in terms of the ice shelves and streams instead of starting (conceptually) from alpine glaciers. I’m never sure how to name these stresses clearly myself ...
  - *page 122 line 6*: If the literature really contains evidence that, or an argument that, the resulting systems are generically “better conditioned”, then I would love a reference. Perhaps other readers as well.
  - *page 122 line 11–28*: Here is the clearest summary of this nontrivial classification I have yet read. Appreciated.
  - *page 122 line 27*: I think the symbol “ $R_{zz}$ ” is not used elsewhere in the paper. Needed? Does the phrase “vertical resistive stress” already sufficiently identify the concept in (Van der Veen and Whillans 1989)?
  - *page 123 line 5*: “on each” perhaps?
  - *page 123 line 6*: “accompanying”
  - *page 123 line 9–10*: I think “for Exp. A” and “for Exp. B” should be parenthetical: “... across the bumps at  $y = L/4$  (for Exp. A) and ...”. Not critical.
  - *page 123 line 8–24*: Clear. Appreciated.

- *page 123 line 24–26*: This difference is made quantitative in Table 4 so that table should be referenced here. (This reviewer was prepared to criticize—not surprised, you say?—until he read 6 more lines to find the reference to Table 4.)
- *page 123 line 26*: I think what is meant is “L1L1 and L1L2 models display by far the lowest accuracy if the average of the full Stokes numerical results is regarded as the correct solution”. If this is meant it should be clearly said. And perhaps that average should be prominently displayed or otherwise reported. In fact the argument seems to be that the tight clustering of the full Stokes results justifies using their average as the correct solution. In this case the tight clustering should be pointed out first, then the explicit claim that their average is virtually correct second, and finally the assertion that “L1L1 and L1L2 models display by far the lowest accuracy” by this new standard. It then seems like a convincing argument to this reviewer, if that is what is being argued.
- *page 124 line 7–8*: The sentence starting “Differences are related ...” is not coherent. It makes sense to me if the first “are” is dropped: “Differences related ...” Is this the intended meaning?
- *page 124 line 8*: “peculiarity”? I am not sure what is meant, but I don’t think “particularity” is a standard english usage.
- *page 124 line 6–24*: Cool. Makes me want to work on that hypothesis (though, by implication, others are already on the trail).
- *page 125 line 8*: “accurate” → “consistent”. Unless an unmentioned exact solution is known.
- *page 125 line 115–16*: “and inversion” seems unnecessary. Is this the intended meaning?: “Similar to Exp. B, a surface velocity field anti-correlated to basal friction is observed for full Stokes models. For the other higher-order approximations this is not observed (but due to the ...”

- *page 125 line 18–22*: I have no idea at all what is meant by “accuracy” here.
- *page 125 line 22-23*: “The results of L1L1 and L1L2 ...”: The meaning of this sentence is clear and supported by the available evidence.
- *page 125 line 24*: (Entire paragraph.) “Discontinuity” actual means something. That meaning is totally ignored in this paragraph. If “sudden change” is meant, then say that.
- *page 125 line 24*: (Entire paragraph.) So now we get to a crux. The rest of the paper has been implying that the small spread of full Stokes solutions implies that their average (or some other unspecified conclusion from the full Stokes part of the intercomparison) should be trusted in evaluating the “accuracy” of the higher order results. Suddenly the reader is asked to be very generous.

I don’t claim these issues are easy. Some up-front, honest evaluation of what can and can’t be known in this intercomparison would be refreshing, however. And some consultation with professional numerical analysts of linear Stokes might be useful, too; the phenomenon may occur already in linear Stokes, or it might not, but either knowledge would help. Later in the paper it is pointed out that the full Stokes solvers mostly derive from existing linear Stokes solvers; now we see a failure to follow this up by asking what is known about such well-established software.

- *page 126 line 14*: Now “accurate” means absolutely nothing at all. It is also not clear what feature in Fig. 13 is being spun.
- *page 126 line 14–15*: Apparently the reader is supposed to classify the results shown in Fig. 14 by the nowhere-stated resolution choice made in producing the results. (Constructively: Smoothness can be measured by summing magnitudes of derivatives, normalized to be integrals. If all that is needed is evidence for the statement “subsampling the input data produces smoother output,” a desirable

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property having nothing to do with accuracy, then a figure supporting that claim is constructible.)

- *page 126 line 18*: “accurately” again, sans meaning.
- *page 126 line 20–28*: This paragraph is progress. Namely, intercomparisons can be useful when they identify issues of communal confusion. Done. *(Not sarcastic! It sounds like a topic which might lead to a very important PhD thesis or too, and perhaps the salvation of some models. Are the authors gutsy enough to stand up and say here with confidence that they did something useful?)*

- *page 127 line 3–4*: The sentence “Real benchmarking . . .” implies that the authors think that the one analytical result mentioned, that from (Gudmundsson 2003), is a useful benchmark result. Presumably that means a solution of one of the continuum models used on the given experiments. I think they are wrong.

Gudmundsson is an author; this point should be made very clear. The first word of the title of this paper is “Benchmark”, so it is not a technicality. (I will publically admit error if necessary, but not addressing this issue is unacceptable.)

- *page 127 line 3–11*: The fact that the asymptotic analysis in (Gudmundsson 2003) is only for the steady state, and indeed that all the models only reported their steady states, should be made clearer given that the experiment is spun as “. . . the only time-dependent experiment and therefore interesting to evaluate the transient behavior . . .”. The numerical models are all producing functions of  $t, x, z$ , and they have some unknown accuracy relative to the unknown exact solution of the full Stokes problem, and I think *that* accuracy is the one of interest if one is desiring good glacier simulation.
- *page 127 line 6*: “a few”

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- *page 127 line 13–14*: Clearly the authors imply (and think, presumably) that the result from (Gudmundsson 2003) is a solution to the full Stokes equations. Reading that paper strongly suggests otherwise. It is reasonable to compare numerical solutions to asymptotic analysis, but the suggestion is that the authors don't know that is happening.

Of course it is complicated: “numerical solutions of shallow and less-shallow continuum approximations of full Stokes, and numerical solutions of the full Stokes problem itself, to asymptotic analysis of the full Stokes problem itself” is probably more descriptive, awkward, and essential in some form.

- *page 127 line 15*: “exercise”
- *page 127 line 19*: Good. At least we have a falsifiable assertion here: “both solve the same equations”. Instead, I claim that the (uncited, so I had to guess) solution in equation (55) in (Gudmundsson 2003) solves equation (53) in (Gudmundsson 2003) but that together (55)–(58) do not solve the full Stokes system along with the boundary conditions which apply to the steady state of the full Stokes system in Exp. F.

If this is what the authors are asserting, did they check?

Finally, we can be either right or wrong; either is progress.

- **Conclusion generally**: As noted above in several places, I think the authors are evaluating the “higher order” models relative to the average of the full Stokes results. Again this happens in the conclusion. This assumes that the full Stokes models are a better model for ice flow. If so, perhaps this should be said here: “Let us suppose that the average result from the full Stokes models in this intercomparison is close to the flow of actual glaciers in these simplified circumstances. In this light, the higher order models in the intercomparison . . .”

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- This is almost said in lines 11 and 12 on page 128, but not quite. Instead the exact solution is brought up in a way that suggests that the authors know the exact solution.
- *page 128 line 5*: “augmenting the representativity of the evaluation”: I am not sure what is intended here. I think the phrase is optional.
  - *page 128 line 7*: Again, no evidence for convergence has been given. Some other word than “convergence” is appropriate, but I don’t know which.
  - *page 128 line 19–20*: There is no evidence for this extraordinary statement.
  - *page 128 line 24*: If “valid” means “good looking” then that should be said. If it means “small spread between results” or “close to average result from full Stokes” then these should be said. I have no idea what is being asserted here (or in several other places in the conclusion).
  - *page 129 line 3–4*: One final disaster. There is no evidence whatsoever for this startling claim of superiority to (Huybrechts et al 1996), which is comparing apples and oranges anyway.
  - *Page figure 9*: This is confusing. I think that the caption may not match the figure. If there is some reason to show results for experiment A for  $L = 5, 10, 20$  km but experiment C for  $L = 40, 80, 160$  km, that should be clarified.

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Interactive comment on The Cryosphere Discuss., 2, 111, 2008.

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