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The Cryosphere
Discussions

Interactive comment on "Lessons from the short history of ice sheet model intercomparison" *by* E. Bueler

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Solicited Review *by Alun Hubbard & Nina Kirchner (Stockholm University)*

General Comments

This somewhat outspoken manuscript appears to be a heartfelt plea from a bona fide mathematician for analytical rigueur, precision of language and a more systematic comparison and analysis of differences between results, observations and exact solutions within Ice Sheet Model (ISM) intercomparison and benchmark experiments. The manuscript draws on E. Bueler's own (considerable) experience in developing exact solutions (thereby providing robust benchmarks) for the thermomechanical shallow ice approximation but is particularly motivated by (and based on) his review of the

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Interactive Discussion

Discussion Paper

manuscript submitted to TC on the 'Benchmark experiments for higher-order and full Stokes ice sheet models (ISMIP–HOM)' by Pattyn et al. (2008). In this role (which translates directly into the present manuscript) the author is negative and disparaging and (in our opinion) incorrect in the somewhat unsubstantiated, speculative and rather flippant remarks made therein. Although there does not seem to be a firm set of conclusions to be drawn from the manuscript, the abstract focuses specifically on the need for the measurement and analysis of differences between model results and analytical solutions, observations or the results themselves in any intercomparison experiment. This is a valid point, as are the two main other points made in the manuscript regarding the precise use and definition of language, the necessity for analytical solutions and an analysis of the limitations of numerical solutions to partial differential equations, all of which the ISM community are aware of and would fully acknowledge. However, these are not particularly new points (as E. Bueler duly acknowledges) and they could be made much more succinctly and professionally and thus do not warrant a basis for a paper in TC (or necessarily in TCD in its current form). Furthermore, although the body of the manuscript is broken down into an introduction (motivation) and three main sections dealing with mathematics, comparisons and language, it lacks a coherent structure and any really useful positive content. It is somewhat superficially written and takes the form of an editorial which is quick to criticise and as such reads at times much like a 'jargon laden' tabloid diatribe but offers very little in the way of concrete positive advice or improvements on the status quo, which the ISM community would welcome.

Our recommendation is that it be rejected in its current form. That is not to say that it is beyond redemption; there are some nicely put and useful gems in there; the very last paragraph is well written, genuine and helpful. However, if it is to provide a useful contribution to The Cryosphere (and the ISM community) rather than represent a polemic on the state of play from the perspective of the *true* mathematician in glaciology who is putting the wayward world of pesky ISM modellers to right then a lot more is required of this manuscript. We would recommend a complete rewrite, toning down

TCD

2, S417–S428, 2008

Interactive Comment

and losing much of the self-assigned superior narrative and editing out the more flippant/unjustified remarks, adding and offering real improvements including the definition of an actual experiment(s) '0' to compliment the ISMIPHOM intercomparison and finally (possibly) getting the participants of the ISMIP-HOM to actually run it.

Specific Comments

Some of the specific issues raised in this manuscript single out ISMIP-HOM as a specific example of 'bad practice' and as such these have previously been addressed in F. Pattyn's response to E. Bueler's review of the ISMIP-HOM manuscript (which can be found within TCD archive). Specifically, the use of language and the requirement of strict definition of the terms validation, verification, confirmation etc. when describing intercomparison results and their relative performance has been acknowledged. Furthermore, the lack of an 'exact or analytical solution' in the case of ISMIP-HOM experiments has also been addressed. In this latter case, it has been established that the prognostic experiment F based on a linear Stokes solution of Gudmundsson (2003) is an analytical solution to the first order perturbation analysis of flow down a uniformly inclined plane which does represent a valid benchmark against which model efficacy can be judged. Moreover, the remaining simplified geometry experiments have sound reference solutions provided by spectral methods. In this, the ISMIP-HOM intercomparison provides a useful and much valued first step towards intercomparison and testing of higher-order and full-Stokes models which goes a long way beyond the somewhat limited benefit than is implied in the abstract.

The addition of an experiment '0', as E. Bueler requests, 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' indeed would be useful. This is particularly true in the provision of a one line formula which could generate a solution against which a model result could be checked and the differences analysed consistently rather than against a dataset held in a "supplemental" file; especially since grid spacing was not predefined in the ISMIP-HOM experiments. However, to criti-

TCD

2, S417–S428, 2008

Interactive Comment

cise ISMIP-HOM for not consulting ‘professionals for this job... mathematicians... as insurance against possible embarrassment’ is disingenuous. All of the ISMIP experiments have been widely publicised within the geophysics and cryosphere community and totally open to consultation, hence we are at a loss as to why any ‘professionals for this job’ have not become involved in the process and suggested an ‘exact solution’ on the lines of experiment ‘0’ before now. Furthermore, the obvious is begging to be made and perhaps needs to be emphasised here: the reason why the ISM community is dealing with 3d numerical solutions to non-linear Stokes equations in the first place is precisely because analytical techniques are not available and capable of the task and there being no exact solutions for a nonlinear flow rheology across complex basal boundary conditions. If they do, then we beg ‘the professionals for the job’ to enlighten us and perhaps even present them in full detail and in a manner which is of actual pragmatic utility to the ISM community. Referring to the main mathematical preoccupation with Sobolev Spaces and specifically to Ladyzhenskaya (1963) as a ‘classical source’ for such a solution does not really illuminate the path to nirvana.

On tracking down this text, it becomes immediately clear that ’precise language’ and ’precise definition of terms ’ is *really* mandatory if major confusion shall be avoided:

“Before becoming involved with precise formulations, we call the readers attention to the fact that the statement ’it has been proved that the problem has a unique solution’ can have very different meanings depending on the function space in which one looks for the solution. The form in which the requirements of the problem must be satisfied is different for different spaces, and different extensions of the concept of a solution of a problem, i.e. different ’generalized solutions’, present themselves. In fact, for every problem there are infinitely many ’generalized solutions’, but they coincide with the classical solution, if the latter exists. In this book, we select from this set the kind of solution introduced in the paper [Kieselev & Ladyzhenskaya, 1957], for which it was first proven that boundary-value problems have unique solutions in the large. These solutions, together with some of their derivatives, will belong to the Hilbert space L2(QT).

The comparative simplicity of the studies in this case is explained by the fact that the Hilbert spaces are structurally related to the variational forms of the hydrodynamic laws, and the basic law of energy dissipation and some others express in the norm of these spaces [...]. In other words, the basic a-priori estimates, on which all these investigations are based, are formulated in the norms of these spaces."

Source: 'The Mathematical Theory of Viscous Incompressible Flow' by Olga A. Ladyzhenskaya, Second English Edition, Translated from the Russian by R. A. Silverman, Gordon and Breach Science Publisher, New York, London, Paris, 1969, 224 p.

The question on the tips of our tongues now is: which solution is meant in E. Bueler's call for an 'exact solution'? Moreover, there are a couple of points to be drawn with regards to this quote from this 'classical source'. 'Spaces' is a key word in the above quote of Ladyzhenskaya, and, as Bueler states on p. 405, it will be 'Sobolev spaces which must be accepted into the world of intercomparison for ice flow'. Similar statements have been made before, in which the introduction of Sobolev space into the computationally oriented Engineering Sciences was loudly proclaimed to definitively address issues over the convergence of numerical solutions (rather than stating that they have converged after some residual had become smaller than some threshold after a certain number of iterations). What ended up happening in the majority of cases was that an increasing number of papers and theses from Engineering Departments contained sections on "the mathematical foundations of the finite element method", including neat passages on variation formulations leading, inevitably, to a definition of a Sobolev space. Since this became merely a formality and nobody in the engineering world could really be much bothered about this, only few checked the original sources and made the effort to copy a correct definition of Sobolev spaces and in the main, they were copied and/or blended incomplete or simply incorrect definitions from various sources with inconsistent notation. This is probably the wrong way of introducing mathematics to the computational engineering sciences. Introducing Sobolev spaces alone is not enough, we can do it right here and just to prove the point; for example, is the space H^k, a common and rather simple Sobolev space defined by:

$$H^k(\Omega) := \left\{u \in L^2(\Omega) : \partial^\alpha u \in L^2(\Omega), |\alpha| \leq k\right\} k \geq 0, k \in N, \Omega \subset R^n \quad (1)$$

where L^2 denotes the space of all real-valued, square-integrable functions on the bounded domain Ω. The α-th partial derivatives of the function u are denoted by the ∂^α, as is common when dealing with partial differential equations. We refrain from introducing scalar products and associated norms here but mention that specific choices of them make the Sobolov space H^k a Hilbert space.

Are we any wiser?

Introducing Sobolev spaces and all the associated mathematical machinery into numerical ice sheet modelling is going to be a pretty thankless task unless it can be done in a sustainable and appropriate manner. To begin with, a 'translation' of the concepts into a language that is accessible for the numerical ice sheet modelling community is perhaps the best way to go about it from a didactical point of view. Once, and if interest is raised, understanding can be enhanced by offering special courses on the mathematical foundations of the ice flow.

The comment 'there are mathematicians and there are mathematicians' is simply derogatory and unnecessary; this sort of language and intent is inappropriate in a scientific journal. Furthermore, there is also a small paradox contained therein. To criticise Kolumban Hutter, Andrew Fowler and Heinz Blatter for being primarily concerned with the formulation of new continuum models rather than 'the main stream of the mathematics of continuum models… (which is) the study the qualities of predictions and the quantification of behaviour of particular continuum models' really has to reflect the external needs and requirements of our discipline and its relevance to society. What mathematicians may or may not be preoccupied by comes as no particular surprise and is likely very far removed from what the ISM community does to earn its keep, in much the same way as what

politicians, bankers, economists or oil companies do to earn their keep is equally far removed from what the ISM community does. One might hope that in time, important breakthroughs filter through if everyone does their job thoroughly and professionally and communicates appropriately.

The ISM community cannot though continue (mis-) applying the Thermomechanical Shallow Ice Approximation (TSIA) *ad infinitum* solely on the basis that it is well understood, easy to apply, is mathematically defined with a nice and neat analytical solution and benchmark provided by Bueler (2006). The TSIA is undoubtedly a very useful solution which can and has been applied to many ice masses in a sensible and successful manner. Unfortunately though, it has equally been misapplied and is simply unfit for addressing many of the problems and scenarios that are of particular interest and concern to the discipline and society as a whole; these are predominantly concerned with fast-flowing streams and calving outlets which have complex patterns of basal traction and longitudinal stress gradients, ice masses which are grounded below sea-level with marine terminating sectors which are buttressed by ice-shelves and smaller outlets and glaciers that flow within complex relief.

The work of those applied 'mathematicians' mentioned above as well as those who contributed to the ISMIP-HOM experiments represents the best efforts of the ISM community to become relevant and to develop and test a new generation of models which have the efficacy and are fit to address the relevant cryospheric issues and questions outlined above. All of these problems are complex and some of them are likely intractable but this does not mean that the ISM community should cease trying or continue to make do with the TSIA simply because it is relatively well understood and has a valid benchmark. The 'explosive growth' of higher-order and full-Stokes models in the last decade attests that the community is getting there and in this respect, that the efforts of ISMIP-HOM should be applauded since it brings some well overdue consensus and comparative order to this new generation of tools.

More Technical Comments

Pg 400:

Abstract – does not reflect the content of the paper.

Line 10 – why ‘cultural’? what is specifically cultural rather than scientific about iii & iv? Why is it particularly ‘your’ perception – i), iii) & iv) would be recognised by all involved and others as a worthy purpose.

Line 13 - (ii) We do not think this is true. There is one case for ISMIP-HOM and this is an exception rather than the rule due to the novel nature of the finite-element solution developed by the ELMER model used; virtually all the other models and techniques have been developed and applied previous to ISMIP-HOM.

Pg 401:

Line 2 – as noted above, ISMIP-HOM did not stimulate ‘explosive growth’ in models; the majority of them were in existence well before it was initiated and some of them for over a decade. We are not sure what the implication is with these sorts of statements... that everyone is jumping on some Higher-Order bandwagon? In the least this implication is irritating and incorrect; at worst it’s slightly demeaning to those who have invested fairly hefty efforts into developing higher-order/full-Stokes models.

Line 3 – We think intercomparison is justified by more than participation and discussion... what about the reasons outlined preveously. This is a meaningless statement and on reflection the whole of this paragraph is fairly pointless; I cannot work out what you are trying to say and much of what you do say is incorrect (e.g. Glimmer was not stimulated out of an intercomparison exercise – it was stimulated out of a need for a community ice sheet model that that could be readily interfaced with GCMs and be coupled to earth system simulators - ask Tony Payne).

Line 9 – We don’t particularly understand this one either – it does not make sense nor is backed up. We do not see any evidence (other than the one paper) of many

TCD

2, S417–S428, 2008

Interactive Comment

ISMIP-HOM & MISMIP modellers getting early papers out of ‘new codes’.

2. On measurement...

The main point seems to be that where exact solutions do not exist then an inter-comparison cannot claim a ‘benchmark’ status; this is fair but can be said much more succinctly.

Pg 403:

If there is an experiment ‘0’ for ISMIP-HOM, then please describe it fully here. The utility of such an exact solution is clear and does not need to be spelt out. It should be developed and the author should get on with the job of presenting and explaining it. Alluding to the fact that there might well be one does not help the ISM community. The overarching claim in this manuscript is that there are ‘professional for the job’; it is right here then that the substantive contribution is begging to be made. Suggesting that Frank Pattyn should be embarrassed for not consulting these professionals and hasn’t done his job properly is not helpful and smacks of, well we’re not entirely sure; sour grapes?

Informing us what mathematicians do or do not do is not really relevant as is the continuation of this section into one concerning Alice & Bob... climate scientists and physicists simulate virtual, reduced ice sheets all the time with highly selective and questionable ‘real data’... in the UK at least, trying to get research agencies to fund ‘slinging a GPS’ on to a glacier is in fact rather difficult in the present GCM obsessed ‘climate’. Generally speaking, from a flick through Journal of Glaciology or TC, the relatively young discipline of glaciology appears to be quite ecumenical/post-modern in its outlook.

Pg 405:

Line 7: Para here and next is well written and useful stuff.

Line 25 onwards: is mainly irrelevant including most of this section into pg 405. The final para of this section has a good point.

Pg 406:

On measurements. . .

Yes, desirable and important but again what is the point here? The main issue that observations need to be set aside for testing that are independent of those used for tuning is well made and readily attributed van der Veen. Secondly, that it is desirable for data-sets to be made public? Yes, well said but this is not a great a revelation and no surprise.

Pg 407:

This seems to be a very general discussion of the use of real datasets in modelling and how this is not appropriate for the tuning and testing of inverse models. Again, I think everyone would agree with you and the point has already been made by van der Veen. I am having difficulty seeing what is particularly new or relevant in this discussion.

4. On language:

There are valid and useful points made in this section and it is nicely written and researched. As noted, it finishes on a good paragraph which is helpful.

TCD REVIEW EVALUATION CRITERIA:

IN YOUR EVALUATION PLEASE TAKE INTO ACCOUNT THE FOLLOWING ASPECTS:

1) Does the paper address relevant scientific questions within the scope of TC?

No.

2) Does the paper present novel concepts, ideas, tools, or data?

No.

TCD

2, S417–S428, 2008

Interactive Comment

3) Are substantial conclusions reached?

No.

4) Are the scientific methods and assumptions valid and clearly outlined?

N/A.

5) Are the results sufficient to support the interpretations and conclusions?

N/A.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

N/A.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

No.

8) Does the title clearly reflect the contents of the paper?

Yes.

9) Does the abstract provide a concise and complete summary?

No.

10) Is the overall presentation well structured and clear?

No.

11) Is the language fluent and precise?

No.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and

TCD

2, S417–S428, 2008

Interactive Comment

used?

N/A.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Yes – most of it.

14) Are the number and quality of references appropriate?

No.

15) Is the amount and quality of supplementary material appropriate?

N/A.

Interactive comment on The Cryosphere Discuss., 2, 399, 2008.