The Cryosphere Discuss., 2, S107–S111, 2008
www.the-cryosphere-discuss.net/2/S107/2008/

The Cryosphere
Discussions

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

F. Pattyn et al.

Received and published: 24 May 2008

We fully agree with referee 1 that the choice of words in the paper is ambiguous. Following his advice, we will take better care of using the words accurate, accuracy, convergence, disparity, validity, benchmark, etc. by referring to the literature on V&V (Verification and Validation) that is available, such as the guidelines that are given by Babuska and Oden (2004) Verification and validation in computational engineering and science: basic concepts, Comput. Methods Appl. Mech. Engrg. 193: 4057-4066. We will therefore better describe the scope of the paper, i.e. verification of model solutions by means of an intercomparison.

As for the analytical model of Gudmundsson (2003), we would like to add that these

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

are definitely analytical solutions, not of the exact Stokes problem, but an analytical solution of first order perturbation analysis of flow down a uniformly inclined plane. It is inherent in this type of analysis that the resulting flow perturbations are linear functions of basal amplitudes. Numerical solutions are usually not limited by this assumption and can therefore, for any finite amplitude perturbation in basal properties, be better approximations to the Stokes equations than the analytical solutions given by Gudmundsson (2003). For an accurate comparison with the analytical solutions, numerical solutions must be calculated for a number of different amplitudes and then scaled by forming the ratio between each solution and the respective basal amplitude. If this ratio is found to be independent of amplitude for small amplitudes, the scaled numerical solutions can be compared to the analytical ones. This kind of test was done by Raymond and Gudmundsson (2005, see Figs. 2 and 3). The exact error estimate depend on wavelength, amplitude, and slip ratio and the reader is referred to Raymond and Gudmundsson (2005) for a detailed discussion. As an example, however, the analytical solutions were found to be generally accurate to within about 1% for sinusoidal perturbations with wavelength larger than about 10 h and amplitudes less than 0.1 h. For experiment F, we expect similar degree of agreement between the analytical solutions and exact Stokes solvers. Therefore, the analytical solutions can be used as a comparison or benchmark. This information will also be added to the revised version of the paper.

Model to model comparisons have been done in a couple of other papers that are cited by the referee (Leysinger-Vieli & Gudmondsson, 2004; Hindmarsh, 2004), but they remain a comparison of one model with another. The aim of ISMIP-HOM is to compare a whole suit of models (intercomparison). While it is not a 'real' benchmark (a benchmark problem is a specific, simplified model problem for which accurate solutions or analytical solutions are known - Babuska and Oden (2004)), experiment F is a benchmark experiment in that respect, and for experiments A-D we have a reference solution given by the spectral method.

TCD

2, S107–S111, 2008

Interactive Comment

Specific comments

In the revised paper we will try to answer the list of questions (those that apply to this paper anyway) raised by referee 1 in the following way:

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

The statistics shown in Table 4 will be expanded. It is true that only maximum velocity in the flow direction is not enough to fully grasp the content of the experiments. We will also analyze the norm of both horizontal surface velocity components, besides maximum and minimum values, that remain key elements in the analysis.

(ii) How far are the higher-order results from the full Stokes results?

It would be great to be able to show a 'double hump' distribution that would clearly distinguish between the HO and FS results. However, the sample with different models is too small to obtain such a nice distribution. On the contrary, statistical parameters (already presented for the maximum surface velocity) such as mean and standard deviation per model category offer a way of characterizing the difference between higher-order and FS models. These results clearly show that the spread of FS models is much smaller than the one for HO models. There is not really a cluster for FS and one for HO models, since they both converge to the same solution, except for experiment B and D at resolution L=5km; the main difference is that the FS results are more clustered together than the HO results. There is however no need to go too far in this: at the L=5km resolution, the FS models show an inversion in the surface velocity field compared to HO models. Statistics are not needed to demonstrate this. Nevertheless, in view of the earlier remarks, the analysis will be rewritten so that terms as ‘accurate’, ‘verified’, etc. are properly used.

(iii) How far are the numerical results from the authors’ understanding of nature?

This is not really the scope of the present paper. The paper treats an intercomparison

TCD

2, S107–S111, 2008

Interactive Comment

of computational models that represent several mathematical models (FS, L1L2, L1L1, …). We do not focus on the mathematical model and how that one represents nature ‘accurately’. The paper focuses instead on how well different computational models fit or are in agreement with a mathematical model (in the case an analytical or exact solution exists).

Comparison with data from Haut Glacier d’Arolla is very difficult, as the models are focusing on specific features, such as membrane stresses. Viscosity for instance is taken constant, while in reality – and if we want to compare model velocities with observations – changes in viscosity and impurity content will matter, as will geometry effects (3D geometry of the glacier). Furthermore, as a boundary condition to the vertical velocity field needed in FS models, surface mass balance matters as well. The lack of all this information and on the transient behavior of Arolla prevents us to make a comparison with observed surface velocities. In that case it does not really matter what type of geometry is chosen (Little Ice Age or modern). Similar aspects relate to any other alpine glacier. The choice of Arolla stems from the basic experiments that were carried out in the Blatter et al. paper in JGLAC.

(iv) How far are the numerical results from the exact solutions, even if those exact solutions are not known, of the continuum models in use?

The comparison of the models is done within each of the model groups, i.e. FS, L1L1, L1L2, …, so that we distinguish between the different mathematical models. On behalf of the analytical solutions we will add some more detailed information provided by Gudmundsson.

The referee also asks for a figure that answers one of the above questions. We intend to calculate the standard deviation for each series of models (FS and L1L2) and plot the mean velocities with the standard deviation in grey scale instead of all separate curves. The separate curves are given anyway in the supplementary material. A refined version of Table 4 will not only give the values for the maximum surface velocity,

TCD

2, S107–S111, 2008

but for other measures as well, such as the norm of the surface velocity (both horizontal components) and the minimum velocity.

As for the analytical results of Gudmundsson, the discussion is given at the beginning of this letter.

Finally, we do not claim that SIA is not an adequate model, on the contrary. As many studies have shown, SIA is valid for large parts of glaciers and ice sheets. However, its validity is limited in some areas of high bedrock variability and where rapid changes in basal sliding occur. The present paper focuses only on that part where the SIA is not valid. Since more and more such higher-order models are being developed, an intercomparison exercise for verification of such types of models is therefore adequate. For non-sliding cases and low bed variability, for instance, the SIA is perfectly valid. We will state this clear in the revised manuscript.

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