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The Cryosphere
Discussions

Interactive comment on "Data assimilation and prognostic whole ice-sheet modelling with the variationally derived, higher-order, open source, and fully parallel ice sheet model VarGlaS" *by* D. J. Brinkerhoff and J. V. Johnson

Anonymous Referee #2

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1 General appreciation

This paper presents the detailed description of a so-called 'next-generation' ice sheet model. It is fully thermomechanically coupled and solves the Stokes equations either completely or according to the higher-order approximation (Blatter/Pattyn). Novelties of the model lie in the way it is solved: it uses a variational principle to solve the Stokes equations and enthalpy to solve for thermodynamics. Both are improvements that have advantages compared to previously developed models. Furthermore,

it comes with an automated framework for model inversion, which is a major asset in model spinup/initialization. The model description is given in much detail and a series of verification experiments are presented to show the validity of the model. Finally, the model is applied to a simulation of the future behavior of the Greenland ice sheet.

The model has definitely major advantages compared to other existing models, but it has moreover some major limits, limits that hamper the proper use/application of the model to, for instance, the Greenland ice sheet. Two major limits are the use of a linear sliding law and the lack of a moving (dynamic) boundary condition at the ice/ocean contact. All evidence on ice loss of the two major ice sheets on Earth is related to either glacier acceleration due to sliding and thinning (or increased calving) at the ocean boundary. Those two key factors are not included in the model, which seriously questions whether or not the model is clearly a 'next-generation' model. Is solving higher-order physics in a novel thermomechanically-coupled way sufficient to solve the problem of future mass loss ice sheets? I don't think so. The model is not adapted to solve a problem posed by the SeaRise experiment, i.e. the future evolution of the Greenland ice sheet due to increased sliding. A recent publication by Gillet-Chaulet el al (2012) in The Cryosphere did a similar job of presenting a full Stokes model to simulate the future Greenland ice sheet, also limited to some extent by its boundary conditions. However, that paper did - in my view - a better job, because it focused on the experiments, analyzed the sensitivity and discussed the consequences of having some limitations in boundary conditions. In the Brinkerhoff Johnson manuscript, however, it seems that the Greenland experiment is dragged in at the end to give the paper a more glaciological context and the SeaRise experiment is maybe not the best way of validating the model performance.

The paper is rather lengthy with a lot of details on the numerical solution. This is all very interesting and useful for people digging into numerical problems, but not for the reader who is interested in finding out whether this open-source model is useful to apply to a particular problem. Most of this information should be send to one or more Appendices.

Given the rather technical content, i wonder why the authors did not favour the other EGU journal typically designed for such papers, i.e., Geoscientific Model Development (GMD). Alternatively, the paper could be split in two, one description of the model with sufficient detail (eventually in GMD) and one with application to the Greenland ice sheet, but i let the editor decide on this.

It should also be more clear in the manuscript that VarGlaS solves both the full Stokes (FS) system and a higher-order (HO) approximation to them. Crucial information lacking is about the computation time of both (or at least one compared to the other). This is essential information for evaluating its performance. Throughout the text it is difficult to find out whether an experiment is done using FS or HO. It is clear for the verification experiments using ISMIP-HOM, but not for the Greenland or the EISMINT experiments.

For the verification experiments, why did the authors leave out experiments B and D of the ISMIP-HOM benchmark? For instance, Exp B showed a nice anomaly at L=5 km for full Stokes models compared to higher-order models. Does VarGlaS produce the same? Similarly for the EISMINT experiments, the authors did not make any reference to the work by Saito et al. (doi:10.1029/2004JF000273; Annals of Glaciology 46 2007), who in a series of papers investigated the behavior of higher-order ice sheet models compared to SIA models in their performance on that experiment and grid dependence. Hindmarsh (DOI: 10.1029/2008GL036877) also discusses thermoviscous instabilities as a function of the inclusion of membrane stresses (higher-order) and spatial resolution. The problem may be beyond the use of an unstructured grid. What about the enthalpy method (which is novel, but clearly is not discussed in this context). A more lengthy discussion is in order.

Finally, it may be good to include a table with a list of parameters and constants, given the complexity of the paper.

2 Detailed remarks

P 1030, L 13: The prediction of the mass evolution of Greenland cannot be supported by observational data (which is lacking for the future). Should be rephrased. The present-day state or evolution over the last decades can be supported by observations.

L 21: define shallow-ice approximation. What are the major characteristics of this approximation and its validity?

P 1031 L17: change ; into ,

P 1032 L15: reference between brackets

L 18: idem

P 1034: rephrase 'some of the things our model does well'. Mention advantages and/or breakthroughs.

L 13: VarGlaS solves for the ...

L 13: does it solve for temperature or enthalpy? I thought the latter and that temperature was derived from the former.

P 1037 L8: ... yields significant ...

P 1038 L9: 'or some constant much less than' could be better written as $\nu \ll k/c_p$

P 1039 L5: (which can be negative to account for basal accretion)

P 1040 L4: Remove the first sentence. Start with the second and rephrase by 'In the following sections, we discuss how the continuity equations are ...

P 1041: remove each time '1 $\times$' before the exponent. 10^{-6} is sufficient. See elsewhere throughout the manuscript

L 9: is the relaxation parameter = 1 the same as Picard iteration? Are lower values similar to under-relaxation? It should be defined, because it doesn't make much sense to the normal reader.

P 1043 L21: ... this functional to satisfy ...

P 1045 L4: define ALE

P 1046: α isn't defined in the first place. Secondly it is just mentioned that it is equal to one. In short, it could be left out altogether, or it should be defined what the meaning of α is

P 1048 top: Maybe this give a better stability, but what are the consequences by doing so? Has this an effect on sudden stress changes (for instance slip/no slip boundaries), or sudden changes from simple shear to plug flow?

P1049 L12: r equals zero

L19: is there any particular reason for the choice of L=80? Would't it be more appropriate to check the convergence for a more challenging experiment in which the friction field changes over short distances (high frequency) to make it more realistic? In that case a L=5 or 10km experiment would be interesting to look at.

L22: It should be mentioned that the F experiment is done for a linear rheology (n=1)

P 1050 (and following): I find the use of % a^{-1} quite disturbing. A percentage change per year. Why not using the real change in velocity/mass as a measure?

P 1052: the section on data assimilation is rather hastily written (definitely compared to the model description). The reader is referred to three graphs in one sentence and should make up his/her mind on what can be learned from it.

P 1053 L4-5: Awkwardly written. What is exactly meant by 'not in exact alignment with model physics'? So the transient is not resolved, but what about the initialization through inversion?

Discussion: an evaluation on the model performance (calculation time) should be given.

P 1055 L15: I don't understand this sentence: accurate positioning (and its changes) does have a major effect on the evolution of an ice sheet on a continental scale; A large portion is of course mass balance driven, but if you disregard dynamics, then a shallow-ice model will suffice.

L26: Not sure about it. If you wait until the (Greenland) ice sheet has retreated away from the coastal boundary so that its effect is not sensible anymore, then the imposed BC at the edge makes sense. Short and medium time scales are rather ill-defined measures.

P1057 L15: not sure that this is a next generation ice sheet model. There is an inversion scheme which is suitable for initialization of the model, but the use linear sliding and the absence of dynamic boundary conditions does not make the model apt to cope with a number of challenges in glaciology. The higher-order scheme and the finite element grid construction are not sufficient as a condition for large-scale simulations. It may well become a next-generation model whenever important dynamical features are implemented.

Interactive comment on The Cryosphere Discuss., 7, 1029, 2013.