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The Cryosphere
Discussions

Interactive comment on "Quantifying the effect of ocean bed properties on ice sheet geometry over 40,000 years with a full-Stokes model" *by* Clemens Schannwell et al.

Anonymous Referee #1

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In this paper, the authors use a state-of-the-art ice sheet model to carry out (slightly compressed) glacial interglacial simulations on the Ekstrom ice catchment and ice shelf. They use a stokes ice-sheet model with a grounded to floating transition and step forward the ice-sheet model over time scales considerably longer than those generally considered in such simulations. By make use of two different linear system solvers, as well as testing the response to a highly uncertain physical parameter dealing with the nature of the sea bottom in the ice-shelf cavity (at least, this is my understanding, see below). The improved numerical solver shows great gains in terms of computational cost, and the bed geology of ice-shelf cavities is shown to play a large role in fluctuation of ice-sheet volume and potentially lead to hysteretic behaviour.

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Discussion paper

I believe this paper can potentially by published in this journal. The simulations they have done are impressive from a computational standpoint alone, but also raise interesting questions regarding our ability to model past behaviour of ice sheets when we know so little about the marine geology of ice-shelf cavities. I have a few general comments, and a number of detailed comments, however, that should ideally be addressed.

General Comments:

1. Though this is quite specific, it is quite important, and i would like to see it clarified, ideally in the response and in a revision. The central science result hinges around the effect of different bed strength. The methods seems to suggest that, between the “hard bed” and “soft bed” runs, the only difference between the bed frictional coefficient (C) is in areas where this CANNOT be inferred from an inversion of velocities as described in 3.4 âĂŤ in other words, bed within the current ice-shelf cavity âĂŤ meaning in both experiments, C is identical in currently grounded areas. Is this correct??? I ask because section 3.4 would imply this, though i could not find any other part of the paper that made this clear. If the only difference is indeed below currently floating ice, this has very strong implications; however, i fear that (a) i have misunderstood and (b) even if i have not, other readers might. This aspect of the methodology should be pointed out with crystalline clarity to the point that maybe even the experiment names should change to emphasise this.

(And i should add if “hard bed” means 10-1 everywhere and soft bed means 10-5 everywhere, then the results overall are not very surprising âĂŤ so this is why it is general point #1)

2. A key scientific result put forth is that of hysteresis with a strong (ice shelf cavity?) bed, in that the grounding line (GL) does not return to its original position. This is used to argue that even without a retrograde slope (line 364) there can be hysteretic behaviour. I point out that there have been previous studies suggesting that a con-

tinuum of grounding lines were possible, but these (and other) authors later showed that correct treatment of grounding zone boundary layers removed this degeneracy, but this treatment involved resolving the grounding zone, the length of which scales inversely with bed strength. In the context of Stokes, Nowicki and Wingham (2008) found that with an effectively non-sliding bed, there was not a unique sliding solution in the presence of a frozen grounding zone. While the authors' results are interesting, they should allow for the possibility that (a) the grounding zone is not sufficiently resolved or (b) there is not a unique solution to the Stokes contact problem with an effectively non sliding bed (therefore raising the question of whether the model finds the physically correct one) rather than assuming that the model results are correct, and hysteresis of ice sheet is possible without retrograde beds.

3. There are extensive mentions of ensemble modelling in the paper; while you do not say outright you are doing ensemble modelling, you don't say that you are not (aside from a mention that your approach is "complimentary" to it, line 310, which is confusing; it is not ensemble modelling because it does not vertically average?) I would argue you tested 2 end members of a (albeit important) physical parameter (the choice of solver is not a physical parameter), so perhaps you should be as clear as you can be that this is NOT an ensemble of experiments

4. A series of 4 experiments are done, varying one of each: bed strength, and numerical solver âĂŤ and the results of the paper are presented as dual: the effects of the hard bed, and the effectiveness of the solver. It is therefore confusing whether this paper is meant to be about numerics (in which case it might be better suited for a different journal) or about the scientific results. Both aspects are presented quite prominently making the message of the paper a bit unclear. If the paper is to be about science, then aspects dealing with numerical methods such as scaling should perhaps be in an appendix and not feature in the abstract (though i do have comments about these aspects as well).

Detailed comments:

line 73, data of shelf cavity âĂŤ should point out this is only relevant to the present study *up to the farthest point of grounding line advance* in your experiments.

line 174 and potentially elsewhere; please say something about the FEM basis functions in your scheme(s). It is important to establish that the basis functions are LBB conforming and that the solutions are exactly mass conserving (ie. not using penalty methods) âĂŤ the latter perhaps not being as important for short term runs but very important for long term.

line 174 how many cells? how many DoFs?

line 203: are you sure? all physical uncertainty? what about ice shelf crevassing weakening? Not to mention these physical parameters, if i understand correctly, are only varied in the ice-shelf cavity (see General Comment 1)

line 223: since this is exceptional is it really of value for general knowledge? also:

a) it is odd to compare one solver on one system and another on another system. how about an additional test (only a few time steps) of both on the same system, with wall times so a comparison can be made. b) ParStokes has great scaling but what about absolute time for a fixed core count on the same system compared against MUMPS?

line 228: following on from comment on line 174, which of the two uses a stabilisation method? if not both, then what about the other one?

Figure 8: Here or in an appendix you should show a similar plot comparing ParStokes and Mumps for soft and hard bed (whichever shows poorer agreement). It is important to establish that the effect of solver, while having a large difference on performance, has very small effects on Volume and GL position relative to the effect of the physical parameter. If MUMPS and ParStokes differ, at most one is correct âĂŤ if the difference is large, how are we to trust the physical results?

line 250: following what?

line 257: then there is a volume decrease âĂŤ what causes it?

line 289, some funny maths. How does increasing performance by a factor of 6 allow runs of 40,000yrs when using the MUMPS solver allowed less than 1000a? this is not a factor of 6. did you do something to increase the time step that was not mentioned?

line 349: explain what you mean by grounding line “fidelity”

Refs:

Nowicki, S. M. J., and D. J. Wingham (2008), Conditions for a steady ice sheet-ice shelf junction, Earth Planet. Sci. Lett., 265, 246–255

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