The Cryosphere Discuss., 3, C359–C365, 2009
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The Cryosphere Discussions

Interactive comment on "Interaction between ice sheet dynamics and subglacial lake circulation: a coupled modelling approach" *by* M. Thoma et al.

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Received and published: 30 November 2009

This paper presents results from coupled numerical simulations of ice flow and subglacial lake circulation. These simulations seem novel and I am not aware of previous studies achieving full coupling of ice-flow and lake-circulation models. The use of an idealised lake geometry is a meaningful and logical first step, which can be used effectively to elucidate potential feedback mechanisms and the magnitudes of impact. In this study, coupling of models results a relatively large increase (22%) in the simulated heat conduction from lake to ice and this reduces extent of ice melting by 7%. Melting of ice over the lake reduces the ice thickness near the upstream shoreline and this causes ice flow to converge and speed up. When ice grounds against the downstream shoreline, ice diverges and flow speeds are reduced comparatively.

The paper is no doubt suitable for publication in The Cryosphere, but I encourage the editors to ensure that the content of the paper is described and displayed more transparently. I believe that this can be achieved from a series of relatively minor corrections.

The main limitation of this manuscript is the presentation of results. There are a number of places where descriptions are unclear and where the text deviates from the logical next step forward. One example is figure 4, which shows the influence of grid size and model physics in the simulated ice sheet domain. These results are not integrated particularly well in the analysis and it is somewhat puzzling that these secondary results are presented before the coupled simulations (figure 7 and 8).

Some details of the coupling aspect are introduced in the lake model section. This is somewhat confusing and it's not immediately clear whether figure 5 represents output from coupled experiments or a stage that precedes a full coupling.

The above examples are related to a surprising choice of structure. The results from the different models are integrated in separate sections that include the respective model methodology. This is different to the more conventional way where methods are described fully before results are presented. I think the results would be delivered more fluently with the latter approach. Using this style, they authors could still present their results in sections related to (i) 'ice flow; (ii) 'lake circulation' and (iii) 'coupled lake-ice experiments'. The latter would make the results stand out very clearly.

The results displayed in figures could also be presented more clearly. More descriptive captions would for instance make it easier for the reader to understand the principal assumptions of each experimental stage, i.e. whether lake and ice domains evolve independently or together. The last paragraph of the introduction should also state more clearly that absence of lake dynamics in the ice flow section and the absence of ice dynamics in the lake section.

I recommend the exclusion of figure 4. The manuscript is pretty short and going into domain numerics seems to merit a somewhat more lengthy paper in the style of e.g.

JGR or Journal Fluid Dynamics, which this is not. Since this is a relatively short paper, I think it could be delivered very nicely if results were presented in key figures showed results from the various model combinations in different panels. This could be (1) ice dynamics excluding and including lake dynamics as well as the difference and (2) lake dynamics excluding and including ice dynamics as well as the difference. The former would be a merger of figures 3 and 8 and the latter would be a merger of figures 5 and 7. Figure 6 could be either before or after these main figures.

Specific comments by line number:

p. 806, 26: add to after prior

p. 806, 26-p.2, 1: These formation alternatives are essentially the same. I suggest "or whether the it formed subglacially after the onset of glaciation" instead of "or whether it could have survived during glaciation".

p. 807, 4: what is a self-consistent numerical model?

p. 807, 8: Add sentence to explain more clearly what came out of all of these subglacial lake studies and how previous results relate to this study.

p. 807, 10: consider 'over' instead of 'across of'.

p. 807, 11: consider 'isolated systems' instead of 'self contained'.

p. 807, 21: explain why conduction cannot be ignored and clarify that (all?) previous models do exactly that.

p. 807, 21: why does latent heat dominate conduction? Is this only true if geothermal heat is not considered?

p. 807, 24: consider 'lakes' rather than 'the lake'.

p. 807, 26: The sentence starting with "Without this flow. . .." is somewhat contrived. There will always be flow because mass has to move laterally, so it's not particularly relevant to consider the case where there is no flow.

p.808, 1: I suggest adding a sentence to this paragraph. To me, this study represents a nice break-through. So, I suggest a short description of the challenge offered by this coupling, e.g. in terms of time-step differences, and why its important.

p.818, 13-14: "The choice of the latter parameter . . ..". This sentence is awkward in this place. It ought to be shown and simply commented in the discussion.

p. 808, 24: 'Therefore, previous . . ... (Pattyn, 2008)'. To me subglacial lake simulations are those where circulation was modelled. Would it be more accurate to say that 'representation' of subglacial lakes in ice sheet models is so far limited to a viscous sliding law? Also what basal parameterisation is used in this study, if its not a viscous sliding law?

p. 809, first paragraph, the ice domain is unclear. I assume that its not the entire Antarctic Ice Sheet, but a simplified domain. A few more details would be helpful.

p. 809, 18: What is a Dirichlet boundary condition?

p. 809, 19: Should the equation have a minus?

p.809, 25: What is an 'Isothermal ice sheet geometry'? Perhaps explain that temperature evolves from an isothermal state.

p.810, 7: '. . . stiffening due to debris . . .'. Is this stiffening due to entrainment of debris? Or is it stiffening due to grounding on bedrock?

p.810, 16-17, Gaussian-type filter. Why exactly must viscosity be smoothed? Add references to others doing the same or explain further.

p. 810, 25: '. . .thermomechanically coupled full Stokes (FS) model . . ..' What makes the ice sheet model thermomechanically coupled? Suggest simply referring to it as a full Stokes model.

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Interactive Comment

p. 811, 5: I am not sure what is meant by 'isostatic adjustment'. Most people think that the surface flattens because basal friction is lost over lakes.

p. 811, 12: Does the ice slow down or does it simply accelerate less fast?

p. 811, 14: ' ... as accumulation and basal melting is neglected.' Details of these assumptions should probably feature at an earlier stage. I assume that accumulation is neglected because because the ice domain has a fixed height. More details are needed to describe the ice sheet model and underlying assumptions used in this study. Accumulation may be important because it influences vertical deformation and therefore the basal ice temperature gradient (see below for related comment).

p. 811, 16: 'freezing point is reached'. Is Tpmp reached or is it simply maintained as a boundary condition of the lake ceiling?

p.811, 18 – p. 812, 4: These two paragraphs are awkwardly placed. I suggest deleting them or referring to the results in a discussion section (see above for details).

p. 812, 15: suggest leaving out the Woodward et al. reference, if it's still an ms. in prep.

p. 812, 22. How can a 3D model be represented by 170 x 88 grid cells?

p.813, 2-10: The information of coupling is confusing here, given that a subsequent section is outlines how models were coupled. Here, it sounds like Figure 5 shows results from coupled experiments, but the caption of Figure 8 describes Figure 3 as 'initial model'. It's important to make the underlying assumptions clear (see above for related comments).

p.813,14: suggest a quick description of what the transport stream function. It's commonly used in oceanography, but readers from others disciplines may not know.

p. 813, 21: suggest shoreline instead of coastline?

p.813, 21-22. Why not try to set up a model run that includes freezing? It's after all an important part of e.g. Vostok's dynamics.

p.814, 10: the '...replace the former neglected lower boundary condition ....' Neglected here or previously?

p.814, 10-19: The word 'run' is used a number of times. I assume that 'run' refers to a number of iterations, but not a full experiment. The communication between models could be explained better here.

p. 814, 20: 'Successive initialisations...'. Why are initialisations successive or what does it mean?

p.814, 24: re-emerging nodes or simply emerging nodes?

p.815, 6:'Melting dominates freezing (which is negligible) ....' This is completely unclear!

p.815, 7-9: if the lake volume is constant, you probably need to be upfront with the fact that you don't have a mass balance. I assume that water simply disappears in the domain. A bit more information is needed.

p.815, 13-14: Why is the impact on water column height related to local melt rates? Has this got to do with how nodes are forming as the lake expands? Or is it related to ice deformation over a slippery lake surface? A few more details are needed to convey this point clearly.

p.815, 28: If vertical advection (of cold ice) from basal melting is important, then surely accumulation is too. What are the consequences of having neglected the latter?

p. 816, 3-4: "....results from the periodic boundary conditions and is not discussed here'. This is rather categorical. If there are numerical consequences of the boundary condition, then these should be explained.

p.816, 6-7: The first sentence of the summary section is a bit of an overstatement. You should explain more accurately, what was seen in the various studies (using references).

p.817, 4: How far beyond the lake is ice flow impacted, e.g. in terms of distance or an approximately number of ice thicknesses?

Interactive comment on The Cryosphere Discuss., 3, 805, 2009.