

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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- > P. Christoffersen (Referee)
- >
- > [...]
- >
- > The main limitation of this
- > manuscript is the presentation of results.

We have addressed all specific comments of the reviewer and think the presentation of the results is now easier to understand for the reader.

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> [...]

> The above examples are related to a surprising choice of structure. We still consider our choice of structure reasonable, however many textual adjustments should have increased the readability a lot. We are thankful for the reviewers suggestions, which we kept in mind during the manuscript's revision.

> [...]

> 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.

We modified this paragraph to describe the structure of the manuscript more clearly:

"In the first two sections [...].

Each section starts with a general description of the particular model, describes the applied boundary conditions, and the results of the uncoupled model runs. Section 4 [...]

discusses the impact of a coupled ice–lake system on the ice flow and lake geometry.
"

>

> I recommend
> the exclusion of figure 4.

We have removed the figures related to test of the robustness of the model, according to the reviewers suggestion.

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>
> Specific comments by line number:

>
> p. 806, 26: add to after prior
Changed

> 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".
We modified the text to
"It is still unknown whether
Subglacial Lake Vostok existed prior to
the Antarctic glaciation and if it could have survived glaciation, or
whether it formed subglacially after the onset of glaciation."

> p. 807, 4: what is a self-consistent numerical model?
We added:
"but self-consistent numerical models, which include the modelling of
the basal mass balance, are lacking at present."

> 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.
We added:
", permitting insights

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into the water circulation, energy budget and basal processes at the
lake–ice interface."

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

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

> p. 807, 21: explain why conduction cannot be ignored and
> clarify that (all?) previous models do exactly that.
The already cited study (Thoma et al., 2008b) shows the impact of heat
conduction. Hence, previous doesn't ignore this. There is nothing to
clarify.

> p. 807, 21: why does latent heat dominate conduction? Is this only true
if

> geothermal heat is not considered?
Geothermal heat and heat conduction into the ice are in the order of
10-50 mW/m². In contrast to this, the latent heat, produced by melting and
freezing, strongly depends on the slope of the interface, which determines
the rates. If we assume a rate of $m = 1 \text{ cm/a}$ the latent heat is
 $q_{latent} = \rho_{Ice} * L * m = 915 \text{ kg/m}^3 * 334 \text{ kJ/kg} * 1 \text{ cm/a} = 100 \text{ mW/m}^2$.

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

> p. 807, 26: The sentence starting with "Without this flow. . . ." is
> somewhat contrived. There will always be flow because mass has to move

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> laterally, so it's not particularly relevant to consider the case where
> there is no flow.

Here we disagree with the reviewer: Consider a valley, filled with an ice-covered lake. If accumulation is small, there will be no significant ice flow across the lake - just towards it.

> 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.

We added: "this study for the first time couples a numerical ice-flow and a lake-flow model with a simple asynchronous time-stepping scheme to overcome the problem of different adjustment time scales."

> 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.

The discussion deals with the coupling. This remark, dealing with the numerical implementation of a certain parameter belongs very well to the general description of the ice model. We modified:

"The choice of the latter parameter has no influence on the model results as long as it is much larger than zero." to clarify this.

> p. 808, 24: 'Therefore, previous . . .
> (Pattyn, 2008)'. To me subglacial lake simulations are those where

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> 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?

We mistakenly used the expression "sliding law" where "rheology" was meant. We also modified the sentence according to the reviewers suggestion: "Therefore, previous subglacial lake simulations within ice models were limited to viscous rheologies (?)."

> 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.

We added "rectangular" to clarify the idealized geometry right from the beginning before we describe the model domain details further.

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

"The Dirichlet (or first-type) boundary condition is a type of boundary condition, named after Johann Peter Gustav Lejeune Dirichlet (1805-1859). [...] it specifies the values a solution needs to take on the boundary of the domain". (From Wikipedia)

We consider the expressions "Dirichlet" and "Neumann" boundary conditions as known to most readers of an article dealing with numerical modelling, and hence do not explain them any further.

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

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Corrected

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

Modified to

"The initial integration starts 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?

The numerical model doesn't (can't) distinguish between these cases.

We left the text as it was.

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

We added the sentence

"Without this filter, the numerics becomes unstable."

- > 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.

The expression "full Stokes model" refers only to the coupled equation of motion. In contrast to a "higher order model" a FS model includes derivatives according to dw/dx , dw/dy as well as resistive stresses.

The expression "thermomechanically coupled" refers to a temperature- and strain related viscosity.

We modified the expression

"Within this thermomechanically coupled, three-dimensional,

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full Stokes ice model ..."

in the first paragraph of the "General description"-section where the relevant references to the model are cited.

- > 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.

It is true, that basal friction is lost. In the model this is a prescribed boundary condition. But the consequence of this is isostasy, as it is the case for ice shelves. We kept the paragraph as it is.

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

see detailed response to review of Fuyuki Saito

- > 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).

The assumptions are mentioned on p809,119, where the boundary conditions of this initial experiment are described. There we also added

"In the subsequent coupling-experiments, basal melting and freezing at the lake's interface, as modelled by the lake flow model, is accounted for (Section ??)."

to pronounce that the basal boundary condition change due to the

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coupling. Accumulation would disturb the interpretation of the impact of the modelled spatially varying basal mass balance at the ice-lake interface on the ice, and is therefore ignored.

> p. 811, 16: 'freezing point is reached'. Is T_{mp} reached or is it
> simply maintained as a boundary condition of the lake ceiling?
The reviewer is correct, we modified the sentence to
"over the lake the freezing point is maintained by the boundary condition. "

> 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).
We added an additional subsection heading "Robustness of the results"
for this paragraph to separate it from the context before.
We also removed the corresponding figures as they are not important for the
conclusions of this paper.

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

> in prep.
We left it in as the manuscript will be submitted when this article
finally will be published. In addition we added two more references of
manuscript which have been also (re-)submitted to peer-reviewed journals.

> p. 812, 22. How can a 3D model be represented by 170 x 88 grid cells?
we changed to text to "170 x 88 x 16"

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> p.813, 2-10: The information of coupling is confusing here, given
> that a subsequent section 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).
To clarify that the Section does not really 'couple' the models, but only
apply results from the ice model as boundary conditions to the lake model,
we reformulated the sentence and avoided the expression 'couple':
" The availability of the results of the thermomechanical ice-sheet
model permits a spatially varying Q_{Ice} ."

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

According to the other reviewers suggestion we added
"positive values indicate a clockwise circulation,
negative values an anticlockwise circulation."
to the caption. Together with the unit (m^3/s) the meaning should be clear
even to readers not familiar with oceanography.

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

> 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.
With the simplified (and artificial) gaussian-shaped lake as introduced
by Pattyn (2008) it is rather difficult to ensure that the lake does not
leak out. Either significant less water, steeper borders would be needed.

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In the initial phase of the project we tried this, but then we decided to move on. After all, the basal mass balance gradient is the dominant feature, which has an impact on the ice flow. However, future studies with more realistic geometries will deal with this issue and can, then, be validated with observations.

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

We changed the text:

"The modelled melting and freezing rates are considered to replace the previously zero-constrained lower boundary condition"

> 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.

The word run is used in

'the initial model run'

'an ice-model run'

'a specific model run'

From our point of view, it should be obvious, that 'a run' is only ONE execution of the (ice or lake) model (=binary). In particular, in combination with Fig.6, showing the coupling scheme in detail, it should be clear. To make this more transparent, we adjusted the caption of Fig.6:

"The gray lines

in the left part of the figure

represent the initial start-up sequence

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(the two initial model runs), dotted loops indicate cycles
(successive model runs)

that may repeat an apriori unspecified number of times."

We also added the following paragraph at the end of section 4.1:

" In Section 2.4 and Section 3.3 the results of the initial runs of the individual models have been described. So far the only parameter exchange between both models was the uni-lateral initialisation of the lake flow model ROMBAX with the modelled geometry and temperature gradient of the ice flow model RIMBAY (indicated by the gray lines in Figure 7). The real coupling procedure starts, when the results ROMBAX are reinserted into RIMBAY (indicated by the black lines in Figure 7)."

"

> p. 814, 20: 'Successive initialisations. . .'. Why are

> initialisations successive or what does it mean?

We don't understand the reviewers question. An 'initialisations' of a numerical model is how the variables are set right at the beginning, before the integration starts. And according to the description and Fig.6 the individual lake- and ice-models are started more than once, hence the expression "Successive initialisations" refers to initialisations that are done for successive individual model runs. To clarify this, we modified:

"In each coupling cycle, successive initialisations of the lake-flow model are performed with ..."

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

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Changed

> p.815, 6: 'Melting dominates freezing (which is negligible) . . .' This is completely unclear! The full sentence (without the bracket) is "Melting dominates freezing and hence the ice sheet is losing mass." From our point of view this should be clear: If melting is larger than (=dominates) freezing the ice sheet is losing mass.

> 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. To clarify this we added "This mass imbalance can be interpreted as a virtual constant water flow out of the lake without any feedback to the ice sheet."

> 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.

In the related paragraph we don't discuss nodes at the lake's edges. We only refer to "the center of the lake [...] slightly shifted to the upstream side". In Section 4 (Coupling) we described in detail, that the lake geometry is derived from the result of the ice flow model. Hence, if

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the basal mass balance changes (from zero in the initial ice model run to melting in the order of 10 mm/a) the ice thickness reduces. Because of the spacial variance of the melting and the constant lake volume, the ice slope adjusts its tilt: Where more ice melts, the water column increased, where relatively less ice melts, the water column decreases. We slightly adjusted the text:
"Consequently, the water column thickness increase where most ice is molten and decreases where less ice is molten (Figure 8b)."

> 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? The reviewer is correct, advection of accumulated cold ice will matter. However, this study deals only with the coupling of a lake model with an ice model and its impact on the basal and internal conditions. The impact of changing surface boundary conditions lies beyond the scope of this paper, but will be studied in successive studies.

> 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. This study deals with the interaction of an ice sheet with a subglacial lake. Numerical artifacts produces in great distance of the lake don't influence the results of this study. We added: "is not discussed here, as these numerical artifacts don't influence the results of the ice-lake interaction in the center of the model domain

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studied here."

> 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).

We added 'large' and 'as they flatten the ice sheets surface.'
as well as some 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?

We added the following statement to the summary:

"The ice flow [...]

This impacts on the velocity can be traced about 75 km up- and downstream
of the lake in this specific setting, which corresponds to about
20 times the ice sheet thickness."

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