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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.

M. Thoma et al.

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> This work is concerned with the coupling of an ice dynamics model with a
> lake dynamics model, that was adapted from an ocean model. The basic idea
> is to improve the estimation of the heat transport from the lake into the
> ice and thus obtain a basal melt rate at the lake/ice interface. This is a
> very valuable goal. However, the work is in not mature to be published in
> this form. It is not explained which equations are additionally solved in
> the ice model, or which boundary condition was chosen. The entire study
> misses a validation and some results are questionable. At some occasions,
> it remains unclear if the assumptions are in fact fulfilled (the lake volume
> is supposed to be constant, while the area might vary, whereas the vertical

> extent does not change). The discussion of the results of the coupling does
> fill only one page, which symbolizes quite well that the entire study is in
> a very early stage and needs some extension, even if it is only applied to a
> simplified geometry. The only 'physical experiment' is the influence of the
> coupling after 100.000 years - a question worth to ask, but the estimated
> effect of melting is dubious. The figures of this manuscript are
> impertinent and definitely need to be improved. Concluding this seems to be
> a documentation of the derivation of the process of improving and coupling
> two models (which is a great amount of work that every modeller
> acknowledges), but it is not a study of the 'Interaction between ice sheet
> dynamics and subglacial lake circulation' as yet only compares 'uncoupled'
> versus 'coupled' modeling.

We addressed all specific comments of the reviewer very carefully.
These should include the following concerns repeated in this summary:
- equations
- boundary conditions
- validation
- questionable results
- lake volume / lake area
- figures

The revised manuscript benefits from all comments of all
reviewers. We hope, our detailed answers to each specific comment satisfies
the reviewer's expectations.

The reviewer is correct, if he states that the manuscript compares the
results of stand-alone ice-flow and lake-flow model runs with coupled runs.
However, from our point of view this is the meaning of the expression

"Interaction", hence we still consider the title reasonable.

> Specific comments:
>
> Page 807:
> Line 15: Schiermeier, 2008 is not a scientific publication and it
> is questionable if news are supposed to be cited this work.

We disagree with the reviewer. Although Schiermeier (2008) is not a scientific article, it ratifies our statement "Several plans to unlock subglacial lakes exist" and provides evidences for suchlike planes by citing the head of the Russian drilling team.

> Entire page:
> This study is partly concerned with the ice dynamics and how the ice flow
> velocity changed while the ice is flowing over the lake. There are studies
> which estimated the flow velocities over some subglacial lakes. The authors
> shall cite this work and use the data later on to compare their estimated
> velocity changes with the measured ones.

All numerical ice sheet models (we are aware of) use a so called 'enhancement factor' or 'tuning parameter' which is used to adjust modelled velocities to observations by assuming all non-resolved physics can be compiled in this one single scalar.
Hence, comparing the velocities of our idealized geometry with real measurements is pointless, as they could be tuned to any desired values.

> Page 808:
> Line 9: Frank Pattyn
> still needs to be in capitals, and it is probably not based on the person

> Frank Pattyn, but on his ice model.

Corrected

> Line 11: full-Stokes (check everywhere)

Corrected (everywhere)

> Line 12: Is beta really vanishing entirely? Or is it some kind of
> epsilon-style value, which stabilizes the numeric's?

No, it really vanished to zero as stated. Due to the model improvements no 'epsilon' value is needed.

> Line 11-13: These lines contradict Fig1 and page 810, line 4-23.

We don't understand where the reviewer sees a contradiction:
This section is the "General description" of the ice flow model RIMBAY, within a vanishing β defines a frictionless lower boundary condition, and hence represents a subglacial lake. On page 810, the "Model improvements" are described. One of these improvements is the possibility of a gentle transient adjustment of β at the grounding line.

> Line 20-24: It remains unclear which value you chose and if
> Pattyn (2008) does not count as a traditional ice model.
> Reword this into something like: There is a general
> agreement to use n=3, while some models (e.g. Pattyn, 2008) use n=1 in
> order to ....

The expression "ice models traditionally assume $n = 3$" is in agreement with the reviewers suggestion "There is a general agreement to use n=3". Again, we don't see any need in the 'General description' to describe our specific parameter choice. On p.810,l.4 (already cited by the reviewer in the last comment) we explicitly explain that we use $n = 3$.

> Page 809:
> Line 2: 168100km2 LARGE domain.
Corrected

> Line 11: Figure 3 does not show the lake geometry in a way that one can see
> where the lake thickness is zero. Chose a color scale that shows a clear
> difference between vanishing lake thickness and a non-zero lake depth.
Color scale adjusted

> Line 12: If this simulations wants to assemble lake Vostok,
> -50°C is an odd choice.
We appreciate the reviewers suggestion to use a value closer to Lake Vostok's 'real' average surface temperature, which according to (http://www.nerc-bas.ac.uk/icd/gjma/vostok.temps.html is about -55°C. However, we never claim to assemble the 'real' subglacial Lake Vostok. In that case we would have to adjust the geometry first. The specific surface temperature has no impact on the results of this study which is based on an idealized configuration.

> Line 15: The motivation of the choice of the geothermal heat flux is quite
> strange. The choice of the value should be motivated by datasets, however
> sparse they might be. Fox Maule 2005, ShapiroRitzwoller, 2004, are two
> sources.
Again the reviewer suggested to use 'real' data, while 'reasonable' data is sufficient for idealized model studies like this. However, we can ease the reviewer as the applied value (54 mW/m^2) is the one suggested for the Lake Vostok region by Maule et al. (2005)

We included this reference.

> Line 20: are ignored
Corrected

> Entire section 2.2: It remains unclear if the
> coupling between ocean and ice model uses the heat conduction and convection
> equation or solves the energy balance at the ice/lake interface. It is
> mandatory to show this or these equation(s) and discuss the magnitude of the
> terms. It also remains unclear what the boundary condition is across the
> lake: for ice and ocean models usually assumes the temperature at the
> ice/water interface to be the freezing point of (sea) water. The choice of
> this boundary condition is critical and you should definitely discuss this
> more in detail.
Section 2 describes only the ice model. The exchange (= the coupling) of boundary conditions is described in great detail in Section 4. This is also mentioned in the last paragraph of the "Introduction". We reformulated a sentence to pronounce this:
"Accumulation and basal melting/freezing are ignored during the initial experiments in this section, where no coupling is applied.
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 **??**).
"
Although the reviewer would appreciate to read some equations, we don't see any reason to repeat already published and cited equations in this manuscript. The 'coupling' applied in this work doesn't solve any additional equations, it merely exchanges two-dimensional fields between models

involving coordinate transformations and regridding. This process is described in great detail in Section 4.
The equations for the energy balance at an ice/lake interface are described in e.g., Holland and Jenkins (1999), cited on p812,l11.
The role of heat conduction for the lake flow model has been discussed in detail in Thoma et al. (2007, 2008b) and also described in the (revised version of the) Introduction:
"However, current knowledge about the interaction between subglacial lakes and the overlying ice sheet is lacking. The most important parameter exchanged between ice and water is heat. The exchange of latent heat associated with melting and freezing is in the same order of heat conduction, both processes have to be accounted for in subglacial lake modelling (Thoma et al., 2008b)."
A discussion of the magnitude of each term would be pointless.
Although the net basal mass loss within subglacial lakes is (mainly) determined by the difference of the geothermal heat flux and the heat loss through the ice, the circulation as well as the basal melting and freezing pattern are (mainly) determined by the steepness of the surface slope. In any case, the latent heat released by freezing and consumed by melting is in the order of one magnitude larger than the heat conduction:
$q_{latent} = \rho_{Ice} * L * m$
$915 kg/m^3 * 334 kJ/kg * 1 cm/a$
$0.097 W/m^2$
$or 100 mW/m^2$ per 1 cm/a melt/freeze
compared to this value heat conduction (30 mW/m^2) is very small.

> Page 810: I suggest to shift this section one level up and
> first explain the new model and its substantial changes to Frank Pattyn's
> original model (You claim a new name, thus a kind of new product, so it's
> presumably a rather large addition? Is the implementation of a Gaussian
> filter a smoothing coefficient an addition that justifies that?) and then
> discuss the model setup. This also solves my question about n=?.

The model improvements are neither part of the boundary conditions nor the general description. Hence, from our point of view an additional section is justified. For easier description a model should always have a name. The original model, published by Frank Pattyn, doesn't have any name. Although, RIMBAY has the same physics as the original model, the code was partly rewritten to structure the code strictly. In addition, a version control system, an automake-autoconf environment, an netcdf-in-output interface, and a graphical GMT-output was introduced. All these changes make the code much more handable, and extendable for future usage. From our point of view, this model version justifies a 'name', where the original author is acknowledged.

> Line 9: If the numerical runs are not stable without this
> stabilization, is Fig. 2a meaningful?
The reviewer is completely right. All parts of Figure 2 were produced from a model runs applying the shallow ice approximation until a quasi steady states is reached. After that, a short(!) FS-integration was performed. Hence, the figures show images from the initial conditions for the FS-model.
The effect of the Gaussian filter is represented, and the figures are very well comparable. We added

"Note that the Figures represent the initial conditions for
a FS-model with the applied filter and β^2–smoothing."
to the caption to clarify this.

> Line 12: time increases without
Corrected

> Line 21: Do you really mean
> that your result that your remedies lead to stability is only a preliminary
> result? If your study is a preliminary study, you should consider to
> re-submit at some point in future, when the results are not preliminary any
> more.
The reviewer misunderstood the meaning: Neither the stability is
preliminary nor the study is preliminary. But preliminary studies, performed
before the presented ones, which weren't published, have shown ...
To clarify this we dropped 'Our'.

> Line 23: Please tell the reader your convergence criterion.
We don't understand the reviewers point: 'numerical stability' has
nothing to do with a 'convergence criterion' here. Instead 'numerical
stability' has the meaning of 'the model doesn't crash.

> Line 26: If quasi-steady state is reached after 300 000 years, why do you
> show in Sect 4.2 the results after 100 000 years?
The reviewer mixed up the 300 000 years applied for the initial
(uncoupled) ice model run (in this section) and the 100 000 years
applied for the coupled model (Section 4.2), which only differs
by slightly changed basal boundary conditions and hence, reaches a new quasi
steady state much faster.

To clarify this difference, we added the following sentence to Section 4.2
where the results of the coupling are described:
"The coupler RIROCO applies restarts from previous model runs. Consequently
the models reach their new quasi-steady state after a significant shorter
integration time."

> Page 811:
> Line 5: I see it in Fig. 3b, but not in Fig. 3c.
Both figures show identical geometries. However, a better view of this is
> presented in the new Fig.5.

> Line 10: 0.7 to 1.2m/a equals 71Typo corrected

> Line 13: This is a cross-section through the temperature field. Please
> note: the most interesting quantity is the vertical gradient of the
> temperature. I recommend to include a panel showing the temperature
> gradient between all vertical layers for all nodes in that cross-section.
We don't see why a figure of the 'gradient' of a variable should contain
more information, than a figure of the variable itself.

> Line 17-24: You conclude that the model resolution does highly influence the
> ice flow velocities. If so, you need to PROOF that 1km, 500m ...
> resolution will not again have a significant impact!
The reviewer is correct. We run an additional model with a higher
resolution and adjusted the text as follows:
"To investigate the impact of the horizontal resolution on the model results,
simulations with a coarser 10 km as well as a finer 2.5 km grid have
been performed.

..

The finer 2.5 km model resolution needs a much longer integration time, without producing significant differences in the results compared to the intermediate 5 km grid resolution.
"
We removed the corresponding figures as they are not important for the conclusions of this paper.

> Line 25: (HOM) is not required.
Corrected

> Line 27: You NEED to tell the reader, if the set-up of the higher
> order model is similar to this model and if there are differences in e.g.
> the definition of the rate factor and temperature boundary conditions ...
> otherwise we can't estimate if the differences are representing 'stabilized'
> full-Stokes vs higher order model or different choices of model parameters
> and setups.

Of course, the set-ups for the HOM- and the FS-model are identical in all aspects, otherwise any comparison would be useless. To address this important point for the reviewer, we added:
"All other aspects of the geometry as well as the parametrization are kept identical.
"

> The entire section misses to discuss and compare measured flow speeds.

There are no "measured flow speeds" for this idealized model geometry available. Even for Subglacial Lake Vostok, there is (up to now) no

ground truth velocity measurement available, which could indicate the velocity change from grounded to floating ice.
Tuning with the 'rate factor' would allow (nearly) any desired velocity.

> Page 813:
> Line 4: Since you do not show any equations it's quite
> confusing what your Qice really is. I first thought it's the heat flux into
> the ice that is usually calculated by ocean models, but this quantity is
> most often called q_sea. Is $Qice = \kappa(gradT * \vec{n}) = q_sea - rho_iceLa_b - (rho_sw - rho_ice)gh_ba_b$?

We wrote that Q_{Ice} is the "prescribed average heat conduction into the ice". This appears in the lake-flow-model section, where the boundary conditions are described. The references also indicate where further information about the "heat conduction into the ice" can be found. We assume, that the expression dT/dz in connection with the terms "borehole temperature measurements and thickness temperature-gradient" is sufficient. We altered the text in this respect.

> Line 9: Fig3c shows the temperature and not the temperature gradient. As
> one of achievements of this work is indeed the treatment of Qice, please
> give the reader the value for Qice at locations similar to those where you
> evaluate 24 27 mW/m^2.

We don't understand the reviewers concern, we wrote:
"The coupling to an thermomechanical ice-sheet model permits a spatially varying Q_{Ice} [...] from [...] 27 mW/m^2 [...] to [...] 24 mW/m^2
..
[which] results from the modelled temperature distribution in the ice
..
".
It should be obvious, that the heat conduction pattern shown in Fig.5a

is derived from the Temperature shown in Fig.3c.

> Line 1718: Could you please be so kind to tell us the magnitude (e.g. as
> number in brackets) of the mass transport for these two lakes, which would
> allow us to guess how close to the sophisticated lake Vostok the lake in
> this study is.

The values for the vertically integrated mass transport stream function for Lake Vostok (Order 10 mSv/m^3) and Lake Concordia (Order 0.1 mSv/m^3) can be found in the cited reference. However, the mass transport is determined by the geometry (in particular the water depth and the slope of the ice-lake interface) and hence, a comparison between the value of this idealized lake and a 'real' geometry is pointless. We only intend to indicate, that the model geometry produces results within the range of more realistic model model geometries. To clarify this, we added: "The strength of the mass transport is between those modelled for Lake Vostok and those for Lake Concordia (?), and hence reasonable for subglacial lakes.
"

> Line 20: 'results' - does this really result in a decrease
> of **, or is the slope a result of the melting/freezing or is this some kind
> of hen/egg problem?

No, it is not.
The ice flow over a subglacial lake maintains the imbalanced ice-lake interface. Without the ice flow, the interface would even out. Hence, the sloped interface as a consequence of the ice flow and the basal mass balance pattern is a consequence of this. This is also indicated by this study (but not mentioned as it was assumed to be trivial

so far): In the initial (uncoupled) ice model run, there is no basal mass balance, but the sloped interface is formed anyway (because of the ice flow). We added the following sentence to the results of the ice-flow model (Sec.2.4):
"The inclined lake-ice interface is maintained by the constant ice flow over the lake and hence, independent of a possible basal mass exchange."

> Page 814:
> Line 13: The use of the initial values might
> be justified here, as the flow speeds are small and you ignore snow
> accumulation anyway, however this is the wrong design for the coupler, as
> you may want to apply it to locations where all that is not longer valid.

The reviewer is wrong. The ice flow of a numerical model is determined from the geometry and the boundary conditions. The final result is independent of the inital values as long as the system has no multiple equilibria, which we do assume. However, the closer the initial values are to the final ones, the faster the model converges. Hence, it is not a 'wrong design' but a feature.

> Line 15: An additional process has to be considered or IS considered? Below
> you write that the volume of the lake is constant. To me this seems to be a
> major inconsistency.

'has to be' is correct.
There is no inconsistency: Although the lake VOLUME is constant, the AREA may change. For a given volume a tilting of the ice-lake interface can change the area very well.

> Line 26: re-use
re-use and reuse are both known in dictionaries. We left this to the editorial office.

> Page 815:
> Line 9: Check the lake volume and convince the reader that you
> assure this definition.
The lake volume is PRESCRIBED in the ice flow model:
"[...] the lake's volume [...] is constant per definition." (p815,8)
Due to coordinate transformation the lake volume may change in the lake-flow model, but this change is less than 0.002statement doesn't require any additional proof or convince.

> Line 10-12: Your snow accumulation rate is still
> zero. Assuming steady state, the basal melt of about 12mm/a, would
> accumulate in 100 000 years to 1200 m (assuming that the flux terms are only
> small)?? How meaningful is the impact on the ice sheet volume anyway, as
> you neglect the snow accumulation?
The reviewer is correct, if the ice sheet would not move, 1200 m of the ice's thickness would be molten away after 100000 years. However, as the ice moves across the lake, which covers only a fraction of the model area and because the upstream ice thickness boundary condition is fixed, the total amount of ice loss is significantly less.
We don't understand the reviewers second question. This is an idealized model study, dealing with the impact of a spatially varying basal mass balance at the ice-lake interface of subglacial lakes. Hence, surface

accumulation would disturb the interpretation of this impact and is therefore ignored.

> Line 18, 22 26: I'm not entirely sure,
> but I think this should be Firstly, Secondly and Thirdly,...
As we are no native speakers we leave this to the copy-editing office.

> Page 816:
> Line 1: This reduced temperature (compared..) - this is not an anomaly.
Corrected

> Line 3: This is not an artificial cooling, this is 'the reality'.
With respect to the 'real' cooling above the lake, due to 'real' bottom melting, the cooling at the periodic boundary conditions is 'artificial', as it is introduced solely by the numerical representation and not the 'real' boundary conditions. We adjusted the text, to make this clear.

> Line 6: I doubt that OBSERVATIONS FROM SPACE show that subglacial lake have
> a significant impact of the dynamics of the Antarctic ice sheet. As far as
> I know remote sensing shows the presence, the location and the dynamics of
> the subglacial hydrology, while you need in situ measurements to show the
> effect of subglacial water on the dynamics of the Antarctic ice sheet (like
> the estimation of flow speeds before, during and after drainage of a lake).
> If you disagree, be more specific and give references.
We disagree indeed. The southward deflection of ice flow across

subglacial Lake Vostok can very well be identified from satellite images.
We added references.

> Line 9: Give citations for the observed re-direction!
see above

> Line 15: No, the lake-exclusive studies are based on a model and not
> vice versa.
We reformulated the sentence:
"This model, previously only applied to lake-exclusive studies,
now receives its geometry directly from the ice flow model."

> Line 16: ...while you didn't change the lake volume???
We don't understand the reviewers suggestion: The lake volume is part of the ice-flow model. When we state: "This [lake-flow] model [...] receives its geometry directly from the ice flow model." This implies that the lake volume is also 'received'. We adjusted the text:
"This model, previously only applied to lake-exclusive studies
with a prescribed ice thickness and bedrock,
now receives its geometry directly from the ice flow model."

> Page 817:
> Line 1: The ice sheet bottom IS the
> bedrock-based ice. What exactly do you mean?
The subject of this sentence refers to the previous sentence dealing with the "ice flow on top of the lake". However, to clarify this, we added:
"The temperature at the ice sheet bottom on top of the lake is at the

pressure melting point, ..."

> Line 6-8: Yes, obviously.
> Delete this sentence or write about plans etc..
We modified the last paragraph:
"Our idealised configuration of an ice-lake system indicates an important impact of the interaction between both systems on the ice sheet's dynamics. Next applications of this new coupled model will focus on realistic configurations, such as Lake Vostok and its glacial drainage system. In order to investigate the flow dynamical impact which can be compared with observations."

> Figures: All figures : annotations are too small.
When we prepared the latex-manuscript we used the provided copernicus.cls, which makes a good usage of A4 in a two-column style. One-column figures are 18cm wide. The so called 'printer-friendly version' reduces the width to 11cm. This implies a reduction in size of the provided figures by nearly 40original size (as from the documentstyle provided by Copernicus Publications) all figures are of reasonable size.
We acknowledge the reviewers request, but would like to pass this issue to the publisher and strongly suggest to revise the 'printer friendly' version.

> Figure 1: y-axis: $\beta^2 * 10^6$.
No, the annotation contains 'Scaled', while 10^6 would be 'unscaled'.

> Figure 2: The viscosity probably has a unit.

> The yellow of beta^2 seems not to appear in the color bar, otherwise it
> would be bright yellow and not pale yellow. Chose a color scale which
> clearly marks beta^2=0 and beta^2=10^6.

We checked the rgb-values of the colors in the pdf with gimp. According to this, the colors are correct.

> The color bar of log(eta) is shifted to the left in my printout.

This is most probably an optical illusion.

> Figure 3: Adjust the color bar in a
> such that we see where the lake depth is zero.

Corrected

> The magnitude 1.2m/a is
> reached nowhere. Chose a vector which eases the measuring of the length of
> the arrow (which one can't anyway, so you might want to show
> the direction with arrows of similar length only?)

We are not sure what the reviewer would rate at 'ease'. The vector scale is chosen such that it is possible to estimate the length and direction of the ice flow approximately. A scale of 1.0m/a (which may seem better in the first instance) would lengthen the arrows and would make a reasonable estimation even harder. We further don't understand what the reviewer means with 'show the direction with arrows of similar length only'. The figure clearly indicates that the flow lateral to the lakes center is significantly reduced. If all arrows would have the same length it wouldn't be possible to see this feature.

> b) Velocity in x-direction. Are these contour
> really need to be labeled?

We improved the color scale and think that the labeling improves the readability of the figure.

> c) Add the temperature gradient from layer to
> layer or at least a plot like a showing the gradient at the bottom. It's a
> cross-section through the temperature field.

The reviewer is right, it is the temperature field shown. We never stated otherwise. We also don't understand why from the reviewer point of view the gradient is so much worth showing than the temperature itself.

> Figure 4: What's that line above the color bars?

It was a typo in the graphic generating bash script, corrected.

> Figure 5: We are all no graphic designers, but this
> figure matches only the lowest level of aesthetics.

We do respect the reviewers wish for aesthetic figures. However, there are some point to consider before condemning them. We guess the reviewer doesn't like the wiggles in Fig.5a? These are results of the coordinate transformation during the coupling process. They may not be very 'aesthetic', however, they are real and are applied to force the lake flow model.

> (a) Label it as Qice.

Added to the caption

> I can't see a value that is larger than 30mQ/m^2 and you even write in the
> text 24-27mW/m^2. How about adjusting the range?

The reviewer is correct, the color scale covers a wider range than needed in this figure. However, we decided to keep a wider range because it is reasonable to have identical scales for Fig.5a (showing the heat conduction WITHOUT coupling) and Fig. 7d (same value WITH coupling). But we already revised the figure.

> (a-d) can you read the
> contour labels without zooming in the pdf file to 300%?

We are sorry, that the size of the figures was reduced so much by Copernicus Publications and that we didn't check the 'printer friendly version', before we agreed to the publication within TCD. We will take care for improved figures during the copy editing process if the manuscript should be accepted.

> Figure 6: You ruin our eyes! Choose rectangular boxes instead of
> triangles and increase the font size!

The triangles are meant to indicate the 'input flow' for the individual numerical model. A few centimeters are left on each side of the figure, we will monitor their usage during the copy editing process, which will increase the letter size.

> Figure 7: The labeling of the contour lines makes it really
> difficult to see if panel d shows something at the margins of the lake!
> Qice instead of heat conduction.

We added (Q_{Ice}) to the caption and improved the contours.

> Figure 8: I'm surprised that the
> temperature in the lowest part of the ice sheet across the lake has not
> changed!! I doubt that. Authors, please clarify!

The lowest part of the ice is per definition of the boundary condition at

the pressure-dependent freezing point. According to p809,l19, the vertical Temperature is $T_b = H * 8.7x10 - 4$°C/m. According to Fig.7b the ice thickness is reduced by about 7 m. Hence, the temperature difference of the ice-lake interface is decreased by about 0.00696°C. This value is not resolved in Fig8b. We added
"The lake-ice interface itself is cooled by less than 0.007°C,
as it is maintained at the pressure-dependent freezing point."
to the last paragraph of Sec.4.2.

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