The Cryosphere Discuss., 6, C3073–C3083, 2013
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The Cryosphere Discussions

Interactive comment on "A balanced water layer concept for subglacial hydrology in large scale ice sheet models" *by* S. Goeller et al.

Anonymous Referee #3

Received and published: 26 February 2013

The submitted paper "A balanced water layer concept for subglacial hydrology in large scale ice sheet models" by Goeller et al. is an interesting application of a coupled model of ice dynamics and subglacial hydrology. The study uses a balance flux description of subglacial water flow modified to allow the formation of subglacial lakes and a shelfy-stream approximation to ice sheet flow. The model is applied to a synthetic domain meant to loosely resemble a mountainous region of Antarctica and is able to qualitatively produce dynamic features associated with ice streams and lakes.

This paper helps fill a gap in existing literature regarding coupled subglacial hydrology and ice dynamic models. While neither the hydrology model nor the ice sheet model are particularly novel, the coupled application is promising. However I find a few major flaws: the description of hydrology and ice dynamics is somewhat weak with regards to current literature, and the implementation of the sliding law makes a number of questionable assumptions that are poorly described. I describe these issues in detail below, followed by a longer list of minor comments.

—Major Concerns—

1. Background on subglacial hydrologic models

line 11: The way this sentence is written it reads like the authors find four possible water flow regimes. It should be reworded to make it clear this is a review.

lines 11-15: This paragraph should acknowledge the fundamental difference between ‘fast’ and ‘slow’ categories of flow regimes (e.g. Fountain and Walder, 1998; Hewitt, 2011). This study only addresses the ‘slow’ (or distributed or inefficient) category and that limitation should be stated. Furthermore, if this list is meant to be an exhaustive list of possible modes of subglacial water flow, additional mechanisms could be added, e.g. canals eroded into sediment (Walder and Fowler, 1994), flow within groundwater/till (Alley et al., 1986).

line 16: Similar to the last comment, it should be acknowledged that the idea that the hydropotential is a direct function of the ice thickness is only a good approximation for distributed flow – it is not a good assumption for efficient, channelized flow. lines 24-26: These recent hydrologic studies should be acknowledged here, but describing them as providing ‘water-pressure-dependent transitions from one water flow regime to another’ is somewhat inaccurate. The major advance of these models is to close the mathematical description of the system and actually diagnose water pressure, which allows the use of water pressure-dependent sliding laws. Only some of the papers cited describe the transition between modes of drainage ((Schoof et al., 2012) does not), and that transition is dependent on water flux not water pressure. Finally they do not describe transitions between the specific flow regimes the authors have listed in the previous paragraph, but instead describe general distributed and channelized drainage without a specific physical mode of drainage implied.

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lines 1-10: I don't disagree with this paragraph in general, but I think it should be toned down. High-resolution modeling of hydrology will of course be computationally expensive relative to coarse-resolution hydrology, but it may still be cheap relative to the cost of higher-order ice sheet models. Additionally, one could model the hydrology at higher-resolution than the bedrock topography data available.

lines 11-25: The distinction to this approach relative to the previous paragraph is that these models only include distributed flow (no channelized flow) and it is unable to describe water pressures on its own (but an assumption is made that it is equal to ice overburden pressure). The fact that it assumes steady-state is important, but the more sophisticated models described in the previous paragraph can also assume steady-state if desired. The distinction that this method cannot provide water pressure is important to be clear about.

line 15: the word 'latter' is confusing here.

lines 17-19: I think it would be useful to readers to explain specifically why these methods lack mass conservation. (Mass is lost because internal sinks exist at local minima of the hydropotential surface.)

2. Sliding law implementation.

a) I am concerned about the implementation and description of the sliding law used in the model. The description is confusing. First of all, an equation describing the SSA momentum balance should be included so it is clear where Taub fits in. Secondly, and more importantly, why is Taub defined as the driving stress in equation 2 if a SSA model is being used? I suspect what is meant is that Eq. 2 is used as an approximation for Taub in Eq. 1 to reduce the nonlinearity of the momentum balance with this sliding law, but there is nothing in the text to clarify that Taub in Eq. 1 is an approximation but it is solved for in the SSA momentum balance. If my interpretation is correct, then some notation should be used to distinguish the approximate Taub (in Eq. 1 and 2 and add a statement in the text stating this assumption is being made) with the calculated Taub (in line 25 on page 5229 and an equation for the SSA momentum balance to be added). Finally, and perhaps most importantly, how good is the assumption of using the driving stress for the basal shear stress within the sliding law? It certainly seems possible to use the calculated Taub in Eq. 1, using a fixed-point iteration in solving the momentum balance. Alternatively, there should be an assessment of how much the final calculated value of Taub differs from Eq. 2 for the types of problems used in this study. Since sliding is proportional to the cube of Taub, this assumption could lead to locally large errors in sliding speed on the 2km grid used.

b) I also have concerns about the implementation and description of the sliding rate C (Eq. 3). Is there a source for the form used (eq. 3)? I recognize that modeling sliding dependent on hydrology is not a mature area of ice dynamics, but this description is presented as if this form is standard or self-evident, which it is not. The use of the term 'first-order approximation' implies the magnitude of the formal error, but the form of this relationship, or even what the independent variables are, is far from settled. I suspect what is meant is 'physically plausible'. A few additional sentences are needed to explain where this equation comes from. I don't necessarily think it is a bad choice, but it needs an explanation. Why, for instance, is the sliding rate chosen to be dependent on water flux, and not water layer thickness as is done in some other balance-flux applications (e.g. Le Brocq et al., 2009)? Again it would be worth pointing out that water pressure/effective pressure is likely to the more appropriate coupling variable (e.g. Clarke, 2005; Paterson, 1994, many others), but the hydrology model used requires using something else.

c) Finally, the exclusion of frictional basal heating term from Eq. 5 is troubling. Without this term present the coupling between the hydrology and ice dynamics is very weak – changes to the melting rate can only happen as the temperature profile changes (very slow adjustment) and changes in ice thickness adjust the hydropotential. The argument that the SSA will overestimate the sliding velocity is not convincing – if the overestimation is large (i.e. an area where sliding is small), then the SSA is not an appropriate approximation of the flow anyway and some other flow model should be used (e.g. FO, L1L2). For the types of problems shown in the results (formation of ice streams) the frictional basal heating is expected to be important and perhaps dominate over the over terms in Eq. 5, so its absence severely limits the realism of the simulations. I recognize that including that term may lead to instability (blow-up or oscillations), and perhaps that is why it is not retained. However, it would be a useful contribution to explain what sort of behavior the model exhibits. I think the paper would be strongly strengthened to include that term and ideally either 1) use a momentum balance that is appropriate for the experiments run or 2) change the experiments to be appropriate for the momentum balance approximation used. Barring that, examples of model behavior when frictional basal heating is included would help justify why this type of simplification is necessary at present and point to areas of needed further study.

—Minor Comments—

Section 1

p5226

line 15: there is an extra comma

line 25: 'acceleration of . . . ice velocity' is awkward. Consider removing the word 'velocity'

line 26: The use of the word 'streams' for both flowing ice and flowing water in the same sentence is potentially confusing. I recommend removing it from describing subglacial water features. Also, the presence of water is only one factor in the formation of ice streams – thick ice and the presence of soft sediments are also important factors (e.g. Alley et al., 2004).

p5227

line 3: ‘imperative necessity’ is redundant.

line 9: unnecessary comma

line 24: ‘what crucially affects the ice sheet dynamics’ is awkward. Consider rephrasing.

line 28: the word ‘preceding’ here is ambiguous. Perhaps ‘additional’ would be more appropriate?

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line 9: ‘simplify’ might be a better word choice here than ‘clarify’

Section 2

* It would make more sense to fully describe the model equations before the implementation of the boundary conditions.

page 5229

line 16: the ‘conventional’ SIA is not that conventional any longer. There are very many recent examples of ice sheet modeling studies using SSA, higher-order, and full Stokes methods (e.g. Larour et al., 2012).

lines 21-23: the description of the coupling between hydrology and ice dynamics is confusing – it sounds here like the adjustment to the basal ice elevation is the only coupling, but that is not the case (as is described in the following pages).

Section 3

p. 5231

line 21: ‘were’ should be ‘are’

Eq. 7: This is a time discretization, but it would be helpful to see the differential equation first (given this is a ‘General formulation’ section. Should there be a flux divergence

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term (e.g. eq. 19) included to allow water to move around as is described in the text (line 6, p 5232)?

p. 5232

line 9-10: the method used for RIMBAY should be in the previous section

p. 5233

Can you define the units for model variables? (either in the text or in a table)

p. 5234

line 21: no comma needed

p. 5235

line 11: no comma needed

line 19: ‘According’ has an extra ‘c’

Section 4

p 5236, line 8. Why are 21 vertical layers used in an SSA model? Is this just for the vertical temperature diffusion calculation? Are they evenly spaced?

line 10: Gamburtsev Mountains are in East Antarctica.

line 16: ‘looses’ has an extra o.

line 17: ‘cover’ should be ‘over’.

line 18: If I understand the model formulation correctly, the hydrology is brought to steady-state with the geometry on every time step. It would be helpful to say that explicitly in section 3 and clarify that here.

* The organization of this section is confusing. First of all, the two Budd and Warner methods should be described prior to the results section, with a clear explanation (table

of all 5 model setups?) of how they differ from the balanced water layer method. It may also make sense to present the models in order of increasing complexity, e.g. Control run (no hydro model), One-way coupling with lakes, Balance-flux method with two-way coupling (BWA), Balance-flux method with filled depressions and two-way coupling (BWA), Balanced water layer method with two-way coupling. Finally, the terminology 'initial state' for the experiment in section 4.1 is somewhat confusing. I assume what is meant is the final (t=20,000 yr) state from this experiment is used as the initial condition (t=0) for the other four experiments. However, when the word 'initial' is used subsequently there is ambiguity about which t=0 is being referred to.

* Are appropriate significant figures used for values reported in this section? p. 5237

line 9: delete 'so far'

line 10: delete 'instead'

line 11: is 'insolates' meant instead of 'isolates'? section 4.2: this experiment may be better described as 'One-way coupling' or 'One-way coupling with lakes'

p 5238

line 4: the word 'a' is not needed

line 13: 'branching' might be a better word choice than 'branchy'

line 18: a single 's' is the preferred spelling of 'focused'

line 20: remove the word 'still'

line 21: comma not needed

p5239

line 5: no comma

line 8: the phrase 'from the beginning' is awkward here.

line 10: It sounds like ‘the significantly bigger part’ of 0.0857 km3 a-1 is being referred to here but I think what is meant is ‘significantly bigger part (0.0857 km3 a-1) of the total flux’

line 23-24: Is this shown in a figure?

line 27: no comma needed

p. 5240

line 1: first comma is not needed

line 8: first comma is not needed

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line 19: This statement seems self-evident and could be removed.

line 24: see comments about hydrology models above. Also the models reference need not be high-resolution, but they are more physically based.

line 27: The flux-friction coupling is only partial since basal frictional heating is neglected (see comments above)

Figure 6. It would be clearer if each subfigure had its own legend.

Figure 5. It might be interesting to identify the location of lakes on one of these panels.

Figure 7 (and p. 5240,line 6): Are these difference from the initial condition at time 0 or the control run?

—References—

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Interactive comment on The Cryosphere Discuss., 6, 5225, 2012.

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