The Cryosphere Discuss., 6, C3086–C3093, 2013
www.the-cryosphere-discuss.net/6/C3086/2013/

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 #4

Received and published: 28 February 2013

Summary comments

This manuscript attempts to tackle the problem of an integrated subglacial drainage and ice flow model. Many ice sheet models account for water by balancing the heat fluxes at the basal interface making a loose approximation to sliding based on water availability. Such models do not adequately account for the water balance and simply use the presence of water as an ersatz for the ability for ice to slide over its bed. Other hydraulic phenomenon

The paper as it stands is not free from errors and requires attention to several issues before it can be accepted for publication. Some of these are simple clarifications. Others require more in-depth revision.

Sections

Abstract

The abstract needs to be rewritten.

There are fundamental questions about the subglacial hydraulic system such as the degree of connectivity and whether it influences ice flow. As such, the leading statement needs to be reworded and integrated later into the paragraph. If the subglacial hydraulic system does influence ice flow, under what conditions does the water system change the flux? For Greenland, the additional flux of ice from lubrication of surface-fed meltwater is roughly 10%. For Antarctica, this is amount is not well characterized. The abstract needs to make that case in a succinct fashion.

Introduction

The introduction convolves separate ideas and is not easy to follow. The basis for including water is that observations link water to enhanced slip. Furthermore, separate observations of water beneath Antarctica show that there are relatively large volumes that move over timescales much shorter than ice flow. However, with the exception of a requirement for till to have enough water to have near-zero effective pressure, the role of water in ice drainage is not well understood.

5227, Line 11. This paragraph is incorrect. Fundamentally, there are two water flow regimes: channelized and distributed. Channelized systems are spatially concentrated and transport large volumes of water at high effective pressure (typically defined as ice overburden pressure minus water pressure). In short, water flux goes up as effective pressure goes up. Examples of channelized systems include Röthlisberger channels cut into ice or Nye channels cut into bedrock. Channelized systems act to reduce slip by drawing water from off-axis flow and increasing coupling there. Their net effect is to reduce ice slip and thus ice discharge.

Distributed systems are laterally extensive and transport a small volume of water at low effective pressure. Examples include subglacial canals (Walder and Fowler, 1994) and linked cavity systems (e.g., Lliboutry, 1968, 1979). Because effective pressure decreases with increasing water flux, these systems tend to enhance slip along the ice–bed interface. It is not clear what role the paragraph beginning on line 16 serves. It starts with how water flows, but then discusses limitations on understanding the basal system. This space should focus on building a case for the modeling effort that comes later in the paper rather than saying why it may not be applicable.

In the paragraph starting on line 24, the point of the models by Schoof, Hewitt, and Werder is that they have collocated distributed and channelized system. Thus, scaling the models from a mountain or outlet glacier is not a problem. What is problematic is knowing what an appropriate channel density becomes once the model is scaled. For example, in a 5 x 5 km Antarctic grid cell, is one channel, ten channels, or 100 channels appropriate? The coarseness of the data grid does not have a direct effect on these models because they are designed to provide both distributed and channelized flow. The text needs to be parsed to reflect the scaling issue rather than the issue of an appropriate DEM.

The DEM used in constructing a hydrology model will ultimately have some effect on the output, that is true, but this is also true for ice sheet models. There has been no shortage of Antarctic ice sheet models despite the coarseness of the DEM.

5228, Line 11 The balance flux model needs to be discussed a little earlier in the paper. While there is no particular reason it does not incorporate specific drainage types, it is commonly treated as a generic flow type. It characterizes the momentum equation for water flow rather than the mass balance. The mass balance, and in particular the closure relationship, determines the flow type.

Again, the authors insistence that it cannot accumulate water is based on what appears to be a limited understanding of how water flows subglacially. There is no particular reason that accumulation in lakes or other depressions cannot be incorporated as the balance flux is formally a momentum assumption rather than a mass balance

The ice model

The justification for equation (3) is not clear. Presumably the scaling flux φ_0 represents a critical amount of water necessary to distribute water over the bed locally. Furthermore, is there a justification for the numerical value chosen? Is this from a previous study? Alternatively, it could be related to grid size or be empirically derived via the present modeling study. The justification for this needs to be clear in the text.

Furthermore, the form of the equation states that the sliding rate increases with water flux. Because that is the case, this model is an approximation to the linked-cavity systems. Typically, sliding rate is coupled to effective pressure rather than water flux. The assumption here is that moving water remains at low effective pressure and channelization does not occur. Later in the paragraph, the authors cite Wingham et al. (2006) who examined the drainage their inferred rates using the channel theory of Rothlisberger (1972). As discussed above, channelization is not incorporated. Can the authors clarify that this number would be reasonable for the model here?

The balanced water layer concept

In general, this section needs to be treated with the same rigor with which the ice flow section is treated. Foremost among these is that the water layer thickness is missing from the section. The equivalent equation to ice flow is,

$$\frac{\partial W}{\partial t} = \nabla \cdot (W\mathbf{v}) + \frac{M}{\rho_{water}}, \tag{1}$$

where water depth is W, water velocity is v following the authors notation. This is equivalent to equation (19) in the text. Equation (7) goes on to discretize this equation in time, but it is not clear how in sections 3.2 and 3.3 this works in a continuum sense. The authors need to build the continuum formulation before working with the discretized form.

Somewhere before section 3.3, the C- and A- grids need to be described in plain language relative to their two-dimensional stencil. The stencils referred to as Arakawa C- and A-discretizations are confusing. The differences are not clear.

Experiments and results

On page 5229, the authors state that they use RIMBAY with an SSA configuration. Then, the authors choose a topography inspired by the Gamburtsev Subglacial Mountains in **East** Antarctica. The Gamburtsevs are beneath the center of the ice sheet under Dome A. It seems implausible that an SSA approximation is appropriate for ice flow through them. Could the authors strengthen their argument or justification for this here?

The Gamburtsevs have valley cross-sections that are $\sim$20 km wide with peaks that are up to 2400 m above sea level (Wolovick et al, 2013). Water networks seem to flow along the valley floors before being frozen-on at the peaks. The “red noise” topography that the authors use does not retain these characteristics, nor is it clear what the characteristics are that are that the authors are trying to simulate. Can the authors please strengthen the case for such topography? Additionally, several papers discuss large scale roughness in Antarctica and how it is organized (e.g., Bingham and Siegert, 2007, 2009).

The result is that the model domain chosen here is too complex for the simple parameterization of the balance flux method. Lakes in Antarctica rarely show inclined roofs

(Fig. 3), and the few that do are much larger than several ice thicknesses. Notably, Lake Vostok has an inclined roof, but the water circulation in the lake causes basal freeze-on. Furthermore, one of the many criteria used for determining subglacial lakes in radar data is that the subglacial roof be bright and flat (see the discussion and references in, Wolovick et al, 2013). The slope amplitudes in Figure 3 appear to be massive, and without at least some justification from the literature for the results (none appear on 5237, line 27 or the first few lines of page 5238) the reader is lead into a territory that is unjustified by data. Furthermore, the combination of unrealistic topography gives rise to a spurious population of lakes in Figure 4a. The authors also identify lakes as any water body greater than 1 m deep (p. 5237, line 22), but again, there is no relationship to topography at all. If the aspect ratio of the roughness along the bed is 1:1, then the basal boundary would not be stress free, for example.

While the authors claim that high surface temperatures (-10C) and high geothermal heat flux (150mW m^{-2}) aid in convergence, it is not clear that the model can perform with a polythermal base. If this is the case, the authors need to state that. The thermal conditions chosen do no mimic the interior of East Antarctica (Gamburtsevs: -50C surface temperature, sim50–60 mw m^{-2} geothermal heat flux) or at the low accumulation rates. The surface temperature the authors choose is close to the equilibrium line in Greenland (-4C (e.g., Ohmura et al., 1992)), and other processes, such as drainage and fracturing could possibly occur in an ice sheet with such parameters. Thus, while there these are set up as tests for a type of hydrological model, it seems that the justification for parameters is weak.

The combination of the enhancement factor in equation (3), the topography, and the thermal and accumulation parameters chosen yield a contrived scenario in Figure 5b where the velocities follow the subglacial water drainage pathways. These are reminiscent of the major outlet glaciers in Greenland (e.g., Jakobshavn) that have a very different flow regime as well as a topographic trough. The other locations that could be analogous are the tributaries to the Amery Ice Shelf. The authors should look for some similarity in the ice sheets, particularly Antarctica to make at least a rudimentary justification for the model.

The authors need to adjust their significant figures to be representative of the model that they are choosing. The accuracy of the numbers reported in the results is incredulously accurate.

Editorial fixes

5226, Line 19: “Many subglacial lakes” instead of “Hundreds”. The numerical value here is not critical and seems to change with every new investigation in Antarctica.

5226, Line 20: Change to decade. The papers cited are all published within the last decade.

5226, Line 23: “are not isolated but **can** belong to **distinct** hydrologic networks.”

5226, Line 25: It is not clear that ice streams form over areas of active subglacial drainage or whether the heat from sliding sustains the water network. The authors should reword this so that either possibility is permissible.

5226, Line 25: This is not an acceleration per se. It is either “increased ice velocity” or an enhanced strain rate. Technically, it is velocity per change in distance, so referring to strain rate is better here.

5230, Equation (4): This should be a divergence operator rather than a gradient operator. Also, indicate that A_s and M are local volumetric rates rather than mass rates. Confusion may arise with subglacial meltwater generation where the melt rates are typically mass melt rates (Clarke 2003, Spring and Hutter, 1981)

References Clarke, G. K. C. (2003), Hydraulics of subglacial outburst floods: new insights from the Spring-Hutter formulation, J. Glaciol., 49(165), 299–313.

Lliboutry, L. (1968), General theory of subglacial cavitation and sliding of temperate glaciers, J. Glaciol., 7(51), 21–58.

Lliboutry, L. (1979), Local friction laws for glaciers: a critical review and new opinions, J. Glaciol., 23(89), 67–95.

Ohmura, A., P. Kasser, and M. Funk (1992), Climate at the equilibrium line of glaciers, J. Glaciol., 38, 397–411.

Röthlisberger, H. (1972), Water pressure in intra- and subglacial channels, J. Glaciol., 11(62), 177–203.

Spring, U., and K. Hutter (1981), Numerical-studies of jökulhlaups, Cold Reg. Sci. Technol., 4(3), 227–244.

Walder, J. S., and A. Fowler (1994), Channelized subglacial drainage over a deformable bed, J. Glaciol., 40(134), 3–15.

Wingham, D. J., M. J. Siegert, A. Shepherd, and A. S. Muir (2006), Rapid discharge connects Antarctic subglacial lakes, Nature, 440(7087), 1033–1036, doi:10.1038/nature04660.

Wolovick, M. J., R. E. Bell, T. T. Creyts, and N. Frearson (2013), Identification and control of water networks under Dome A, Antarctica, J. Geophys. Res., 118, doi:10.1002/2012JF002555.

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