The Cryosphere Discuss., 6, C464–C469, 2012
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The Cryosphere Discussions

Interactive comment on "Simulating the growth of supra-glacial lakes at the western margin of the Greenland ice sheet" *by* A. A. Leeson et al.

Anonymous Referee #1

Received and published: 2 May 2012

Review of "Simulating the growth of supra-glacial lakes at the western margin of the Greenland ice sheet" by Leeson et al.

This paper is one of the first attempts to numerically model the transient evolution of supraglacial lakes in western Greenland. My general comments focus on (1) the initial condition imposed on the model, (2) the numerical method implemented, and (3) the absence of parameterizations for firn and/or end of season refreezing or drainage. These sticking points, which presently limit both the credibility of the result (1 and 2) and the predictive power of the model (3), are certainly remediable and may just require further clarification.

General comments:

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(1) While not explicitly stated, it seems that the initial condition imposed for ice sheet surface is an ice sheet surface completely devoid of any lakes (i.e. just empty "sink" points). Thus, all lakes are modelled as filling from empty within one melt season. In actuality, substantial volumes of lake water overwinter in supraglacial lakes in western Greenland. Thus, at the beginning of a melt season the majority of lakes already contain substantial water, and only a minority of lakes are empty following late melt season drainage events. The initial condition therefore does not seem consistent with the observed seasonality characterized in many of the references cited. If my interpretation is correct, it would imply that the paper presents a maximum upper bound on the lake volume generated and retained within a single season, when in reality a much smaller fraction of annual melt would be retained in lakes that already contain melt from many years. As a corollary, that would mean the paper presents a lower limit for runoff.

(2) The dependency of the agreement between simulated and observed maximum cumulative lake area on model time-step seems more like a concern, rather than a result, to me (P1319 L9). It is my understanding that neither modelled discharge nor head should significantly differ with the choice of time-step in hydrological modelling. When this happens, it typically indicates a shortcoming in the numerical method used to solve the system of equations describing node-coupled fluxes (i.e. not true mass conservation of fluxes). In fact, producing an identical secondary simulation with a time-step 1/10th the size of the time-step used in a primary simulation is considered a good way to demonstrate sound numerical implementation. The paper presently suggests the opposite. While not stated in the paper, the relatively small time-steps imposed lead me to believe an explicit numerical method was implemented (i.e. Euler forward?), as opposed to an implicit numerical method which would likely run on hourly or daily time-steps. Explicit numerical models are widely recognized to be highly time-step sensitive. For example, it is easy to imagine how discharge differences can arise between runs in which the free-surface gradient is maintained a single 60-sec time-step, rather than allowed to gradually decay over 60 1-sec time-steps. I suppose more clarification is required on the numerical method, but it is quite possible that an implicit

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numerical model method have to be implemented to convince the reader there are no numerical artefacts.

(3) Supraglacial lakes tend to form at higher elevations on the ice sheet, where firn is present (year-round by definition). Routing meltwater in these high elevation regions without even a crude parameterization for firn effects seems to be quite limiting. For the early portion of the melt season, Darcy porous flow is far better for describing the horizontal movement of meltwater than Manning's open channel flow. Indeed, even at the end of the melt season, the vast majority of the ice sheet surface above 1200 m is still covered by firn as opposed to open channels. It is easy to imagine firn effects (such as runoff delay and/or refreezing retention) as primarily responsible for the discrepancy between observed and modelled lake area growth. Similarly, the absence of a parameterization for lake refreezing or draining at the end of the melt season limits the predictive power of the model. I can appreciate that the authors have limited their title to modelling the onset of supraglacial lakes, but the introduction of the paper couches the paper in the importance of what happens at the end of the melt season.

Other general comments:

The presentation of lake location as an independent model output seems to be slightly misleading, as lake locations are primarily the result of the DEM accuracy (i.e. the DEM determines the sink locations, the hydrology model just fills them). Perhaps the good prediction of lake location should be presented primarily as an endorsement of the DEM and secondarily evidence of realistic routing?

I would encourage the authors to also include sensitivity analyses of additional key variables such as the Manning coefficient and DEM accuracy.

Specific comments:

P1308 L2: "routing" seems to be more frequently used scientific literature than "routeing".

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P1308 L10: The 17 % presumably refers to fast drainages observed by Selmes et al. (2011)... after a quick look at Selmes et al. (2011), it seems to me that number is specific to NE Greenland?

P1310 L8: Should that be "Sole et al."?

P1311 L10: I imagine it is supraglacial lake surface, rather than supraglacial lake bed, that is incorporated in this ice sheet surface DEM? Presumably the majority of the true ice sheet "sinks" were already filled with lakes at the time of DEM acquisition... is the m-scale vertical offset between lake bottom and lake surface important in 100m-scale horizontal routing? I would think this may make your model overestimate lake area, as the modelled lakes have shallower and broader sink points in which to accumulate, in comparison to the actual ice sheet surface / lake bed topography.

P1312 L8: "additional physics are".

P1312 L25: From where does this assumed 12% ASTER-MODIS discrepancy come? Sundal et al. (2009) suggest it is only 4% (see their figure 2 and section 3.2).

P1313 L11: Rather than "a dynamical model of water flow...", can you say "a fully transient 2D hydrology model..."? At present it is left to the reader to assume the model is 2D and can be run in both steady-state and transient modes.

P1318 L15: Comparing the red solid (6.5 %) and red dotted (4.0 %), it looks like more of a 63% overestimate ((6.5-4.0)/4.0) to me, rather than the 51 % stated. In either case, it certainty tests the bounds of "reasonable" agreement..

P1318 L28: The present estimate of the observed meltwater volume is very zeroth-order. From where does the assumed mean lake depth of 3.11 m come? Box and Ski (2007) provide a very wide range of (maximum) lake depths (not sure how to translate that into a mean lake depth), and Liang et al (2012) use a first-order area-to-volume conic approximation. At a minimum, with the presently employed estimation, a range of lake depths should be used to provide a range of comparable observed water volumes.

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P1321 L5: See Colgan et al (2011) regarding crevasses in western Greenland. To be consistent with the notion that the presence of crevasses reduces the meltwater available for lake, is there a way to simulate the presence of crevasses by say reducing the melt available by routing based on an assumed crevasse fraction?

P1321 L16: You just previously argued that the surface topography (and hence lake position) was stable (on P1313 L5 and again on P1322 L19)?

P1327 L20: Duplicate reference.

P1331 Fig1: Some confusion over whether it is a Bamber or Layberry paper being referenced.

P1333 Fig3: Having the modelled lakes the same color as the background ice is not ideal. Perhaps for easier interpretation you might consider representing the observed and modelled lakes with primary colors (i.e. red and blue), and their overlapping area with the corresponding secondary color (i.e. purple)?

P1336 Fig6: I think the description of these results is over optimistic in the text... three of four years appear to have a 100%+ discrepancy between observed and modelled lake area coverage...

References cited:

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123: 127-138.

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Interactive comment on The Cryosphere Discuss., 6, 1307, 2012.