

Interactive comment on “Modelling the transfer of supraglacial meltwater to the bed of Leverett Glacier, southwest Greenland” by C. C. Clason et al.

Anonymous Referee #1

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—General Comments—

The submitted paper “Modelling the transfer of supraglacial meltwater to the bed of Leverett Glacier, southwest Greenland” by Clason, et al. describes a model of the transfer of supraglacial melt to the bed applied to a well-studied catchment of the Greenland Ice Sheet. The previously described model includes components for meltwater production (using a degree-day scheme), meltwater routing across the ice sheet surface, and formation of full-thickness crevasses/moulins and their subsequent transfer of water to the bed. The application to the Greenland Ice Sheet includes a parameter sensitivity analysis, forcing sensitivity analysis, and comparison of model results to observations of supraglacial lake drainage and ice velocity.

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The paper is well-written and fills a gap in current efforts at understanding the basal lubrication process in Greenland and how it may evolve in the future, which are primarily focused on melt production or subglacial hydrology. My primary concerns are 1) that rigor in treatment of supraglacial lakes does not seem commensurate with the rest of the analysis, potentially undermining some of the conclusions; 2) the lack of a control simulation makes it difficult to assess model skill. Treating both of these issues would increase the contribution to the field provided by this paper. In addition to these major concerns, I have listed additional issues below.

—Primary Specific Comments—

Supraglacial lake treatment (p4250/10-15): Is there a reason specified lake locations and volumes were used rather than using the 100 m resolution DEM to identify closed basins? I imagine the 100 m resolution is not quite high enough to accurately identify closed basins, but if that is the case, please say so. How confident are you that the 15 and 31 July 2009 Landsat images show the maximum lake extent, particularly given that the model is applied to two different years, and that the timing and magnitude of maximum lake extent varies with elevation and interannually (e.g., Morris et al., 2013)? Finally, how do you know that the maximum lake volumes associated with the 15 and 31 July 2009, areas are appropriate for overtopping? Perhaps those lakes drained rather than overtopped, and they would have filled to greater volumes otherwise.

I realize these are difficult problems, but if they cannot be adequately addressed, I think the paper should acknowledge these limitations and/or perform a sensitivity analysis to determine if these uncertainties are even important. Some greater treatment of these issues is warranted if the paper is billing itself as “the first predictive (rather than prescriptive) model for . . . the transfer of meltwater to the ice-bed interface applied to the Greenland Ice Sheet.” Prescribing lake locations is contrary to this stated purpose.

Absence of control simulation: In section 5.2 a comparison is made between modeled delivery of meltwater to the bed and observed ice speed. The authors subjectively

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state that the model matches well with observations. I do not particularly contest this assertion given the uncertainties in the model and the lack of a subglacial hydrologic model. However, the reader would be much better able to assess this comparison if a control simulation were included. Specifically, if all melt was able to reach the bed locally and instantaneously, or after a delay associated with snow cover, how different would Figure 8 look? (This is effectively what was assumed in a recent large scale modeling study of the effect of increased Greenland melt on basal lubrication and sea level rise (Shannon et al., 2013).) The model performance relative to a control simulation could be assessed with simple statistics (such as a correlation). The relation to a control is hinted at on p4259/12, but a more thorough assessment would be helpful.

Should the model outperform a control simulation, it would strengthen the authors' case about the importance of adding englacial hydrologic processes to larger models. Even if it turns out that the model does not significantly outperform a control simulation, there may be good reasons to think these extra physics are worth modeling. But without it, readers are unable to assess what is gained by the extra trouble of modeling these processes. (In this vein, the section on sensitivity to atmospheric warming is compelling.)

—Other Specific Science Comments and Technical Corrections—

p4245/12: I'm not convinced that "The temporal and spatial patterns of modelled lake drainages are qualitatively comparable with those seen from analyses of satellite imagery" is a meaningful highlight to include in the abstract, given the method of prescribing lake location and volume. See detailed remarks above.

p4247/21: A brief clarification is needed here if this is the purely supraglacial catchment or an inferred catchment from which subglacial discharge is sourced. If the latter, a brief explanation of how this was determined is appropriate since surface elevation is not sufficient for such a determination. For example, was a certain effective pressure assumed? See Schoof, et al. (2014) for an example of such a discussion.

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p4249/1: I do not see "UDG" defined prior to this usage.

p4249/5: Does this sentence mean that additional development of the model occurred after the application to Croker Bay? If so, I would reword to "...model has been _further_ developed..."

p4249/28: Is there precedence or justification for the linear scaling of runoff delay with snow thickness? It does not seem unreasonable, but if there is a source to this approach, it would be good to add it.

p4250/1: It seems present tense is used to describe the model in this section, whereas past tense is generally used elsewhere in the model description.

p4250/19: I think another sentence or two of supraglacial lake drainage modes would be appropriate (either here, moved to the beginning of this paragraph, or in the introduction). It would be helpful to the reader here to make a clear distinction between lakes that drain slowly overland due to overtopping their basins and those that drain catastrophically through moulins formed in their lakebed. Both processes are being modeled here, but the distinction is not immediately clear without reading through Appendix C. This distinction has been discussed by previous authors (e.g., Hoffman et al., 2011; Selmes et al., 2011; Tedesco et al., 2013 [this distinction is discussed in detail by the last citation]).

p4253/7: For clarity, start a new paragraph with each new sensitivity parameter.

p4253/18: 'limitation' might be a more appropriate word here than 'constraint'.

p4253/18-28: This is an interesting result of the sensitivity analysis. Certainly resolution-dependence is a significant limitation of this kind of model. Demonstrating that the resolution does not substantially effect the modeled amount of water transferred to the bed is important.

p4254/6-18: Are the results of these tests included anywhere? It seems they could be added to Table 1 (or a separate table) without lengthening the manuscript much.

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p4254/15-16: Should this be “tensile strength” instead of “tensile stress”?

p4254: Section 4.3 title has ‘density’ misspelled.

p4254/21: “was” -> “were”

p4256/2-11: See general notes above regarding “Supraglacial lake treatment“. It should be acknowledged in this section that lake positions and volumes were prescribed, so it is unclear how much skill the model actually has regarding lake drainage. For example saying “In both approaches... drained lakes mostly occur between 1000 and 1400 m elevation” is a trivial comparison to make with the method used.

p4257/8: (Fig. 8) -> (Fig. 8c,d)

p4258/11: “Hoffamn” -> “Hoffman”

p4258/25-: Discussion of Meierbachtol et al., (2013) would also be appropriate here.

p4264/11: It would be interesting to see a bit more description of the tensile strength tuning process or a figure showing both areas of surface crevassing and surface tensile stresses. I do not expect a perfect match, but it would be nice to get a sense of how successful such a comparison is. I think this would be valuable considering the authors say that “the most important control on the spatial extent of moulins is the value of the tensile stress”.

Table 1: Why do the columns “Meltwater transfer (% transfer from surface to bed)” and “Supraglacial storage (% of total generated meltwater)” not sum to 100%? Is there another fate of meltwater? Either the caption or p4252 might be an appropriate place to explain this discrepancy.

—References Cited—

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