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Interactive comment on "High-resolution modelling of the seasonal evolution of surface water storage on the Greenland Ice Sheet" *by* N. S. Arnold et al.

Anonymous Referee #1

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Arnold et al. provide an overview of their meltwater routing and lake filling/draining model, applied to Pâkitsoq area, West Greenland. The model appears to reasonably reproduce observed lake areas and volumes. The reader is referred to Banwell et al. (2012b) in lieu of the description of some model parameters and conditions. Given strong similarities of this present manuscript with Banwell et al. (2012b) at the abstract level, this work may be considered an incremental increase over previous work. For me, the most interesting finding of the study was not establishing a threshold drainage volume, but rather that synchronous synoptic-triggered neighboring lake drainage events are dependent on similar ice geometry (i.e. depth). I find the inference that 50+% of meltwater travels to the margin via overland runoff difficult to rationalize with my field time in Pâkitsoq.

General Comments

1. Given the close relation of this lake filling approach with that of Leeson et al. (2012), the authors should acknowledge Leeson et al. (2012) and compare and contrast their approach and results with Leeson et al. (2012).

2. The authors consistently describe a "threshold volume", when a "threshold depth" seems to be the root of the lake draining mechanism they are exploring. A sentence such as: "Model performance is maximised with prescribed lake volume thresholds between 4000 and 7500 times the local ice thickness" appears dimensionally challenged: threshold depth vs. local ice depth is obviously the meaningful/root ratio that has essentially been cubed. "The volume needed to fill an inferred fracture extending from the ice surface to the bed" is similarly awkward. Obviously water depth and volume/area are related, but depth ultimately serves as the drainage trigger.

3. The initializing DEM conditions is not entirely clear to me. It would seem that the DEM being employed reflects the ice sheet surface as observed in its "natural" state (i.e. with lakes), rather than the "idealized" (i.e. lake-free) surface that would be most suitable for initializing a model in which depressions become filled with water. Can the authors please clarify how they artificially empty the observed lakes to initialize their model. A corollary query would be if/how inter-annual hysteresis in lake volume is dealt with. In reality, not all lakes begin each season empty, but rather with some volume of water remaining from the previous season. Indeed the authors suggest a non-trivial 5 % of annual melt is stored in lakes at the end of the melt season. So how are the years 2001, 2002 and 2005 represented in isolation of essentially "spinning up" lake volumes from the preceding year?

4. Lateral boundary conditions of the model are not described. Presumably some melt generated within the study region laterally exits the study region, while some melt generated outside the study region laterally enters the study region?

5. The implemented numerical method is not described. If it is an explicit (e.g. Euler Forward) implementation with very small time-steps has been employed, then I would think the authors are obliged to demonstrate time-step independence of the final solution.

6. The flow routing mechanism could use some/more description. Presently the reader is not informed whether tuned Darcy flow, or a combination or Darcy / open-channel flow (which is closer to reality), is being used to move meltwater across the ice sheet surface into lakes.

7. The parameter "fracture area" is not clearly defined. In what dimensions does the area span (e.g. xz or xy). An illustration may be helpful. The choice of fracture area thresholds is similarly not explained, and thus seems rather arbitrary, meaning that fracture area is a tuning parameter (as it is implicitly acknowledged to be). While the authors refer to Clason et al. (2012) regarding the "water volume threshold-based model of surface lake drainage", my admittedly quick read of Clason et al. (2012) is that it employs the now "classical" Van der Veen (2007) 1D approximation nested in a 2D model, so that it deals in length units (i.e. not fracture areas).

*An overarching comment at this point: The reader should not be referred to secondary material for the basic methodological points of 4 to 7 (e.g. Banwell et al. 2012b or Clason et al., 2012).

8. The results currently recognize three fates for meltwater: (1) supraglacial runoff, (2) storage in lakes, or (3) drainage into the subglacial system. I suppose the recent work of Forster et al. (2014) shows us that englacial storage can be a substantially term in lower firn zone (where some of the lakes under discussion reside).

9. The authors assert that 40+ % of the meltwater of the study site leaves the ice sheet as supraglacial runoff, while the remainder enters the subglacial system after some period of temporary supraglacial lake storage. On the surface this agrees with McGrath et al. (2011), whose in situ supraglacial water budget (within the "Pâkitsoq" study area

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I believe) also suggests that about half the meltwater in there catchment goes into a moulin. McGrath et al. (2011) do not invoke 50 % overland runoff from their delineated catchment, however, but rather suggest that the non-moulin discharge leaves the supraglacial system via crevasses. I find it more reasonable to suggest substantial crevasse drainage than invoke substantial overland flow all the way to the margin. Those of us who have spent time on the ice sheet in Pâkitsoq in August can attest that overland flow is restricted to local moulin catchments, and there are neither great rivers extending inland, nor great waterfalls cascading off the ice sheet margin (the occasional waterfall perhaps, but certainty not enough overland ice to ground discharge to move a Gt of water).

10. How does the model account for the presence of crevasses? The supraglacial hydrology map of Thomsen et al. (1988) suggests a substantial portion of Pâkitsoq is sufficiently crevassed as to prevent the establishment of supraglacial streams and lakes. If the author's model does not permit some volume of water to drain via (non-lake-associated) crevasses, then presumably too much water volume is being lumped into the three water fates (e.g. lake storage, lake drainage, and runoff)?

11. Perhaps the authors can soften their criticism of observational (or remotely sensed) studies being of "limited temporal resolution", given that their own model approach only covers a fraction of time and space of some of the implicated studies (e.g. Liang et al., 2012; Fitzpatrick et al., 2013).

12. While I think the discussion could be substantially streamlined, a bullet-point conclusion would be atypical of The Cryosphere.

Specific Comments

1. Colgan et al. (2011) not in references.

2. I believe the Geological Survey of Greenland uses "Pâkitsoq", not "Paakitsoq".

3. I do not think Asiaq is an acronym (i.e. sans capitalization). It is also spelt "AI",

rather than "IA" in one instance.

Sentence structure, spelling and grammar are all of high quality.

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