Response to Anonymous Referee #1

We thank the referee for their considered comments, and we provide a response to each comment below, using italics to highlight our response.

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.

The methodology we apply is very similar to that of Banwell et al. 2012b, but the point of the paper is very much to apply those methods to a wider area of the ice sheet to enable us to consider the merits of an approach to simulate lake drainage based on a volume threshold (as has been used in other approaches, not just our own (e.g. Clason et al. 2012)). We will make this motivation clearer. We agree that the clustering of lake drainage events produced by our model (also highlighted by referee 2) is an interesting result, and we can re-word the conclusions to emphasise this. As we indicate below, we are happy to include additional methodological details if requested in a revised version of the manuscript, and we will also re-word the discussion of supraglacial runoff volumes to make it plain that this water does not necessarily reach the terminus of the ice sheet, but rather represents the portion of water generated at the ice sheet surface which does not enter the subglacial drainage system via drainage of supraglacial lakes.

General Comments

C3220

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).

We agree that we should also acknowledge the approach of Leeson et al (2012) and will do this as appropriate.

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.

Although the 'threshold depth' of a crevasse changes spatially due to changes in ice thickness across the model domain, it is the 'threshold surface area' of a crevasse that we are able to manually alter between model runs and endeavor to find the optimum value for. This is our parameter Fa. We are therefore investigating the 'threshold water volume' by altering the 'threshold surface area' (which is consistent for all lakes) and multiplying it by the ice thickness (i.e. 'threshold depth') at each lake location. Multiplying this assumed surface area by the local ice depth beneath the lake yields the water volume we assume is necessary to trigger lake drainage. This is the same approach as used in Banwell et al. (2013), and Clason et al. (2012), and we will clarify the approach in this paper. We appreciate that the wording we have used in the two sentences highlighted by this reviewer is confusing, and we will re-word these sentences to ensure that they are clearer and not 'dimensionally challenged'. See also our response to comment 7 below.

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?

Full details of the production of the GIMP DEM are now available as a TCD discussion paper (Howat et al. 2014), and we will reference this in any revised MS. The DEM is produced via a complex process using an enhanced version of the 1km DEM produced by Bamber et al. (2001), linked with the ASTER-derived GDEM2 product, and the SPOT derived 'SPIRIT' DEM (Korona et al. (2009)), and ICESAT GLAS elevation data. Given the wide variety of imagery (visible, photogrammetric and radar), acquired over many different dates, used in the production it would seem unlikely that there are any systematic tendencies for the surface of lakes to appear in the DEM as the ice surface elevation; in some ways, the fact that filling the DEM with water produces realistic water depths (when compared with visible satellite imagery) suggests that the DEM typically does include the bed of lakes rather than the surface. In some ways this is a similar point to number 1 by referee 2; as far as we are aware, we are the first group to use the GIMP DEM in this way, and in a sense, validate it by comparing the extent of surface depression on the ice sheet (and hence the possible lake extent and depth predicted by the DEM with observed lake extent and depth. In terms of initialisation, we begin each year with the DEM 'empty' of water. This is an approximation, but visible imagery from early in the summer suggests little supraglacial water storage, certainly at lower elevations on the ice sheet. It would obviously be difficult to identify 'full, frozen' lakes in visible *imagery, and these are likely to be much more common at higher elevations close to the ELA. We do not see this as a significant source of error in our overall results as our domain extends only to around 1500m elevation, and the highest lake drainage events we observe occur at between 1200m and 1300m elevation, around the ELA; at this elevation, and above, runoff production is small. If requested for a revised manuscript, however, we could do some simple sensitivity testing to investigate the impact of some existing water volume on model results.*

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?

The model allows for water generated within the study region to flow out laterally; this is quite prevalent on the southern edge of our domain, where water flow is orientated more towards the SW (as indicated by the contours in Figure 1. We do not see this as a problem, however; it is no different to water leaving the 'domain' defined as the ice sheet itself. This water does appear as what we call supraglacial runoff at present, but (as discussed in reply to comment 8 below, and point 3 by referee 2), we will clarify what we mean by this more clearly in a revised manuscript, as it was not intended to be literally 'water flowing off the ice sheet margin in supraglacial streams'.

In terms of water flowing into our model domain (which could be a more serious source of error), this is likely to be very small. The eastern boundary of our domain is above 1500m elevation, and is therefore likely to only generate very small quantities of surface runoff. The western boundary is ice free. Water typically leaves our domain at its southern boundary (as discussed above), and again on the northern boundary, marginal water flow is typically directed towards the edge of our domain, meaning water will leave at this edge, rather than enter.

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.

See also reply to comment 6. We are happy to add more details about the modeling algorithm used, but we do not model the small-scale water flow properties within the model; as detailed in Banwell et al (2012b), and used by Banwell et al. 2013, we use an assumed channel geometry (constant for each grid cell for each hour of the model run) and the Manning equation (or Darcian flow for snow-covered cells) to calculate the mean water velocity, and hence the time taken for water to cross each cell. The overall time for water flow between 'source' cell and 'sink' cell (lake/moulin/ice margin/domain edge) is then integrated down the surface slope to calculate the total delay time for that 'parcel' of melt. Given that the melt model has an hourly time step, and that we are comparing our results with satellite imagery taken as representing the depth of lakes in a given day (rather than at the time the image was actually acquired) we do not see this as a problem. We would be happy to show the impact of different timesteps on the model results if requested, however.

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.

As explained in the paper, we use the same routing algorithm as described in Banwell et al (2012b) and Arnold (2010). We therefore didn't feel the need to repeat all the details of the algorithms in this paper, but instead we refer the reader to those papers. However, we are happy to add more details of the routing algorithm into this paper if it is thought to be necessary, as discussed in comment 5.

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).

See reply to comment 2 above; additionally, the 'fracture area' is the 'xy' area, i.e. the surface area of a potential crevasse in the bottom of a lake of a certain length (x) and width (y). We agree that this parameter is not clearly enough defined when it is first mentioned and we will make this clearer for the next iteration of the manuscript. Again the approach is the same as is used in Banwell et al (2013) (which uses the ideas of Clason et al (2012)), which will also be referenced. In this latter paper, whilst the Van der Veen model is used to simulate slow downward growth of crevasses due to filling by water for crevasses outside lakes, drainage of lakes is assumed to occur by rapid fracture once the lake contains sufficient water to fill an assumed crevasse from the bed to the surface.

*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).

We will ensure that we include a more full description of the methods in this paper as well as referring to existing literature.

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 substantial term in lower firn zone (where some of the lakes under discussion reside).

These are the three main fates of water, but water is also stored supraglacially within our model. The surface energy balance model we use to calculate distributed melt inputs to the supraglacial hydrology model includes a water storage term for storage in saturated snow, and also allows for refreezing; not all the melt generated at the surface in this *model reaches the base of the snowpack, which forms the input to the supraglacial flow model. Some water is also stored 'in transit' within supraglacial streams (or in any remaining snow pack) as we discuss in our paper (e.g. line 21, p 6153). See also our reply to comment 5 by referee 2. We do not include englacial storage within the model; we are effectively assuming that all water which enters a moulin at the bottom of a drained lake reaches the subglacial drainage system, but we do not explicitly model the behaviour of this water. We will ensure that we mention the possibility of englacial storage in the next iteration of our paper.*

In terms of firn water storage, the surface mass balance (SMB) component of our model allows for storage within snow or firn; such water does not enter the SRLF component of our model, and we have not quantified this stored water in the current version of the manuscript. We could quantify this in terms of the total amount of surface melt calculated by the EBM component versus calculated runoff at the base of the snow/firn pack from the full SMB component (which forms the inputs to the SRLF/SLD model) if requested for a revised manuscript. As an indication, over a subset of the Paakitsoq region, we have previously calculated that 6% of surface meltwater and rainwater refreezes in the snowpack and does not become runoff (Banwell et al. 2012a). The model domain in this study extends to higher elevations, but overall we would still expect this to be a relatively small proportion of total melt over the model domain, as melt production at higher elevations is small.

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 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).

We entirely agree with the reviewer that some of the calculated meltwater runoff which we currently call supraglacial runoff will not actually leave the ice sheet surface at the margins, but is instead likely to be intercepted by crevasses and/or other moulins. We will clarify this in the paper; by the term 'supraglacial runoff', we do not mean water that literally 'runs off' the front of the ice sheet, but rather water which does not enter the subglacial drainage system via drained lakes, and hence is water that is available as meltwater on the ice sheet surface after snow/ice has melted and not refrozen. This water could enter the subglacial system via crevasses and/or moulins outside lake basins, or it could be stored supraglacially in crevasses that do not reach the bed. Drainage of water via crevasses outside lake basins remains an under-researched area (although we *currently have a PhD student working on this issue, and it will be addressed in future papers by us), and other papers which have modeled the filling of lakes have also not modelled potential storage or interception of surface water by crevasses (e.g. Leeson et al. 2012). See also our reply to referee 2, comment 3.*

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)?

Please see our reply to comment no. 9. Water we currently call 'supraglacial runoff' could be intercepted by crevasses, and we will make this possibility clear.

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).

We agree with this comment and will reword the relevant sentence accordingly. See also our reply to comment 9 by referee 2.

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

We are happy to remove the bullet point format of the conclusions.

Specific Comments

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

We will add this.

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

Various spellings for the Paakitsoq region exist. A quick Google Scholar search shows 52 results for Pâkitsoq, 118 for Paakitsoq, and 130 for Pakitsoq. To be consistent with our previous work (Banwell et al. 2012a, 2012b, 2013, 2014), we think it's sensible to continue to use this spelling. Other authors using Paakitsoq include Mottram, Machguth, Ahlstrøm, Reeh, Ohmura, Thomsen, etc.

3. I do not think Asiaq is an acronym (i.e. sans capitalization). It is also spelt "AI", rather than "IA" in one instance.

Thank you for pointing this out. We will change ASIAQ to Asiaq.

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

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