This paper presents a new model of subglacial hydrology, based on concepts from groundwater hydrology. A distributed system is modelled as a confined/unconfined till aquifer, while the effects of channelization are mimicked by varying the permeability/storage of the aquifer. This approach to subglacial hydrology is similar in nature to De Fleurian et al. (2014); however, rather than modelling channels as a separate layer, this approach allows the subglacial system to be modelled using a single layer. The aim of this approach is broadly capture the behavior of the subglacial system, rather than to model the impact of discrete channels. This upside is thats its designed to be computationally cheaper than more explicit models such as Werder et al., (2012) or Hewitt (2013). The authors show the behavior of the model on synthetic test cases from SHMIP, and present a real world application to NEGIS.

Efforts to parameterize the subglacial system in a simple, computationally efficient manner are important for incorporating subglacial hydrology into modelling studies covering large spatial and temporal scales. The work of the authors is novel and addresses an important area of research. However, I have several concerns that the authors should to address.

Assumption of till

I think an implicit assumption in the model is a soft-bed subglacial hydrology system. This should be clarified. Since the model is applied to Greenland, I think it would be relevant to briefly cover literature discussing whether a soft-bed system exists there, since there is disagreement about the nature of the bed. This could include recent seismic studies, and touch upon the previous modelling studies making this assumption (e.g. Bougamont et al, (2014)). Further, it may also be beneficial to readers to discuss the relationship between till and sliding laws.

Model Formulation

The model description is unclear about how to conceptually understand this model. I am puzzled about the motivation and physical interpretation of scaling K with channel opening/closing while the aquifer is unconfined. This would imply that channels are forming in the porous medium? The equations for channels are not applicable there. I'm perplexed since I would expect K to be constant (or perhaps depend on other variables like strain rate, stress, sediment properties) in the unconfined case, and then force a switch to channelized behavior once the aquifer becomes confined.

In your model formulation, you scale conductivity (Eq 9) using the equations for channel opening/closing. However, wouldn't it be more appropriate for K be scaled such that flux through a grid cell scales to flux through an idealized channelized system? In other words, I would expect an attempt to scale $\nabla\cdot(T(h)\nabla h)$ with discharge through channels (using an assumed channel spacing). Where the discharge through channels is (e.g. Equation 3 from Hewitt (2013))

$$Q=-K_c S^{(5/4)}\left|\frac{\partial\varphi}{\partial s}\right|^{\left(\frac{-1}{2}\right)}\frac{\partial\varphi}{\partial s}$$

When channelization is introduced into models, they can grow unstably and dominate the system as effective pressures in channels decreases with increasing input. This doesn't appear to occur in your model. You should give a physical description of the terms (and point out the terms in your equations) preventing this.

The argument that the amount of water released from an unconfined aquifer is larger than a confined aquifer is counterintuitive (P4L5). If the head drops 1m in the unconfined case, than wouldn't the water released be much greater than in the confined case, due to the volume of water in the latter being limited by the porosity?

Numerics

Your conductivity doesn't appear to show grid convergence, even at resolution of 500m. However, I would expect large scale model runs to require convergence at much coarser resolutions. While you identify the conditions under which you get the checkerboard pattern, you don't really explain whats causing it to form. Is this not an artificial pattern due to the numerics? You're solving a highly non-linear equation. In a second order discretization, the dominant truncation error is odd (third order), and hence the error generally will behave in a dispersive manner. Equations 7 and 8 suggest that N ~ Pi_i – h. Since N appears smooth in your plots, this implies h should be smooth, and then I'm uncertain why K isn't smooth as well.

Application to NEGIS

Your study domain encompasses areas of both fast flow and slow flow. While the assumptions of the SSA are valid for the ice stream itself, the SSA is not the right approximation for ice flow over the majority of the domain. This is evident in Fig 10a, where we can see that your modelled ice speed is ~1 m/a over the majority of the domain. Over this part of the domain, internal deformation is the key component of ice flow. The comparison of panels A and B in Figure 10 shows not only the effect of subglacial hydrology, but also of different ice physics. Aschwanden et al. (2016) use a 'hybrid' model described in Bueler and Brown (2009), which is itself an approximation (in effect) to the Blatter-Pattyn approximation. It is worth noting that other 'hybrid' approximations exist, such as L1L2 or that of Goldberg (2011). Because of the different regimes in your domain, I think it is necessary to either use a hybrid approximation (if its available in ISSM), or Blatter-Pattyn to test the results of your coupled model.

Minor Comments:

P1L2: ...'drives freshwater into the ocean'... State the explicit impact of this (e.g. undercutting at calving fronts?)

P1L13-16 This paragraph would benefit from references.

P2L1: 'predominant in alpine glaciers and on the margins of the Greenland': This has more nuance, as channels develop seasonally, and are not predominant year round.

P2L7: I think Hewitt (2013) should be mentioned here, as should Hoffman and Price (2014)

P2L8: …'remarkable results for spontaneously evolving channel networks'. The wording/concept could use clarification

P2L27-28: 'While the assumption … with lower water input'. Citation needed

P3L15: hydraulic head needs a definition

P3L18: Eq 2: porosity cancels itself out. Can you confirm that the units match up?

P3L19: the definition of alpha in table 1 reads the 'compressibility of water'. The definitions of alpha/beta_w in table 1 needs to be switched I think.

P3L25: although h is defined as the hydraulic head, it appears to be used as the saturated height

P3L28: In Equation 5, it is unclear why Se(h) in the unconfined case depends on b. When the aquifer is unconfined (say with a saturated thickness of 1m), does it matter if the aquifer thickness is 10m or 20m? To gain a better overview of this formulation, I looked at the 'Groundwater flow equation' page on wikipedia (https://en.wikipedia.org/wiki/Groundwater_flow_equation). Although I admit that it is not an authoritative source, the formulation there states that Se(h) = S_s*b in the confined case, and S_e(h) = S'(h) is the unconfined case.

P4Fig1: This should be updated/supplemented to show the physical interpretation of the models with channels.

P5L17: I would move discretization to an appendix. It's beneficial to any reader looking to reimplement your model, but not necessary in the main text.

P7L16: Can you cite the upcoming results as Author(s) (In Prep)?

P8 Table 3: I think it would be beneficial to discuss these values compared to the inferred hydraulic conductivity values of till: 10^-9 - > 10^-4 m/s (Fountain and Walder, 1998).

P11L4: 'less' → 'lower'

P13L18: basal topography has no influence at all in the unconfined case?

P17L19: You need to define N_HUY.

P18L1: the quotient of X^-1 and Y^-1 could be simplified to the quotient of Y and X.

P18L8: It's important to add a citation here to MacAyeal (1989) and/or Morland (1987). In particular, the term SStA is confusing, since this approximation is often known as the SSA in the community. There is a proliferation of 'hybrid' models now, combining SSA and SIA, so a variation like SStA could be misinterpreted as one of those. I looked up the ISSM documentation to be sure that SStA was equivalent to SSA.

P18L31: The improvement/comparison of your results should be quantified.

P19L8. 'This decisively illustrates the importance of having a real two-way coupling between the ice model and the basal hydrology model in order to obtain good results.' I don't believe you have shown this. You would have to show that you cannot reproduce the results with 2-way coupling, and then show that you reproduce the results when 2-way coupling is introduced. Since it is not cited, I would point the authors to Hoffman and Price (2014) for a detailed discussion on coupling of ice flow and subglacial hydrology.

P19L17: This sentence reads oddly.

P24Fig10. The descriptions and panel labels are mixed up [there is no (d) in the figure caption].