Review of "Active lakes in Antarctica survive on a sedimentary substrate – Part 1: Theory" by S. P. Carter, H. A. Fricker, and M. R. Siegfried.

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General Comments

This paper aims to construct and analyse a model of subglacial lake drainage through channels that form by melting basal ice and channels that form by eroding subglacial sediments. Subglacial lake drainage is a subject that has relevance for our broader understanding of the cryosphere and the scientific questions addressed by this paper will be of interest to readers of this journal. The paper's findings are potentially significant and should be published. However some significant issues with presentation, the model and the interpretation of results need to be addressed.

An aim of the paper is to use this drainage model to explain oscillatory filling and drainage of subglacial lakes inferred from satellite observations of the Antarctic Ice Sheet surface. Their model fails to explain these observations when channels are allowed to form only by melting upwards into basal ice – the mechanism by which these channels close (ice creep) is too slow to permit oscillations in filling and drainage. However, when channels are allowed to form by the erosion of basal sediments, the model appears to explain the observations remarkable well, with modelled lake volume tracking lake volumes inferred from satellite observations over multiple cycles of filling and drainage. This is apparently made possible by the faster deformation of the sediments (compared to ice creep), which allows the channels to close on a timescale conducive to oscillatory filling and drainage. The authors make the leap from this that in order for a lake to be 'active' (i.e. for it to fill and drain periodically), it must be underlain by sediments. If correct, this is a substantial conclusion. As discussed by the authors, it may explain why active lakes have proved difficult to detect with ice-penetrating radar. An exploration of the model's sensitivities is also informative, indicating which of many physical parameters, which are often poorly-constrained by observations, control lake drainage.

However, due to unclear descriptions of the model and the results of numerical experiments, the validity of these findings is difficult to assess. This is a shame because there are many interesting ideas in the discussion and the match between observed and modelled time series of lake volume change are at first glance impressive. Unfortunately, the physics underlying some key parts of the model (for example the till incision model, see below) are not explained and in several places there appear to be potentially significant errors in the model equations (e.g. Eqns (8d), (10) and (12); see below). These apparently erroneous equations may actually be 'correct' (i.e. describe the physics that the authors want to describe), but as they are not derived or discussed, just simply stated, it is difficult to have confidence that they have been derived correctly. I get the impression that equations have been lifted from several different papers and used here without very much consideration of the physics they were designed to describe. For example, the sheet model used here consists of several equations used by Kingslake and Ng (2013) to describe a linked-cavity system. It is not clear that the concepts used in deriving these equations (e.g. ice flow of bedrock bumps opening the drainage pathway) can be applied to describe sheet flow without modification to the equations. Other potential problems include the definition of the hydraulic gradient (eqn 1), two equations that parameterise momentum conservation (eqn 11 and 12), an equation for conservation of mass in one of the channelised systems (eqn 10) and the calculation of shear stress at the till-water interface (eqn 8d). Either these issues, and several others mentioned below in more detail, are: (1) typos, in which case they need to be corrected, (2) genuine differences in the way the model is formulated in comparison to previous work that will need to carefully described and justified, or (3) mistakes made when the model equations were taken and applied here, in which case the mistakes must be corrected and the model analysis re-done completely.

Furthermore, the poor presentation of the paper does not make it easier to understand. For example, many symbols are not defined either in the text or in the table of variables. Some key parameters that are varied in a parameter search are not explained (e.g. R_1; see below). There are typos where variables are referred to by two different symbols (S_s and h_S; $Q_{cessation}$ and $Q_{shutdown}$) and equations are referenced incorrectly.

Overall the conclusions of this paper have the potential to increase our understanding of subglacial drainage and this work could be published. However, in my opinion, this paper does not adequately describe the methods used to reach these conclusions and some of these methods may in fact be significantly flawed.

Specific comments

Title: As described below I do not see how your modelling tells us about the substrate of the lake itself. The success of the T-channel model only suggests that water from active lakes drains over a sedimentary substrate. Perhaps a more appropriate title would be something like: 'Antarctic subglacial lakes drain through sediment-floored canals – Part 1: Theory'.

p2056, line 9: These aims do not seem to reflect what you do. Aim (i) seems vague, would something like 'increase our understanding of the physics of subglacial lake drainage' be more informative? Aim (ii) is about lake drainage and ice flow. Although ice flow enters the model implicitly through the model of cavity/sheet flow it is not mentioned and I don't really think this work addresses how lake drainage affects ice flow. Aim (iv) mentions parameterising lake activity in ice-sheet models. This is only time in the manuscript that such parameterizations are mentioned (except perhaps implicitly in section 5.2), so I don't think it should be a main aim of the paper.

p2056, line 9: I am not sure that 'T-channel' is the most appropriate name for conduits eroded downwards into the sediment. It is unclear how these are different from features that have been called 'canals' in previous work and erodible/deformable subglacial sediments are not necessarily glacier till, i.e. formed by glacier erosion, for example they could be marine or fluvial sediments. Perhaps a better term for the material is simply 'sediments'. This makes no statement about their genesis.

End of section 1: A paragraph describing the structure of the rest of the paper would help the reader here.

p2057, line 6: Must a channelized system be 'low-pressure'?

p2057, line 6-8: R-channels are only one type of channelized system, this sentence (combined with the previous one) seems to suggest that all channelized systems are called R-channels and '…are thermally-eroded into the ice…'.

p2057, line 9-21: These sentences are hard to follow because they start talking about transient behaviour. Perhaps this is not needed. I suggest replacing these sentences by one sentence or phrase mentioning that R-channels have received a lot of attention in Greenland. Perhaps more obvious reasons for this than the larger gradients and temporal changes in input are (i) the fact that at the terrestrial margin of Greenland you can walk up and see the mouths of channels and (ii) surface water is supplied to the bed at discrete locations from surface-lake/moulin drainage.

p2058, line 5: Almost all modelling of lake drainage is based on Nye (1976). This reference should be here and elsewhere in the model description.

p2059, line 9: In this scenario, where there is a temperature gradient in the ice, some of the heat supplied by the turbulent water will flow into the ice at a rate controlled by the conductivity of the ice and the magnitude of the temperature gradient, but most of the rest will be used in melting. These two energy transfers will happen simultaneously. So I agree, an englacial temperature gradient decreases melt rates for a given heat supply, but the ice need not be heated up before melting can start, as is suggested by this sentence with the words 'first' and 'before'. Also are you missing a reference to Hooke and Fastook (2007)?

p2059, line 9-12: So is the point that lake drainage is observed to stop before lakes are empty, but Fowler (2009) suggests that an R-channel would not close down fast enough to halt drainage after the hydraulic head in a lake has been decreased by only a few meters? Perhaps this can be made clearer here?

p2060, Line 11-13: It is not clear how comparing the output from your R-Channel and T-channel models enables you to 'develop tools to predict future lake drainage' or 'form a conceptual basis for coupling more complex models of sediment and water dynamics.' You do not do this in this paper, so perhaps saying our work can help to do these things in the future is more accurate and in that case does this paragraph belong in the introduction or the discussion rather than here where it reads as a statement of what you do in this paper?

p2060, Line 17-20: The definition of the hydraulic potential in eqn (1) differs from the usual one by a factor of $\rho_w g$, for example c.f. your eqn. (1) and Cuffey and Patterson's (2010) equation (6.7). Also the description of the equation that precedes it in lines 17-19 does not correctly describe the equation; i.e. the second and third terms on the right of eqn (1) are not the 'overburden pressure, minus the effective pressure N'.

p2061, line 4-5: It is not clear until later in the manuscript what this sentence about local minima in θ means, in particular 'up to about 3 m.w.e' is perplexing.

p2061, line 7: This is the first we here about a sheet flow system. I assume it is the same as the distributed system mentioned at the beginning of section 3 (p2060, line 1). Kingslake and Ng (2013) actually modelled a cavity system, so this is a bit confusing. Perhaps you can say clearly somewhere before this paragraph what systems you are modelling and in what combinations: e.g. sheet and R-channel; sheet and T-channel.

p2061, line 9: The model description would be much easier to follow if all the symbols were defined in the main text.

p2061, line 14: Confusing use of the word 'erosion' here when you are referring to melt-enlarged channels. Also it is important to point out that m_R is mass flux per unit length of the channel (as opposed to a volume flux).

p2061, line 15: Missing 'R' subscripts in eqn (2).

p2061, line 17: Equation (3) needs explaining. It is a statement of conservation of energy, but what do all the terms in the numerator on the right describe? What is k_h? It appears in Table 1, but the 'meaning column' is left blank. Also what previous work has this equation come from?

p2061, line 19: K_R does not appear in Table 1. It would be much clearer to mention in this section that K_R and n are constants related to ice rheology and give their values with a reference. Currently the reader needs to frequently find Table 1 to understand what these equations mean and the values quoted there are often not referenced.

p2062, line 1: 'source', is 'transfer' more accurate?

p2062, line 2: Why is the parameter R_k included here? Kingslake and Ng (2013) used this to perform experiments where they varied the strength of the transfer between drainage systems. What is it used for here?

p2062, line 5: This equation needs a reference and an explanation. For example, it varies from Kingslake and Ng's (2013) eqn (3) in its use of $\partial\theta/\partial x$ instead of the basic hydraulic gradient ψ, which is only a function of ice geometry. In your formulation $\partial N_R/\partial x$ appears twice in your parameterisation of momentum balance (eqn 7), once on the left-hand-side and a second time through $\partial\theta/\partial x$ and the definition of θ (eqn 1). This doesn't seem correct to me, but if it is, it needs to be explained here.

p2062, lines 11: 'presumably more precise language' is a strange phrase. The purpose of this whole sentence is unclear .

p2062, line 19: the max function needs to be defined.

Equations (8a), (8b), (8c), (8d), (8e): These equations need to be presented and described properly. Many symbols are not defined either in the text or in Table 1 (e.g. μ_w, μ_T, T, w), and so the equations that contain them cannot be understood. What previous work are these relationships borrowed from? The physics behind these equations needs to be explained otherwise it is impossible to know what assumptions have been made in the derivation of these equations and if they are being applied correctly here. For example, what is the difference between the characteristic grain size and the median grain size and what is the explanation for the dependences of deposition and erosion on these till properties? Equation (8d) describing the shear stress exerted by the water on the till does not include any hydraulics, e.g. water flow velocity. So shear stress is constant. Why is this? If this is an assumption made in the model it needs to be discussed and justified, or if it is a mistake it calls into question the validity of this section of model concerned with the evolution of the T-channel's cross-sectional area.

p2063, line 3-8: this sentence does not make sense. In line 3 the phrase before the colon needs to make sense independently.

p2063, line 11: the sin function has not been defined.

p2063, line 14: Does α_T have units?

p2064, line 1: The only velocity mentioned in this section, previous to the statement here that channel growth depends on it, was the sediment settling velocity. I see in Table 1 that *u* is the mean water velocity downstream. Is this supposed to have made it into equation (8) somewhere? Presently erosion and deposition are both constant (assuming the undefined variables T and w in the definition of the critical shear stress are also constants, which seems likely).

p2064, line 3: There seems to be confusion between velocity, shear stress and flux. Equation 8a seem to state that a critical shear stress must be exceeded, this sentence mentions a threshold velocity and the following sentence says that at 'lower water fluxes channels' do not form. These are not the same, in particular velocity can change without flux changing by the channel size changing.

p2064, line 6: N_infinity was called the till effective pressure previously rather than the till strength as it is called here.

p2064: line 10: m_T is not defined anywhere. It must be the mass of sediment removed from the channel floor per unit time per unit length of the channel. However, if so, the first term on the right is dimensionally incorrect. In fact, if you assume that sediment doesn't change volume as it is eroded (like ice does when it melts) this term should disappear, leaving $dQ_T/dx = C_{vT} + T_T$. The analogy is eqn (5) with two identical densities. The derivation of this equation should be explained fully as it varies in form significantly from eqn (5).

p2064, line 15: Why is f_T underlined here?

p2065, line 2: R_1 needs explaining properly, not least because it is varied in a set of experiments later.

Equation (12): The dependence of Q_S on $d\theta_S/dx$ is possibly incorrect. c.f. this equation to Kingslake and Ng's (2013) equation 9. Perhaps $d\theta_S/dx$ should be replaced by $(d\theta_S/dx)^{1/2}$. This and the other potential errors in the equations mentioned above have the potential to change the dynamics of the model considerably. Either the analysis needs to be re-done with the correct equations or this different model needs to be derived properly and the differences to previous work justified.

p2065, line 4: Is h_s the same as S_s?

p2065, line 6: How are T_{SR}, T_T and T_R related?

p2065, line 6: Explain the source term M_C. Also why has it got a 'C' subscript?

p2065, line 12: Eqn (14) does not say that S_s and N_s are inversely proportional. They are inversely related, as n and q are positive, but not proportional because n+q ≠ 1.

Equation (14): This equation was used by Kingslake and Ng (2013) to describe a cavity system, in contrast to its use here to describe a sheet system. This should be justified. For example, implicit in this equation is cavity opening by sliding of the ice sheet over bed obstacles of height R_1. How does this apply to ice sliding of deformable sediments? Also it should be mentioned that this equation assumes a steady state cavity geometry.

p2065, line 14: There should be a section entitled 'Simplifying assumptions' but this section is mostly about the lake.

p2065, line 17: What does 'changes uniformly' mean? In time during drainage? Across the lake?

p2065, line 18: What does 'conditions as the flowpath crosses the source lake' mean?

p2065, line 19: Why do we have a different t for the lake: t_L?

p2065, line 19: $z_{srf} \rightarrow z_s$

p2066, line 1-2: Equation (16) describes change in hydraulic potential in the lake not lake volume as stated in the phrase in line 1.

p2066, line2 5-8: Can you explain H_L a bit more? What do the end members of H_L = 1 and H_L = 2 correspond to in terms of bridging stresses? Also what dimensions does H_L have?

p2066, line 19: Is it worth pointing out here that θ_0 and the seal position are controlled by ice sheet geometry?

p2066, line 22: We need an introductory paragraph describing what experiments you want to do before going into details about the initialisation procedure.

Section 3.4.1: This section is very difficult to understand and I think the equation references are all wrong

p2067, line 15-17: Again, is there a confusion between critical flux and critical shear stress here? Also isn't this built into the sediment erosion model already, why does it need to be put in artificially here? What is the physical explanation for this modification in the R-channel case?

p2067, line 19-22: This means very ad hoc. It doesn't seem as if the model conserves mass with this instantaneous creation of a channel and it is not clear why it is necessary. Why is it not possible to simply let the channel shrink away to nothing when it wants to, then grow on its own?

Section 3.4.3: It took me several readings of this section to understand what you have done. When you use dN/dx to find the next value of N at each grid point, do you start from the lake with the known lake effective pressure? If so, this is good because the channel is pressure-coupled to the lake, but how is the lake effective pressure defined? And how is it related to θ_L which is what is evolved forward by eqn (16)? Line 11 is the first we hear of a grid or any descritisation scheme, can you describe this? In line 16, is the 'known' N_R = 0? In lines 18 and 19 are 'downstream lake' and 'destination lake' the same? The adaptive time step scheme (line 21-25) needs more explanation. How is the next timestep calculated so that your criterion on the change in S is not violated. Line 21: what boundary conditions do you use for the distributed system?

p2069, line 7-8: What constant values are y_s and h_i given here?

p2069, line 8: This is the first time Q_b has been mentioned. How does it affect the model?

p2069, line 20-23: From looking at figure 3 it looks like the hydraulic potential calculated from observations must have been smoothed. If so, how is this done?

Section 4: Broadly the set of experiments conducted seems appropriate, but the descriptions of them and their results are perhaps lacking. Frequently the reader is not pointed to figures containing results before the results are discussed. This makes these discussions very difficult to follow. The fundamental physics of the oscillatory behaviour needs to be explained. The R-channel model is dismissed very quickly without an exploration of its parameter space.

p2070, line 1: This section needs an introductory paragraph describing what experiments you did and in which upcoming sections you discuss each one.

p2070, lines 4-5: Have you explored the parameter space of the R-channel model? Here it reads as if you only did one run and gave up on it.

p2070, lines 5-7: Figure 4 is very unclear, but it looks like peak elevation (if this means lake level or similar) peaked after three years rather than the 10 years mentioned here.

p2070, lines 8-9: The comment about 'temperate ice frozen to the base' is odd here. The basal ice will be temperate, if it wasn't neither a R-channel or a T-channel could survive. Furthermore, if the englacial temperature gradient were steep and removing a lot of heat, even a T-channel would shrink due to freeze-on – just because the channel is eroding sediment doesn't mean it isn't in contact with the ice or it is immune to closure cause by cold 'polar ice'.

Section 4.1.1. I am not completely convinced that these findings mean that active lakes cannot drain through R-channels. During a model run lasting 8 years the R-channel model oscillated slower than some observed active lakes. Firstly, could this be due to the fact that this is the period of time over which we have observations and there are other lakes that oscillate at much longer timescales? Secondly, isn't the period of oscillation sensitive to many unconstrained parameters (e.g. input from the distributed system; input to the lake; reaction of the downstream lake to filling, etc.). Perhaps you can present a more thorough exploration of the R-channel model to rule out the possibility that it could explain the observed active lakes with a different set of parameters. Thirdly, it is not clear how the approach you have taken to switch on and off channelized drainage applies to a R-channel. Finally, Fowler (1999) and Kingslake (2013) showed that environmental conditions, like water supply to the channel and lake, can affect the period and magnitude of oscillations through the dynamics of the *moving* subglacial water divide (different than the 'seal' as defined in the current work). These dynamics may not be included here (see my comment immediately below this one). So a more complex R-channel model might do a better job of matching observations. These possibilities need to be explored or at least discussed before the R-channel model can be dismissed.

p2070, line 13: The reader could be pointed towards the results discussed in this section. Also we need to have a description of the physics of these oscillations. Are they similar to Fowler (1999) and Kingslake and Ng (2013)? Does a water divide form between floods?

p2071, line 5-6: Is 'has been shown to hold true for several real lakes.' referring to the results discussed later in section 4.3? This should be saved for the discussion as these results have not yet been presented.

p2071, line 8-9: This seems a bit misleading. Parts of these papers demonstrated that in special cases when spatial variability is ignored (i.e. when their models were reduced to become zero-dimensional), oscillations grow unstably, but in both papers the oscillations simulated with the full models either die away over time, like they appear to be doing in the current work, or reach stable limit cycles.

p2071, line 15-16: Could point the reader to where they can see the results of this comparison.

p2071, line 17-22: It would be more interesting to know why there is little difference between the idealised and realistic runs in terms of the physics rather than the differences in computer run times.

p2073, line 21: Describe what experiments you did at the beginning of this section.

p2073, line 21: It is not clear that the inverse relationship is linear.

p2074, line 21-25: It is not clear how this discussion of obstacle size relates to the physics described by the sheet model. As mentioned above, the equations you use to describe sheet flow are taken directly from a model of cavity flow (Walder, 1986; Hewitt and Fowler, 2009; Kingslake and Ng, 2013). In the cavity model, water is thought of as flowing round obstacles behind which ice flow creates the cavities. There is no notion of water overcoming the obstacles. I am not convinced that these concepts can be applied here, but if you think differently the model needs to be derived properly and these ideas discussed in the model derivation.

p2075, line 26-28: From looking at figure 9d, it looks like the frequency of drainage events is about right they are just 180^{o} out of phase with the observations.

Section 5: Even ignoring the potential problems with the model formulation, I do not think that the model results sufficiently back-up some of the conclusions drawn from them in this section.

p2076, line 14-20: I do not understand why a major implication of your modelling is that lakes are shallow. Also, even if we trust the finding that the T-channel model is superior to the R-channel model in reproducing observations, this only actually indicates that the channel needs to be formed in sediment, it doesn't seem to imply much about what the lake sits on. Surely all we need, to overcome the problem of the channel not closing fast enough to allow oscillations, is a patch of sediment somewhere along the drainage pathway.

p2076, line 20: I do not see how your modelling casts doubt on the conclusions of Le Brocq et al. 2013). Apart from the points raised by Le Brocq in a comment in the interactive discussion of this paper (Short Comment, C359; their model does not require a channel in the ice upstream of the grounding line), the T-channel model does not rule out melting of an ice channel over the top of the till channel; till could be eroded and be squeezed back into the channel episodically as the lake fills and drains, while the ice is steadily eroded. This presumably could form an esker.

Figure 3: Missing label on horizontal axes.

Figure 4: This figure is very confusing. Instead of having two sets of results on the one panel, would it be clearer to have outflow in one panel and 'Peak Elevation' in another? Also does peak elevation mean lake surface elevation or something similar? 'Peak elevation' suggests the lake highstand to me. The vertical axis needs labelling. Looking at the plot of channelised component of the flow raises the question of when the $Q_{cessation}$ and Q_{onset} come into play. It looks like channelised drainage is always present after the initialisation in this simulation. Can this be explained in the main text?

Figure 5: Missing a label for the horizontal axes. Include the symbols for alpha, till effective pressure and sediment size in the panels and explain what they are in the caption.

Figures 4 and 5: Labels and lines in legends are not aligned properly.

Table 1. Densities of ice, till and water are incorrectly given the following symbols: P_i, P_T and P_w. k_h has no entry in the 'meaning' column. 'Glenn's' → 'Glen's' in at least two places.

Typing errors

As addressing all the specific issues above will likely involve re-writing large sections of the manuscript, I have not been through the manuscript and taken note of the numerous typing errors. These are just a few of the more obvious errors.

p2069, line 1: Typo in section title.

p2070, line 2: Typo in section title.

p2071, line 21: Extra 'the'.

p2071, line 23: Typo in section title.

p2071, line 25: Typo here, possibly missing a 'to'

p2072, line 17: Are these the correct equation references here? Should it be eqn 8a and 8b?

p2073, line 5:Do you mean you multiplied these two quantities by a factor?

p2075, line 10 and 14: Is $Q_{shutdown}$ the same as $Q_{cessation}$?

References mentioned in comments

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