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Interactive comment on "Active lakes in Antarctica survive on a sedimentary substrate – Part I: Theory" *by* S. P. Carter et al.

Anonymous Referee #1

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General comments:

This is a well-motivated study that attempts to model subglacial lake filling and draining of the style inferred in the active lake regions of Antarctica. The authors construct two models, both adapted from previous work by the Fowler line, to explore the plausibility of Rothlisberger-channel versus "till-channel" (canal) drainage between lakes. They explore model behaviour on synthetic topography, quickly discard the R-channel model, and pursue simulations that emulate the observed signature of lake filling/drainage for four different Antarctic lakes over multi-year periods. The fact that the authors achieve a qualitative match with observations seems impressive, though the rather complex model construction, numerous model parameters and several ad-hoc model interventions (e.g. discharge threshold for channel initiation/termination) could make the reader

somewhat ambivalent about the results. Due to some fundamental confusion about the model formulation, I am left unable to judge whether the match with observations serves as a justified (albeit qualitative) validation of the model. While the suggested tendency for Antarctic subglacial drainage to favour canals over R-channels seems plausible, I don't think the authors did all they could to discredit the R-channel model (details below) or went far enough in explaining the till channel model and demonstrating its robustness. Some of the implications/conclusions seemed over-reaching, given the model limitations.

To allow a more transparent evaluation of the model, I suggest the authors focus on cleaning up the model presentation, and make clear what elements are borrowed versus original. The models that form the basis for those in the paper are cited and properly credited, but it would be very useful if the authors provided specific details on what aspects of the existing models were adopted, adapted or replaced. In reading through the equations it is not clear where a new formulation is being introduced versus simply taken from another study. This gets tedious with equations that include unfamiliar (to me at least) constants like the 6.6 in (12). In addition to more precise attribution of borrowed model components and clear highlighting of what is new/different, it would be very useful to have variables and parameters (except the universally obvious ones) defined in the text. While Table 1 is great, I found the constant flipping back and forth distracting.

Fundamental to evaluating model performance is an understanding of how lake surface elevation is modelled and how it is related to ice surface elevation (presumably the observation). I think this is tied up in Equation 15, which I cannot parse.

Given that the R-channel model is quickly dismissed in the paper, the authors might consider omitting its description altogether and simply summarizing its inapplicability. Before doing this, however, I recommend the authors do all they can to show that it is impossible to emulate the data with this model. For example, Glen's flow-law coefficient appears to have been held constant in all tests and at a value stiffer than that for temperate ice (which made sense until deep into the paper when it was stated that the ice was assumed temperate). What happens if this parameter is varied? Can different model behaviour be elicited? If this conceptual model is to be discredited, along with other studies that invoke it, the authors need to show that under no plausible circumstances and within no plausible range of parameters can reasonable behaviour be elicited.

Specific comments (page.line):

2055.29. See also Peters et al. 2009. Numerical analysis of rapid water transfer beneath Antarctica, Journal of Glaciology, 55(129), 640-650.

2057.6. Since low pressure is only obtained for steady state, and there have been numerous demonstrations of channels not exhibiting low-pressure steady-state behaviour, this statement seems an over-generalization. Suggest rewording.

2058.24-25. Clarify that borehole observations apply to basal environment, not to lakes in particular. Also, it's not clear why Carter et al (2009) is cited here – are borehole data used at all in this study?

2059.27. Further explanation should be given for the choice of the Kingslake and Ng (2013) model, as distributed drainage in K&N is treated as a system of interconnected hard-bedded cavities. Since the current paper makes the point that the active subglacial lakes exist in a sedimentary environment, it would seem that a fair comparison between R-channels and till channels would employ a sediment-based sheet rather than a system of cavities to describe distributed drainage. Later in the model description the distributed system is described more like a sediment sheet; if the authors have replaced the K&N cavity system with a different sheet, this should be stated at the outset.

2060.20 (Eqn 1): Is there a good reason for writing the hydraulic potential in this awkward way with three terms? Also, as written, Eqn 1 is more conventionally termed

hydraulic head. Why not just write it in one of the usual ways: hydraulic head h = z_b + psi (where psi is pressure head = Pw/(rhow g)) or hydraulic potential phi = rhow g z_b + Pw ? Writing hydraulic potential in the form of Equation 1 also makes Equation 3 unnecessarily complicated.

2060.22. Please write out the definition of theta_0 in this case (= zb + (zs – zb)rhoi/rhow presumably). This is relevant later for understanding the onset of lake drainage.

2061.5. Not clear where 3 m w.e. comes from (mentioned later in the text). Please explain.

2061.17 (Eqn 3). Given the definition of theta in Eqn 1, and my guess that k_h = ct*cw*rhow $\sim$ 0.3 (according to Table 1), I think Eqn 3 has a sign error and an extra term k_h dN_R/dx. The melt opening term as written in Eqn 2 (m/rho) implies that m_R in equation 3 should be:

m_R = (Q_R/L_h)*(-rhow*g*dzb/dx – (1-k_h)*dPw/dx),

where zb = bed elevation and Pw is water pressure. When I manipulate Eqn 3 as given, I get:

m_R = (Q_R/L_h)*(rhow*g*dzb/dx + (1-k_h)*dPw/dx + k_h*dN/dx). Please verify the form of this equation, given the definition of theta in Eqn 1.

2062.5 (Eqn 7) I think this formulation comes directly from Kingslake and Ng, with a modified definition of theta and the inclusion of an evolving water pressure in the definition of theta (K&N assume the pressure potential gradient is defined by ice geometry alone). Please note these details in the text.

2062.12. Hewitt (2011) gives a nice discussion of possible distributed drainage morphologies but settles on a mathematical treatment that does not include canal incision or any other sedimentary processes. I don't think this citation belongs here.

2062.19 (Eqns 8a – 9) Please elaborate on the precise source of these equations and their forms.

2063.11. Equation (9) intentionally allows both inward and outward creep of sediment?

2064.8 (Eqn 10): m_T should be the 'opening' term, so is it not just identical to E_T – D_T? If not, please explain; if so, please note this.

2065.8-9. 'only way for water to clear obstacles' Not clear why this is relevant. It doesn't seem relevant in 2-D reality where water can go around obstacles.

2065.19. (Eqn 15). I'm confused. Text says this is the change in lake level: dzb/dt = dz_srf/dt. dzb/dt seems like the change in bed elevation with time, but this doesn't make sense. (I assume zb is bedrock, rather than the top of a sediment layer, but a change in the elevation of the sediment layer still wouldn't make sense to me as reflecting a lake level). What is z_srf (it seems to be missing from the table) – the lake surface elevation (i.e. base of the ice)? This seems like a critical part of the model that I do not understand.

2066.12. 3m w.e. was determined to be a good threshold for defining the existence of a lake?

2066.15. If downstream sediment transport is neglected, does this mean that any sediment eroded (E_T) is assumed to leave the system instantaneously? Where does the sediment that contributes to deposition (D_T) come from? These processes could be better explained as they are introduced in the model.

2067.10. The threshold approach to channelization is also used by Hoffman and Price (Hoffman, M., & Price, S. (2014). Feedbacks between coupled subglacial hydrology and glacier dynamics. Journal of Geophysical Research: Earth Surface, 119(3), 414-436.) and in a less similar manner by de Fleurian et al. (Fleurian, B. de, et al. A double continuum hydrological model for glacier applications. The Cryosphere 8.1 (2014): 137-153.).

2068.19. Please specify criterion that defines 'satisfactorily close' if there is one. (Also

applies to 2069.23 'We validated the model' – how is validation established?)

2069.8. Prescribed discharge range also from Carter and Fricker 2012?

2070.6. I understand that a discharge threshold is used to govern the timing of channel initiation, but it is not clear what happens when this threshold is reached. For the case of the R-channel, is a channel of some small size assumed to exist instantaneously once the discharge threshold is obtained? Is the timing of R channel growth (10 years) reported here at all affected by the way in which the channel is arbitrarily instated? This seems potentially important.

2070.23. 'lake drainage began when theta_0 was still…'. This may be cleared up if theta_0 is explicitly defined (see above), but if theta_0 is the hydraulic potential when N=0, it is a static quantity that depends only on ice geometry. I'm not sure what it then means to state that lake drainage occurs while theta_0 is still below the value at the seal. My understanding of theta_0 would suggest that it does not change in time, unless there is substantial variation in the surface slope or ice thickness. I may be misunderstanding this aspect of the model owing to my confusion about Eqn. 15.

2072, Section 4.2.1. This section did not seem to provide much insight into why the three parameters under investigation affect the period and amplitude of modelled floods. That seems important and would also be interesting.

2073. Section 4.2.2. This section should probably address the fact that the model is a bottleneck model where the seal is stationary (c.f. Clarke 2003). Perhaps the authors could speculate on how the results would be different if the seal were permitted to move according to the evolving hydraulic potential profile.

2074.5-11. Please cite Figure 7c,d here and describe results in reference to the figure.

2074.24. I don't see how this is paradoxical. It seems to me that anything that inhibits flow should lead to more pronounced flood cycles than conditions that promote quasi-steady drainage. Perhaps I'm misreading this.

2075.1-3. It is surprising that the onset threshold is not an important parameter. In Table 2 it is varied from 0.75-3.5. What happens if it is 0.1 or 10? It seems that its apparent insignificance may be due to the limited range over which it was tested (at least in the simulations presented).

2075.9-15. I'd condense this text and get rid of the graphics. How is model 'accuracy' assessed? It doesn't seem there are any true tests of model accuracy, so it is hard to know what this means. Similarly, what is premature shutdown of the channelized system? How is this judged?

2075. Section 4.3. Missing from this section is an explanation of what the parameters required to fit the drainage events from the different lakes tell us about the lake environments. Are the parameter differences meaningful, or are there so many tuning parameters in the model that interpretations of their values cannot be made?

2075.20-25. I'm not clear on what variable is actually being compared here (and in Figure 9). I'd assume the observations have to be ice-sheet surface elevation. Is lake surface elevation taken as identical to ice-sheet surface elevation for the purpose of the model? Could this not be a poor approximation on the multiyear timescales involved in these lake flood events?

2076.20-23. Given that the LeBrocq et al study covers multiple sites in Antarctica, and the present study does not consider the possibility of sedimentary canals roofed by thermally eroded ice, the doubt casting that occurs three times in this paper seems over-emphasized and a little off-putting. While I am inclined to think canals are more plausible than R-channels under Antarctica, the current study cannot rule out the possibility that some R-channel-like features could develop under the grounded margins of the ice sheet.

2077. Section 5.1. One major conclusion of this paper is that active lakes and their environs are likely to be (or must be) underlain by sediments. This idea seems partly based on previous work. Is the primary evidence provided here that the R-channel

model does not produce results that look like the observations, while the till channel model does? If so, the authors should clearly state that this is the foundation of their reasoning.

2078.1-3. I am left wondering whether the model definitively demonstrates that sediment-floored lakes will undergo episodic filling/draining. What if there were no ad-hoc threshold set for channel initiation/termination? Might these channels not be more persistent and suppress episodic drainage in some cases?

2079.3-6. 'drainage of subglacial lakes in regions of fast flow actually results in net slowdown'. I don't see any evidence for this conclusion in the paper. Canals (T-channels) behave more like distributed than classical channelized drainage systems; moreover, the authors have considered only a single channel, whereas the real system may be comprised of multiple (braided?) channels, making this much more like a distributed drainage system. I may be missing the point, but I don't see why active lakes would result in net slowdown.

2079.14-28. Ditto earlier comment on skepticism toward LeBrocq et al.

2080.8. 'previously accepted' is misleading here. The model of K&N was previously accepted as a model of glacier lake outburst floods in an alpine setting. It was not a previously accepted model of Antarctic lake filling and draining.

Figure 3. I'm confused by the way hydraulic potential is shown here. Is the value plotted assuming zero elevation head? I would expect to see the hydraulic potential, if in m w.e. as it is here, lie between the bed and the ice surface. Would this be the case if the bed elevation were added to the potential? It would make more sense to me if potential were plotted in the standard way as the height to which water would rise in a borehole (the potentiometric surface).

Figure 8: Suggest omitting panels b and d. This model does not introduce new or sophisticated numerics, so plotting the grid refinement test and simulation time (where

one would need to include machine specifications to make this meaningful) seems odd. The results of these numerical tests can be stated in the text but do not need to be shown.

Technical corrections/queries (page.line):

Throughout: Some letters seem to have been dropped from the ends of words in section/subsection titles (perhaps a problem with TCD template)

Title: I suggest extending the title with something to the effect of “and model testing on real and idealized domains” to better reflect the content of the paper.

2054.10. “T-channels” already have a name in the literature: canals. I’m a bit averse to the attempt to introduce new terminology here when it is not needed and it doesn’t fit the existing convention of “X-channel”, where X is the first letter of the last name of the person to whom these features are attributed (e.g. Rothlisberger, Nye).

2054.11 and elsewhere: in general I would advise defaulting to present tense for this sort of modelling study, e.g. reproduced => reproduces on line 11 of the abstract. In many places I found the past tense confusing.

2054.19-20: ‘ice’ repeated

2055.29: the => to

2057.2. modes => models ?

2057.16. ‘e.g.’ needed before Andrews reference since this point has been demonstrated elsewhere.

2058.27. is => are

2059.1-10. This section would be clearer if all the reasons were listed together in the first sentence, followed by the elaboration.

2060.8 and elsewhere. Use of “combined R-channel model” is confusing, because

it often reads as though R-channels and till channels are combined in a model. I think “combined” always refers to the channels (R or till) being coupled with the sheet. Suggest just omitting “combined” in all cases.

2060.8-9: I don’t see how these three objectives follow from “comparing lake volume timeseries output by the combined R-channel model and the T-channel model”. Should this read “comparing simulated and measured lake volume timeseries. . .”?

2060.17. Suggest “drainage system elevation” or “elevation of the point of measurement”

2061.1-4. Sentence in need of punctuation or shortening (continued from p. 2060).

2061.15 (Eqn 2). m => m_R ? rho should probably be subscripted with ‘i’ since rhow and rhoi have already been introduced.

2061.19 (Eqn 4): K_R must be Glen’s flow-law coefficient modified for channel shape (∼2A/nˆn ?), but it is not in the table.

2062.12. Hewitt et al => Hewitt

2062.13. Kryke => Kyrke

2064.4. delete “in the”?

2064.5. effective pressure of the water => effective pressure (delete ‘of the water’)

2064.14 (Eqn 11) Since the form of this equation is identical to that of (7), it seems they should be described in the same way. (7) is introduced as an adaptation of the Manning equation and (11) as the conservation of momentum.

2067.4-7. Some of the equation references are wrong here: 2 => 12, 3=>13, 4=>14.

2068.13. gird => grid

2088 (Table 1): Units => Dimensions Glenn => Glen pa => Pa Description of k_h missing (I think it is cp*ct*rhow) Dimensions of vs (mean settling velocity) really L/Tˆ2,

not L/T?

2071.21. the => that?

2071.25. response to changes

2072. The writing seemed to take a slight turn for the worse here, becoming more conversational (e.g. using ‘decent fit’ and other not-so-useful subjective judgments) and containing more errors, e.g. line 17: there are no equations 6a and 6b. I've stopped making editorial comments from here on. This just needs a good proof-reading.

2078.5. Fricker et al (2014) not in reference list

Figure 5: x axis label missing (Time in years)

Figure 9: Caption suggests that observed and modelled lake-surface elevations are shown, along with modelled outflow from distributed and channelized systems. I see only one curve per panel (dashed blue) representing a model result. Where is the modelled outflow?

Interactive comment on The Cryosphere Discuss., 9, 2053, 2015.