**Review of 'Antarctic subglacial lakes drain through sediment-floored canals: Theory and model testing on real and idealized domains' by Sasha. P. Carter, Helen. A. Fricker and Matthew. R. Siegfried**

**Jonathan Kingslake**

I reviewed a previous version of this manuscript (doi:10.5194/tcd-9-2053-2015), and under the direction of the editor have restricted my comments below to the responses of the authors to my comments.

The authors have done an excellent job of responding to my comments and the revised paper is hugely better than the original submission.

I thank the authors for taking my comments so seriously and persevering through what must have been a hard revision process. I am really pleased that they did persevere and, as I say, in my opinion, the paper is much better for it. It now presents a more compelling argument for the drainage of active lakes being controlled by sediment erosion and deformation rather than ice melting and creep.

What follows is a discussion point that might be interesting and a few comments on the details of the rebuttal.

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One exciting implication that occurred to me when reading an added sentence (page 17, lines 14-16: "Work by Carter et al. (2013) has inferred that the filling rate for SLM varied between 2.25 and over 50 m3 s−1 and was controlled primarily by outflow from SLC, suggesting that the misfit could reflect the poor assumption of non-varying Qin."), is related to the predictability of lake drainage.

I think that one of the things that this work supports is the idea that the filling and drainage of subglacial lakes is controlled by the same fundamental physics as those described by more traditional models (e.g. Fowler, 1999; Evatt et al., 2006, Kingslake and Ng, 2013; Kingslake, 2015), which only really considered R-channels. In the 2015 paper (Kingslake, 2015; Kingslake, J., 2015. Chaotic dynamics of a glaciohydraulic model. Journal of Glaciology, 61(227), pp.493-502.) I showed that an R-channel model can behave in a few interesting ways like nonlinear oscillators when it is supplied by a time-varying input - i.e. it can be chaotic and fundamentally unpredictable beyond a certain time in the future. In that paper I speculated that this could happen in subglacial lakes, but I stopped short of speculating on the implication for the fundamental unpredictability of ice-sheet dynamics.

Carter et al mention in the sentence quoted above ("…filling rate for SLM varied between 2.25 and over 50 m3 s−1 and was controlled primarily by outflow from SLC…") that lake input is controlled by outflow from other lakes. This is exactly what I speculated would be needed for chaotic dynamics to be produced by a subglacial lake system. Now that it has been shown that the same fundamental physics apply to subglacial lakes (albeit with effective-pressure-dependent viscous ice flow replaced by effective-pressure-dependent viscous sediment creep and discharge-dependent ice melt replaced by discharge-dependent sediment erosion), perhaps this connection is worth thinking about.

It potentially says something quite fundamental about the predictability of ice sheets! Because water pressures control ice flow and because lake drainage and filling controls water pressure and because lake drainage and filling could be chaotic, could ice-sheet dynamics behave chaotically? It would be

quite fun to hypothesize that the details of ice-sheet dynamics can never be predicted beyond a few fill-drain cycles into the future.

Anyway, just a suggestion, but maybe the authors would like to think about these ideas and maybe add a paragraph in the discussion if they think it's interesting.

From table 1, the parameter $R_{kRC}$ is equal to 0.05. Does this mean that the transfer between drainage systems is 20 times smaller than in previous work (Hewitt and Fowler, 2009; Kingslake and Ng, 2013)? Admittedly these values are highly uncertain, but I was thinking that this might be the explanation for the weak sensitivity to the distributed system supply term MC.

I think a typo remains in eqn 12 after the correction. Should the exponent of $(d\theta\_s/dx)$ be ½ rather than -1/2, so that discharge increases with hydraulic gradient?

A small point is that the subscript 'C' in the source term in the eqn (13) has not been changed as mentioned in the rebuttal.

It has not been explained that eqn 14 assumes steady-state.

I cannot find the following passage that is mentioned was added in the rebuttal: "If the model was allowed to continue to run for longer timeframes, however, then it was possible for discharge to increase. Even in a domain with a perfectly horizontal ice base the channel still grew too slowly taking 12 years to drain back to the initial lake level (Figure 5b, 5c)."

I am sorry to say that I still do not understand eqn 7. If you differentiate eqn 1b to get $d\theta/dx$ and substitute this into eqn 7, it seems to me that $dN/dx$ cancels and you are left with an equation that does not include the effective pressure.

In summary, I am really pleased that the authors have produced such an interesting and well-presented paper. I expect it will be well-read and useful and as I mentioned above it is interesting to think about its immediate implications for, among other things, the predictability of ice sheets.

Best wishes,

Jonny