Dear Dr. Bingham,
Thank you for the time, consideration and assistance throughout this process. Below is our response to the reviewer. Reviewer comments are in regular type, our responses are in italics

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.
We thank you for your time energy and consideration. This paper truly could not have been what it has become without your insight and attention that went far beyond the call of the standard reviewer. Reading your words has made all the efforts feel worthwhile and we hope more good does indeed come of it.

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.

1. Carter et al mention in the sentence quoted above ("…filling rate for SLM varied between 2.25 and overm50 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.
In short, we have thought about this a lot, especially in light of your recent J. Glac paper on chaotic lake drainage dynamics. Indeed, we actually are working on a model where inflow varies with time and, when several lakes are chained together, the dynamics can turn chaotic quite quickly. However, we felt this next step was beyond the scope of the current paper, which was establishing that the canal model as a viable alternative to R-channels, and therefore did not include it in this manuscript.

2. From table 1, the parameter RkRC 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.
Our value for RkRC is indeed really small. We struggled with model compilation at higher values. We now include the following sentence in Methods (p. 8):
"It should be noted that our value for R_{kRC} is near the lowest end of values explored by Kingslake and Ng (2013) primarily due to model stability issues."
And in Results (p. 19):
"This low sensitivity lmayikely results from our low value for R_{kRC} which limited the transfer of water between the channelized and distributed systems"
3. 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?

We inspected the equation as it appears in the paper and in the code and agree with you that -1/2 should be 1/2. We also corrected the preceding term in this equation, where sqrt(6.6 rho_w g / f_r) was meant to be sqrt(6.6 / rho_w g f_r). The latter representations are all consistent with how this was coded. We apologize for these typos.

4. 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.
We have now changed the subscript to ‘S’ everywhere to maintain consistency with other variables related to the distributed system.

5. It has not been explained that eqn 14 assumes steady-state.
After eqn 14 we have now added the language, “This formulation, assumes N_S reaches steady-state instantaneously. Thinner water layers (and therefore higher values of N_S) are maintained over hydropotential maxima, while thicker water layers (and therefore lower values of N_S) are maintained over hydropotential minima.”

6. 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).”
This is likely an issue related to differences between the manuscript you reviewed (submitted in March 2016) and a previous version that responded to your original review (submitted in December 2015). Looking through the response letter, the original comment concerned discrepancies between the figure illustrating the R-channel dynamics and what was written in the text. The dynamics of the R-channel are now illustrated in Figure 5; the text quoted above was removed from the manuscript and was replaced by the following language concerning the time necessary for outflow to exceed inflow:
“From the start of the model run, it took nearly 10 years for a significant channel to begin growing, by which time the stiffness of the ice was too large to halt the lake drainage once the lake drained back to its initial level (defined as 0~m). Only after draining for almost 10 years and losing almost 16~m of elevation from its high stand did Q_{out} fall below Q_{in} and lake volume began increasing.”

7. 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.
We took a close look at this equation in the paper and in the code. We have now rewritten the last term as “rho_w g\frac{\partial \theta_{0}}{\partial x}”

*With the reformulated equation, the hydropotential gradient in the channel (partial theta_RC / partial x) is equal to the base hydropotential gradient (rho_w * g partial theta_0/partial x) minus the gradient in effective pressure (partial N_RC / partial x) consistent with equation 1b where theta_RC = rho_w*g theta_0 – N_RC. This is consistent with equation 3 from Kinglake and Ng (2013).*

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.

We could not have done it without you.