The Cryosphere Discuss.,
doi:10.5194/tc-2016-113-AC1, 2016

The Cryosphere Discussions

Interactive comment on "The past, present, and future viscous heat dissipation available for Greenland subglacial conduit formation" *by* K. D. Mankoff and S. M. Tulaczyk

K. D. Mankoff and S. M. Tulaczyk

mankoff@gmail.com

Received and published: 29 June 2016

We thank Anonymous Referee #1 (AR1) for providing insightful reviews. We respond here to the two reasons AR1 is hesitant to recommend publication. We summarize these reasons as (1) nothing new and (2) convoluted explanation of methodology.

For (1), we agree with AR1 that the calculations follow a long-established methodology for treating energy balance during water flow through glacial systems, but we ask for clarification about the statement from AR1 that there is nothing new presented here. Our intent was not to provide new insights into the theory of subglacial hydrology, but rather to apply the well-established theory to the entire Greenland ice sheet over three time frames spanning from recent conditions to the predicted future state of the ice sheet.

There are many theoretical papers modeling subglacial hydrology (from Röthlisberger (1972), Weertman (1972), or Nye (1976) to Schoof (2010), Hewitt (2011) or Mauro Werder et al. (2013)). There are fewer papers examining energetics of meltwater drainage in Greenland, and the few existing ones are focused specifically on the present and the southwest sector of Greenland. Only a few that we know of address how it may change in the future (for example, Banwell et al. (2013) and Mayaud et al. (2014), cited in our Introduction section). We are not aware of any that consider future subglacial hydrology outside of the southwest sector of the Greenland ice sheet.

If AR1 has a reference to a paper, or papers, that discusses quantity, effects, and potential impacts of changing subglacial hydrology historically, at present, and in the future, for all of Greenland, we would greatly appreciate it if AR1 can provide such reference(s) as it appears that we may have missed some highly relevant publications.

For issue (2), we can simplify the explanation of the methodology. Although the energy balance of water flow has been resolved a long time ago and we are not trying to change this we, did not think that simply citing Röthlisberger (1972) was sufficient, and opted to include a more detailed methods and assumptions section.

Responding to some other points raised by AR1, we note that:

+ The results of this study do not "essentially re-express the modelled increase in surface runoff [. . .] in terms of gravitational potential energy". Our results express the modelled increase in surface runoff in terms of J and W/mˆ2 at the bed, after accounting for along-path energy losses and flow routing.

+ We can clarify our treatment of the outflow elevation and remnant gravitational potential energy due to that elevation when outlets are above sea level. In section 3.6 we state, ". . .ideal scenario of a flat bed and outflow at sea level". In the full analysis we do know and use the outflow elevation. The point of the hydrology routing is that

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the spatial changes in elevation and thickness matter inland of the outflow location, as these changes induce spatial variability in heat released at the bed.

+ AR1 is correct that the pressure melting point is not zero sum, but we explicitly state that this manuscript is only focused on heating at the bed, as they point out in the comment.

We are grateful for the comments from AR1 and agree with the remaining issues raised by AR1. We will address them fully if given an opportunity to revise this manuscript.

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