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Discussions

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Interactive comment on "On the possibility of a long subglacial river under the north Greenland ice sheet" *by* Christopher Chambers et al.

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Review of Chambers et al

Sorry for the delay, I was doing field work last week. I wrote parts of my review before other comments were posted, thus some of my comments may be redundant.

Best wishes,

Andy Aschwanden, University of Alaska Fairbanks

Review of "On the possibility of a long subglacial river under the north Greenland ice sheet"

I was quite excited when I saw this topic presented at AGU 2018, and happily accepted

the review. The manuscript is generally well written and I find the idea of assessing the impact of the canyon on ice flow a worthwhile endeavor. Unfortunately in its currently form the study is poorly designed and a missed opportunity.

First (as already pointed out by Jonathan Bamber), the title is misleading because Bamber et al (2013) [B13a] already demonstrated the existence of a long subglacial channel. I suggest to change the title to something that more closely reflects the core of this manuscript. E.g. along the lines of "On the influence of a long subglacial channel on basal hydrology and ice flow".

The hydrological and ice flow modeling, which is the most novel aspect of this manuscript, is ill-conceived. The authors compare a simulation "Valley" to a "Control" simulation which to me has limited value. Why not compare the simulation to the readily-available surface velocity measurements, as one would hope that better physics leads to a closer agreement with observations? In Figure 6 the differences between "Valley" and "Control" exceed 10 m yr-1. Looking at observation of surface speeds, I find no clear signature of a channel, contrary to what is expected. I wonder if this means that the sliding model is too sensitive to changes in basal water. Furthermore, with a channel width of 5-10km, it is not clear to me how well a model resolution of 5km is able to capture the dynamics that the authors are interested in.

Right now the manuscript consists of a lot of material that was already discussed by B13a, an interesting but not well thought out modeling part, and a very speculative discussion on geothermal flux. While the manuscript has potential, I cannot recommend it for publication in its current form; a conclusion I did not reach lightheartedly. Maybe these suggestions can help to rewrite the manuscript, with focus on 1. what is novel compared B13a 2. the ice flow modeling and how the inclusion of the channel improves agreement with the observed flow structure.

Detailed comments:

P2, L 1: Please note that in the interior of the ice sheet, BedMachine is based on krig-

ing, mass conservation was only used near the coast. It is thus more or less equivalent to bed map of Bamber et al (2013) [B13b].

P2, L 11: change "when you consider", this is too colloquial

P2, L23: Maybe I don't understand the initialization procedure correctly, but why is 1990 the target date? P2 L 30 that corrections are made to bring the simulated ice thickness in agreement with observations. But to the best of my knowledge there is no 1990m DEM of Greenland available that can be used as a target?

P5, L5: "extremely gently" sounds awkward. Maybe just "gently" or "very gently"?

P5, L 25: "where you get" is too colloquial.

Figures: To increase readability, I recommend using the same color scales for bedrock elevation in all figures. Currently there a 3 color scales (Fig 1, 2, 5).

Fig 4: A close up of the NEEM zone would be helpful, the flow lines are hard to distinguish here.

References:

[B13a] Bamber, J. L. et al. (2013) 'Paleofluvial Mega-Canyon Beneath the Central Greenland Ice Sheet', Science, 341(6149), pp. 997–999. doi: 10.1126/science.1239794.

[B13b]]Bamber, J. L. et al. (2013) 'A new bed elevation dataset for Greenland', The Cryosphere, 7(2), pp. 499–510. doi: 10.5194/tc-7-499-2013.

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