

Review of Chamber et al

General Comments

This appears to be a poorly conceived study or, at least, a poorly conceived description of the study. It reproduces a key inference from a paper published six years ago and presents it as a new result or inference. As mentioned in the already posted comment, this study is actually about the influence of a continuous subglacial canyon on water flow and ice dynamics. While that is a topic that could be interesting, almost all of the discussion presents flawed, hand-waving supposition that has little evidential basis and has little, if anything, to do with the model experiments.

The paper would benefit from a substantial rewrite and refocusing on the actual topic of the experiments conducted: what is the impact of a continuous canyon on the subglacial hydrology beneath the Greenland ice sheet? However, as the hydrology model used assumes thin film flow and no sources from surface melting are included, it is debatable to what extent the experiments can address this question with adequate confidence.

Specific Comments

- 1) As already mentioned in the posted comment, the title is not only extremely misleading it addresses a question that was tackled previously and this study provides no new evidence to answer it. The entire abstract needs to be rewritten focusing on the results of the model experiments and nothing else.
- 2) It is unclear to me why the authors have to introduce or replace existing terminology. They need to remove the word “river” throughout and replace with either conduit, R channel, or thin film as appropriate. In addition, why do they rename the canyon, “valley”. Their explanation (p1, l23) is nonsensical.
- 3) The introduction is misleading and needs to be rewritten. Quoting directly from Bamber et al 2013 (B2013), they state “we present evidence from ice-penetrating radar data for a 750-km-long subglacial canyon in northern Greenland that is likely to have influenced basal water flow from the ice sheet interior to the margin”; “In all cases, above ~76° N and within the entire length of the Petermann catchment, the canyon exerts a control on basal water flow. For ~200 km, it provides an uninterrupted hydraulic pathway (Fig. 3 and fig. S3A) that ends at the terminus of Petermann Gletscher” and so on. I do not understand why the authors are proposing this as something entirely new.
- 4) P2 l1. The authors appear to be unaware that the bed topo in BedMachine v3 and that which was used in B2013 are essentially the same. The only difference is in the use of mass continuity near the margins where IPR coverage is poor. Any conclusions drawn in B2013 will be identical for BedMachine.
- 5) P4, l35. Fig 5 does not show slope, it shows surface elevation. The slope in the interior of ice sheets is small everywhere. This is not the same as “near flat” which is meaningless. Eyeballing the Fig it looks like the canyon follows the surface slope quite closely in the interior. This sentence is a good example of the hand-waving vagueness that pervades other parts of the paper.
- 6) P5, l5-6. I don’t understand the logic of this statement. The canyon is not linear and doesn’t follow the streamlines when it is not present (Fig 4a). This is nonsensical.

- 7) Section 4 Discussion. I found this section far too speculative, hand-waving and non-scientific. The first part is a qualitative overview of subglacial water flow theory. The arguments for why water may be present in the canyon are OK, in general although the discussion of enhanced GHF was a bit muddled and unclear. It could have all been stated in half the space as basal frictional heating, warmer ice at depth are all well established concepts. By page 7, l9 the discussion becomes too speculative. The authors appear to be unaware that B2013 examined hydraulic potentials for the isostatically compensated bed (Fig 3A) and discuss this in some detail. In addition, the authors do not explain how 300-800 m of bedrock erosion from subglacial water flow is possible during the Holocene. That would be a challenge even for rapid basal sliding over a soft bed, neither of which is the case here. At LGM, ice flow and hydraulic routing was different due to larger ice sheet cover.
- 8) P7, l27. Why?
- 9) P7, l30-32. This appears to demonstrate a complete lack of understanding of how the lithosphere responds to changes in ice loading. Why would there be a differential viscous response of the mantle across a < 10 km wide channel? In addition, how flat is the canyon and how flat is it after isostatic compensation? This is really incoherent.
- 10) Section 5. As for the abstract, title and Discussion, this section needs to be rewritten, focusing on the actual results and not wild and unsupported speculation.