Response to Reviewer 2: Andy Aschwanden

Thank you very much for taking the time to review our paper. We are sorry to hear that you are not recommending this study for publication and appreciate your careful consideration in coming to that decision. Nonetheless we would like to address the issues that you raised. *(Reviewer statements are in italics).*

“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 title has been chosen because the focus of our investigation has been on the possibility of a currently active long subglacial river system along the valley route. We would prefer to keep the original title after considering the following points:

- The article is about the “possibility of a long subglacial river” and as such presents evidence for such a system while recognizing that the huge void in data precludes a conclusive result.
- The word “river” is appropriate when defined as “subglacial river” which has different properties to a river over open land. In addition, many “rivers” on Earth do not flow at all times, or flow at certain times only along certain sections.” We are going to add this definition to the introduction to be clear and we are considering adding an additional appendix that discusses potential discharge estimates.
- We are also going to add a section to the discussion that details reasons why there may not be a subglacial river within the valley

Regarding the originality of the study, B13a did not demonstrate that an uninterrupted water pathway was possible under present day conditions from Interior to Petermann in the evidence they presented. Their figures show water pathways along numerous independent sections of the valley. They infer that the valley is probably continuous but do not present scientific evidence of the potential effects the continuity of the feature could have on the basal hydrology or ice sheet sliding. Therefore our results are original. We will add references to B13a beyond what is already included to make it clear what their results showed, how they differ from ours, and how our results relate to inferences made in B13a.

“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?”

Thanks for the suggestion to compare with observations. Our goal in relation to ice sheet sliding is to provide an assessment of what impact an open valley introduced into a simulation can have. To do this it makes sense to compare two cases that are identical aside from the topographic modification, as we have done with “Valley” and “Control”. The results therefore only show the consequences of the topographic change and so reveal the influence it has. For assessing whether the introduction of a valley improves the simulation with respect to observations it would be valuable to make the comparison you suggest, however this has not been our aim up to this point.

"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."

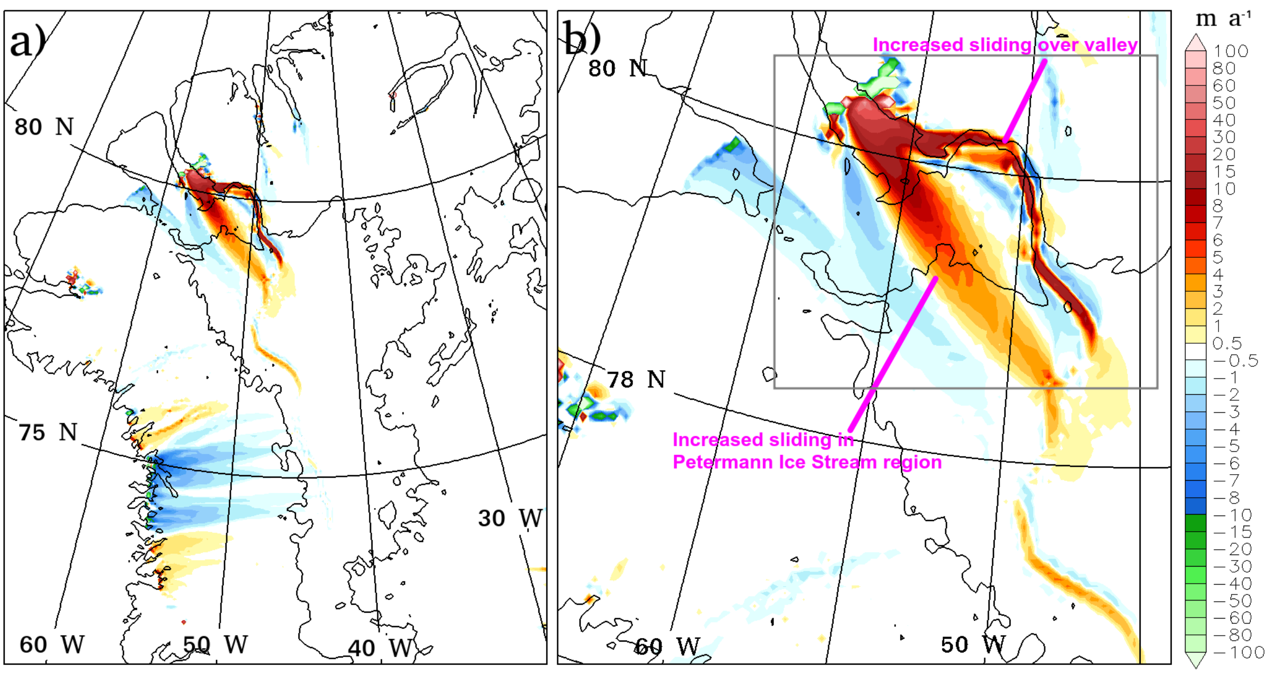


Figure 1: Surface ice velocity difference (Valley – Control) in metres per year for a) north Greenland and b) the Petermann catchment. Grey box indicates the region used in Figure 2.

The figure above highlights the relevant areas. There are two regions of increased sliding in the Petermann catchment. One associated with the Petermann Ice Stream region and another narrower one associated with the valley.

We did the simulations with the shallow-ice approximation (SIA), which produces a too localized response of the ice flow to changed basal sliding. However, we are planning to re-do the simulations with the hybrid shallow-ice--shelfy-stream scheme that captures the dynamics of fast-flowing grounded ice more realistically. This should lead to a more distributed response of the surface velocity, thus a less pronounced signature at the surface.

Other possible explanations for the lack of a 10 m a^{-1} speed up directly over the valley in the observed surface velocities include:
1) There is no subglacial river, or significant subglacial water within this section of the valley, i.e. it is mostly frozen to the bed.
2) Since the model relies on a thin film basal hydrology it fails to model any type of subglacial river. Consequently it fails to channelize the water, potentially causing an unrealistic distribution of water in a wide film that has a greater influence on sliding.
3) As you suggest, the sliding model could be too sensitive to changes in basal water.

This issue will be noted in the paper.

The valley in this region is noticeable in the observed surface velocity field when you focus on speeds between 10 to 40 m a^{-1} (Figure 2 below). However this is not like what we see in the model and may simply be due to the topography rather than basal water.

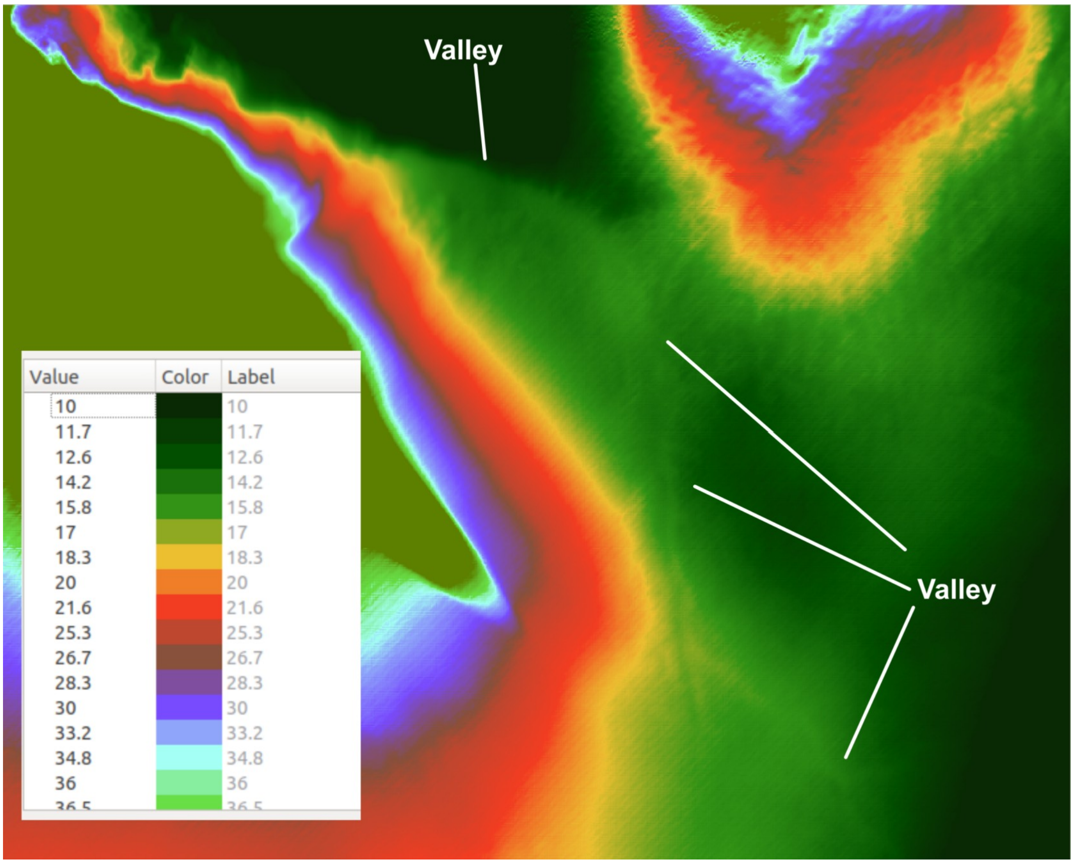


Figure 2: Observed surface velocity in the coastal valley section.

For the second area in the vicinity of the Petermann Ice Stream, differences exceeding 10 m yr-1 only occur in areas where the ice flows pretty fast, so that the differences amount to only a few percent. This does not produce a notable signature, and it is too small to allow a reliable judgement about whether "Valley" or "Control" matches observed surface velocities better.

“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.”

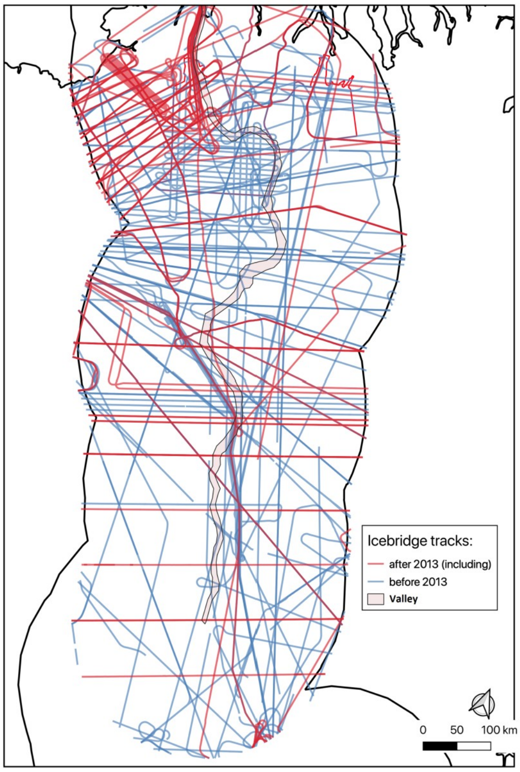
This is a significant model issue and it is one that important to highlight. Unfortunately we are currently unable to simulate the Greenland Ice Sheet at higher resolutions than 5 km and this restricts our ability to correctly reproduce basal topographic features. We can only present the results we have and detail potential error sources. We will add further mention of this issue. Judging by the basal water fluxes we present, we have successfully removed the blocks along the valley route, which was our goal. From a modeling perspective, this is a first step to demonstrate the potential extent of the impacts.

“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.”

We present new scientific evidence that was not presented by B2013a and will reference B2013a further where appropriate. We hope that the points above help to address the issues presented on the modeling aspects. We can improve the discussion on geothermal heat flux if we proceed further with the paper.

“Detailed comments:
P2, L 1: Please note that in the interior of the ice sheet, BedMachine is based on kriging, mass conservation was only used near the coast. It is thus more or less equivalent to bed map of Bamber et al (2013) [B13b].”

This is noted. Our study is focused on adjustments made to open the valley rather than any minor changes in the data since B13b. For reference the additional flight lines used since 2013 are shown in the figure below.



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

Thanks. We will make this change.

“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?”

Our SICOPOLIS model setup is based on that used for the Ice Sheet Model Intercomparison Project for CMIP6 (ISMIP6). The reason why we chose 1990 as the initialization date for ISMIP6 is that the Greenland ice sheet was approximately in balance around that time, while its mass balance has become more and more negative since then. You are correct that there is no DEM for 1990. The nominal date for BedMachine Greenland v3, from which we also took the surface DEM, is 2007 <https://sites.uci.edu/morlighem/dataproducts/bedmachine-greenland/>.
However, the difference between the overall topography 1990 and 2007 is very small, so that it doesn't really matter. More important is that the climate forcing for the spin-up run ends in 1990 and produces an ice sheet that is very much in balance.

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

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

We will make these changes.

“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.”

We can make these changes if we proceed further with publication. Thanks for the suggestions.

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.