

The Cryosphere Discuss., author comment AC1  
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## Reply on RC1

Anna Derkacheva et al.

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Author comment on "Seasonal evolution of basal environment conditions of Russell sector, West Greenland, inverted from satellite observation of surface flow" by Anna Derkacheva et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-170-AC1>, 2021

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Dear reviewer,

We thank you for the positive general assessment and given comments. Please find below answers to your questions and comments.

- I. 9 - are these modelled or observed water pressure variations? Might be worth specifying here.

These are modelled water pressure variations. We will update the text accordingly.

- I. 84 - "observed geometry" - the problem is, most velocity observations aren't contemporaneous with surface observations - it would be good to show that the impact of this is low.

As discussed in lines 179-181, we consider the cumulative surface elevation changes over about 20 years (lag between elevation and velocity data collection) here to have a minor impact on our inversion (e.g. driving stress). Based on the cited study of Helm et al. (2014), the average thinning rate in 2011-2014 was about -1 m/yr. Similar values were also found by Csatho et al. (2014) for the years 2007-2008 and 2008-2009, and by Yang et al. (2019) for the period between 2002 and 2012 (about -0.6 m/yr). The total estimated thinning of roughly 20m is much smaller than the average ice thickness over the area.

In terms of seasonal fluctuations studied here, we do not expect that this will give a major change in surface elevation either. In-situ GPS measurements made at several locations in the region show only minor seasonal surface changes on the order of one meter (Bartholomew et al. 2010, 2011; Cowton et al. 2016; Nathan Maier's personal communication ). We give the detailed rates below for the I. 180-184 comment.

Therefore, while it is possible that using a non-contemporaneous surface with velocity observations induces biases in the inversions, these biases should remain negligible compared to other sources of uncertainty (e.g. errors on velocity, ice thickness or rheology parametrization) and staying almost identical over seasons should not influence the inferred results of seasonal evolution of the basal conditions.

*Helm, V., Humbert, A. and Miller, H. (2014) 'Elevation and elevation change of Greenland*

and Antarctica derived from CryoSat-2', *Cryosphere*, 8(4), pp. 1539–1559.

Csatho, B. M. et al. (2014) 'Laser altimetry reveals complex pattern of Greenland Ice Sheet dynamics', *Proceedings of the National Academy of Sciences of the United States of America*, 111(52), pp. 18478–18483.

Yang, Y. et al. (2019) 'Space-Time Evolution of Greenland Ice Sheet Elevation and Mass From Envisat and GRACE Data', *Journal of Geophysical Research: Earth Surface*, 124(8), pp. 2079–2100.

Bartholomew, I. et al. (2010) 'Seasonal evolution of subglacial drainage and acceleration in a Greenland outlet glacier', *Nature Geoscience*. Nature Publishing Group, 3(6), pp. 408–411.

Bartholomew, I. et al. (2011) 'Supraglacial forcing of subglacial drainage in the ablation zone of the Greenland ice sheet', *Geophysical Research Letters*, 38(8), pp. 1–5.

Cowton, T. et al. (2016) 'Controls on the transport of oceanic heat to Kangerdlugssuaq Glacier, East Greenland', *Journal of Glaciology*, 62(236), pp. 1167–1180.

- l. 122 - suggest alternative terms to "master/slave - see, e.g., <https://comet.nerc.ac.uk/about-comet/insar-terminology/>"

We will replace the terms master/slave by "primary" and "secondary".

- l. 132 - vx/vy or vx/vy (l. 157)?

We mean vx/vy. This will be corrected.

- l. 138 - presumably the MWS map is the median of January/February/March?

No, MWS refers to the mean of these three months. We use it instead of the median because the variability between months is so small that the mean and median are almost identical. We will clarify this in the manuscript.

- l. 141 - what are typical values of n here?

n represents the number of speed measurements at each pixel and is highly variable in space (see Fig.2-e) and time. Close to the ice front, the range of n is between < 5 images in winter inland to > 60 in summer. A sentence will be added to better describe the typical range of n.

- l. 161-167 - this seems like a reasonable (and interesting!) explanation, but wouldn't these changes also have an impact on the magnitude of the velocity vector?

Yes, the effect would also have an impact on the magnitude, but we estimated that on average across the year this error is smaller than errors from other sources (e.g. related to the geometrical resolution of source imagery). For the most extreme cases (the lowest sun angle in spring/autumn and when only optical imagery is used), we theoretically estimate that the magnitude will be overestimated by less than 10% compared to the real speed. That would correspond to a bias of less than 10 m/yr for the typical speed in this sector of 100 m/yr. As the issue affects only a few time-steps of the velocity database and the bias is compatible with average uncertainty for those months, we did not apply any special corrections on magnitude. This explanation will be more clearly described in the revised manuscript.

- l. 180-184 - it would be good to back this up using citations/example data, if possible. Are there any GPS observations for the area that demonstrate this (e.g., from Maier et al. 2019)?

Please see the response to the first comment for the multi-annual surface elevation changes that have been observed. We will complement the references accordingly.

Seasonal rates of surface elevation change have been recorded at several GPS stations installed in the region along a flowline (Bartholomew, 2010, 2011, Cowton, 2016). These observations show small seasonal ice surface changes of less than 1.5m, and more commonly this change is less than 0.5 m. Further, they are interpreted by authors to be partly induced by glacier uplift, thereby the ice thickness changes are even smaller. This is consistent with the seasonal rates recorded from GPS stations in 2014-2017 used in Maier et al. 2019 which showed seasonal uplift of about 0.25 m/yr (currently unpublished; personal communication). We will add the values for the rate and citation in the text.

*Bartholomew, I. et al. (2010) 'Seasonal evolution of subglacial drainage and acceleration in a Greenland outlet glacier', Nature Geoscience. Nature Publishing Group, 3(6), pp. 408–411.*

*Bartholomew, I. et al. (2011) 'Supraglacial forcing of subglacial drainage in the ablation zone of the Greenland ice sheet', Geophysical Research Letters, 38(8), pp. 1–5.*

*Cowton, T. et al. (2016) 'Controls on the transport of oceanic heat to Kangerdlugssuaq Glacier, East Greenland', Journal of Glaciology, 62(236), pp. 1167–1180.*

*Maier, N. et al. (2019) 'Sliding dominates slow-flowing margin regions, Greenland Ice Sheet', Science advances. American Association for the Advancement of Science, 5(7), p. eaaw5406.*

- l. 201 - why 0.9 m?

This value is not physically meaningful; it merely allows an easy separation between ice-covered and ice-free areas in the model. The model does not support meshes that have null thickness. In order to include ice-free areas, we therefore impose the arbitrary value of 0.9 m thickness for them. This thickness is sufficiently small that the remaining "ice" in the ice-free areas will have no impact on the results of the inversions and will avoid crashing the model. We will add a sentence to better explain this point.

- Eqn. 5, elsewhere - assume this is meant to be a centered dot indicating a dot product? (i.e., " $\text{dot}(u,n) = 0$ ")

Yes, we will correct the text.

- l. 266-268 - I understand what you're saying here, but it seems circular to say "our choice of input is consistent with our output (which somehow depends on the choice of input)" - maybe just use the reference to Meier?

We understand what you mean, but in fact this is not totally circular. A few ice thicknesses from the boundaries the results should be insensitive to the details of the boundary condition (e.g. Mangeney et al., 1996, Gagliardini and Meyssonier, 2005), so the results in the interior can be used to justify that this is also a reasonable assumption at the boundaries. We will better clarify this point so that it does not appear as circular.

*Mangeney, F. Califano, O. Castelnau, Isothermal flow of an anisotropic ice sheet in the vicinity of an ice divide, J. Geophys. Res. 101 (12) (1996) 28,189–28,204.*

Gagliardini, J. Meyssonier, *Lateral boundary conditions for a local anisotropic ice flow model*, *Ann. Glaciol.* 35 (2002) 503–509.

- I. 284 - might be good to include references for this statement.

The following references will be added : Jay-Allemand et al. (2011), Gillet-Chaulet et al. (2012); Larour et al. (2014); Shapero et al. (2016); Maier et al. (2021)

Jay-Allemand, M. et al. (2011) 'Investigating changes in basal conditions of Variegated Glacier prior to and during its 1982–1983 surge', *The Cryosphere*, 5(3), pp. 659–672.

Gillet-Chaulet, F. et al. (2012) 'Greenland ice sheet contribution to sea-level rise from a new-generation ice-sheet model', *Cryosphere*, 6(6), pp. 1561–1576.

Shapero, Daniel R., et al. 2016. *Basal resistance for three of the largest Greenland outlet glaciers*. *Journal of Geophysical Research - Earth Surface* 121(1): 168–180.

Maier, N. et al. (2021) 'Basal traction mainly dictated by hard-bed physics over grounded regions of Greenland', *The Cryosphere Discussions*, pp. 1–31.

Larour, E., Utke, J., Csatho, B., Schenk, A., Seroussi, H., Morlighem, M., et al. (2014). *Inferred basal friction and surface mass balance of the Northeast Greenland Ice Stream using data assimilation of ICESat (Ice Cloud and land Elevation Satellite) surface altimetry and ISSM (Ice Sheet System Model)*. *The Cryosphere*, 8(6), 2335–2351.

- Fig. 3 - why not show the mismatch here, instead of in the appendix?

We will add a subpanel in Figure 3 showing the mismatch.

- I. 353 - what do you mean by "relatively" short distances?

Here we mean that basal conditions can be heterogeneous even over distances of a few ice thicknesses (a few kilometers in our results). The sentence will be rewritten as : "as the basal conditions of this sector are heterogeneous and can likely change from an inferred hard to weak bed over distances of a few ice thicknesses."

- I. 368-372 - I'm not sure I completely understand these lines, and I think part of my confusion might come from calling the 24 datasets "time steps". By "restart from the optimal solution" do you mean that you use the parameters from the optimal solution as a starting point for each of the 24 datasets?

Yes, "time steps" might be misleading. You understood correctly. We use the solution obtained for the MWS observations as a starting point for each of the 24 independent inversions corresponding to 24 velocity maps. This part will be rewritten as : "To reduce the computational burden for the monthly inversions, the basal friction coefficient field is initialised using the optimal solution obtained with the MWS observations. A new independent inversion is then run with the 24 data-sets using the same optimal value for the regularisation parameter"

- I. 380 - why only the early half of each month? If there aren't significant differences between the early and late halves of each month, it would be good to mention that here.

We kept only the early half of each month so that the size of the figure would remain reasonable. The mismatch for the early and late parts for the majority of months is similar. We believe that showing only the early half of each month is sufficient to

illustrate the average difference between the model and the observations and its trend over a year. We will add a statement that the second half of months usually shows similar mismatches to the first half.

- Figures - it would be good to have some scale bars to help readers connect the text (e.g., "10-15 km from the margin") with the locations in the Figure

Instead of scale bars, we use the uniform 10-km white grid on all figures from Fig.1 onwards. We will add the reminder about the grid size to all captions.

- I. 417 - how significant a change is this 2%? Would be good to have some idea of the variation here.

Agreed. We will add the corresponding absolute values in the changes in speed for  $u_d$  and  $u_s$ .

- I. 465-466 - this is an interesting observation - is there a physical interpretation for why this might be the case?

Yes, there is a physical interpretation of the hysteresis between sliding and basal friction. Similar observations or modeling results have been found in other studies. Sugiyama and Gudmundsson (2017) studied short-term variations in ice flow on an Alpine glacier in relation to subglacial water pressure. They showed that velocity was greater as pressure increased than as it decreased (for equivalent water pressures - consistent with a hysteresis). The increase in velocity with increasing pressure was interpreted as an opening of the subglacial water cavity and/or longitudinal stress coupling with the upper parts of the glacier. This linkage was also studied numerically by Iken (1981) by modeling basal slip on undulating bedrock as the water-filled cavities grew and shrank. Iken's modeling also predicted that a small drop in water pressure below steady-state values would result in a rapid decrease in slip rate. We will discuss this point in the revised version of the manuscript.

*Sugiyama, S. and Gudmundsson, G. H. (2017) 'Short-term variations in glacier flow controlled by subglacial water pressure at Lauteraargletscher, Bernese Alps, Switzerland', Journal of Glaciology, 50(170), pp. 353–362.*

*Iken, A. (1981) 'The effect of the subglacial water pressure on the sliding velocity of a glacier in an idealized numerical model.', Journal of Glaciology, 27(97), pp. 407–421.*

- I. 497 - could you (briefly) describe the differences between Eqn 12 and the "similar expression" proposed by Zoet and Iverson (2020)?

Eqn 12 is directly the sliding law proposed by Gagliardini et al. (2009) for  $q=1$  following the formulation from Schoof (2007). This sliding law has been developed for hard beds with cavitation. The form of the expression proposed by Zoet and Iverson (2020) combines processes of hard-bedded sliding and bed deformation. While this friction law has additional parameters to account for the different physical processes controlling basal motion over till (i.e. till strength and clast size), the form of relationship between basal motion and friction ends up being quite similar to that over a hard bed. The main difference is that the effective pressure that appears in the denominator in Eq. 12 is not to the power  $n$  in Zoet and Iverson (2020). Because of this, for a given basal friction, the formula given by Zoet and Iverson predicts that the velocity should tend to 0 at high effective pressure, while Eq. 12 tends towards the velocity predicted by the Weertman friction law. We will explain this point in the revised version of the manuscript.

- I. 510 - this seems like it would be an issue?

We assume that water pressure exceeding ice overburden is usually a very short-term event, thereby on the addressed "longer" timescale its neglect would not be a problem for the interpretation of the results. Additionally, from a technical point of view, both very small positive and negative  $N$  (e.g.  $1e-5$  and  $-1e-5$ ) practically lead to the same outcome of near-zero friction in the model (which is unable to reproduce the other effects one might associate with water pressures exceeding overburden, such as hydraulic jacking). Note that the water pressures greatly exceeding ice overburden is very unlikely; for instance, Doyle et al. (2015) found that an unusual cyclonic late-summer rainfall generates maximum water pressure only of 100.5% of overburden.

*Doyle, S., Hubbard, A., van de Wal, R. et al. Amplified melt and flow of the Greenland ice sheet driven by late-summer cyclonic rainfall. Nature Geosci 8, 647–653 (2015)*

- I. 536 - supplementary materials? I don't see Fig. S9 or Table S1.

We are referring here to the supplementary materials of the cited paper Maier et al. (2021).

- I. 634-636 - I'm not sure I understand what you're saying here.

We are trying to explain that the total force balance during summer is still sufficient to prevent the glacier from collapsing (i.e. large-scale unstable sliding). Therefore, when friction locally becomes very small and the ice accelerates, the local change in stress is transmitted by longitudinal stress coupling to other places that will thereby offer enhanced flow resistance (larger friction) to maintain the global force balance.

- I.638 - missing reference

We will correct the text.

- I. 718 - missing reference

We will correct the text.

- I. 731 - this is probably true for satellite observations, but ground-based radar interferometry potentially provides a way to do this (e.g., Caduff et al., 2015)

Correct, we will change "remote" observation to "satellite".