Response by Jan Pronk and others to William Armstrong his comment on tc-2021-90.

We would like to thank the referee for his second look on our manuscript. His comments, that had a specific focus on a more thorough consideration of ice thickness estimates, helped to improve our work considerably. As a result, current major changes to the manuscript are largely limited to the paragraph related to the ice thickness differences (lines 590 – 612 & Figure A5). Below we provide our response to each comment.

1) Can you provide more quantitative data on the ice thickness difference, rather than just using qualitative terms (e.g., "substantial factor") and pointing to Fig. A5? For example, by what factor (e.g., 1.5x) thicker are lake-terminating glaciers than land-terminating glaciers? If you used a simple estimate like the shallow ice approximation, what velocity difference would you expect? What fraction of the observed velocity difference could this explain?

   Response:

   We agree with the referee that providing more quantitative data on the ice thickness difference would improve this relevant section, and we therefore decided to adopt his suggestions. For this, we followed the approach of Dehecq et al. (2019), where a first order approximation is made to relate changes in surface velocity to changes in driving stress. Assuming all factors other than ice thickness changes remain constant, we relate all changes in driving stress to changes in ice thickness:

$$1 + \frac{\partial U_s}{U_s} = (1 + \frac{\partial H}{H})^4$$

   This gives a rough estimate of how many of the observed velocity change can be explained by changes in ice thickness, when no other factors (such as a lake or slope) play a role. We solely apply this approximation to the whole ablation zone, as this approximation is only appropriate in the limit of $\frac{\partial H}{H}$*<<1. Finding a mean median ice thickness of lake-terminating glaciers of ~110m and land-terminating glaciers of ~100m over the ablation zone, we find that only 28% to 64% of the observed velocity contrast can be explained by this approximation.*

2) The fact that Fig 6b shows substantially faster flow for lake-terminating glaciers within the same area class suggests that variables in addition to ice thickness are driving the speed differences (because thickness generally scales with area). This may be worth pointing to to better support your claim of ice thickness differences not driving your observation.

   Response:

   We thank the referee for this suggestion and adopted this in the manuscript.

3) It is not clear to me what you mean by "Concurrently, it is worth considering that the difference in ice thickness between land-terminating and lake-terminating glaciers is also due to the very presence of a lake". This seems like a "chicken-or-the-egg" problem – is ice thickness different there because the lake exists, or does the lake exist because ice thickness (and/or subglacial/proglacial topography) differ? Lakes are found in basins (to state the obvious), and basins/overdeepenings also promote thick/fast ice. We wrestled a lot with covariance between ice thickness and other variables in the context of proglacial lakes in the paper referenced below (particularly in Secs 4.4 & 5.4). It may be worth reading and referencing those ideas.

Response:

We agree with the referee that this raised a "chicken-or-the-*egg" issue, and therefore decided to remove this argumentation from our manuscript. Also, we gladly added the interesting study from Field et al. (2021) to our discussion (lines 627 – 628).*

4) In Lines 605-607, I don't think you can say the link is causal because of the above-referenced "chicken-or-egg" issue. You show an association between lakes & faster velocity, but not necessarily lakes causing faster velocity.

   Response:

   We agree with the referee and would like to thank him for pointing this out. In line with the previous comment form the referee, with decided to not argue that this link is causal.