The Cryosphere Discussions

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Comment on tc-2021-286

Anonymous Referee #2

Referee comment on "Land- to lake-terminating transition triggers dynamic thinning of a Bhutanese glacier" by Yota Sato et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-286-RC2, 2022

Sato and colleagues present a short study on the effects of a Himalayan glacier retreat with a transition of termini type from a land-terminating to lake-terminating. They argue that the recently observed significant acceleration of ice flow velocities of Thorthormi glacier can be explained by a switch from a compressional to an extensional flow regime in the ablation area caused by a detachment from the terminal moraine. These findings are supported by analysis of ice flow velocity and ice elevation change and are built upon the results of a previously published study by Tsutaki et al. (2019).

Unfortunately, at the moment the novelty of this submission is fairly limited as the major points have already been addressed by Tsutaki et al. (2019) and by the recent regional study of Pronk et al. (2021). As far as I'm concerned, the purpose of the paper is to show the observational data that confirm the Tsutaki et al. (2019) hypothesis of a plausible ice flow acceleration of Thorthormi glacier following a detachment from the terminal moraine. As the authors have noticed, similar acceleration has already been observed in other regions and, according to current understanding, is something likely to happen. Unfortunately, the authors did not make a sufficient effort to show the new data convincingly. Figures with maps of ice elevation change and ice velocity field or a profile of the emergence velocity would have been useful and could give a quantitative grasp on what is happening during the retreat. Secondly, the observations of the glacier speed up are limited to only one season (2016/2017 - five years ago), it would have been much more interesting to see a time series of the annual velocities covering the full period from 2011 to the present. Moreover, the calculation of the emergence velocities is flawed as the authors incorrectly assume a constant no-slip condition (mean vertical velocity equal to 80% of the surface velocity) along the entire ablation area of the glacier. Lastly, the authors disregard frontal ablation flux in their analysis whereas Pronk et al. (2021) have shown that frontal ablation is controlling the strain rate and whether the terminus is in compressional or an extensional regime. In my opinion, at this stage, the manuscript lacks sufficient quality and novelty that would ensure publication in The Cryosphere and needs a very substantial revision to be accepted.

Specific comments:

L41-42: This is true not only for land- vs. lake-terminating glaciers, it is not uncommon to see a different response to climate forcing even when the glaciers share the same terminus type.

L62: "with the 2017 terminus position and its elevation range spanning 4,400–6,900 m above sea level (a.s.l.)" please re-write

L65: Which year does this area refer to?

L66: Please be consistent with lake names and introduce them on the Study Area map: here you refer to Lugge Glacial Lake whereas before,(L65) you introduced Lugge Tscho

L116: If the dh/dt values are at the same positions, how do you explain such a large variance in your dataset compared to previous estimates (Fig. 3)? Did you aggregate your DEMs to 30m resolution as well? Otherwise, both datasets cannot be directly compared. Additionally, the Leibnitz notation (dh/dt) is normally used for instantaneous values (differential) implying that both dh and dt are infinitesimals.

L118-122: Please provide more details on data processing (template size, templatematch algorithm)

L135-136: Did you look on ITS_LIVE scene-pair velocities as well?

L149: flotation

L155: Why is this Eq. 2a and not 2?

L169-170: How reliable is the assumption of a fixed glacier width and thickness? Please remember that the glacier bed geometry is inferred from inverse modelling (Farinotti et al., 2009; Tsutaki et al., 2019) and has not been measured.

L170: You are using velocities along the centreline yet Eq. 2b works for depth- and width-averaged velocities. There may be an important change in lateral drag due to the presence of lakes on the sides of the Thorthormi glacier, thus the cross-sectional

distribution of the ice flow is not constant along the flowline.

L172: Farinotti, not Farinottie

L174: Here you are assuming no basal motion whereas one can expect an increasing sliding towards the glacier terminus in case of a lake-terminating glacier (Tsutaki, 2019). You are incorrectly referring to Sakai et al. (2006) and Berthier and Vincent (2012), here are quotes from those articles:

Sakai et al. 2006:
"The ice velocity averaged over the depth (h) is taken to be 80% of the surface velocity (u_s) by assuming that the ice flow is laminar, that there is no basal motion (consistent with the glacier being of the polar type) and that the empirical constant (n) in Glen's law is 3 (Paterson, 1994)."

Berthier and Vincent, 2012:

"The next step is the conversion from mean surface to depth-averaged velocity. Without basal sliding, theoretical calculations suggest that the depth-averaged velocity is 80% of the surface velocity (for n = 3 in Glen's law; Cuffey and Paterson, 2010, p. 310). With basal sliding, this percentage increases, and for example in the case of Athabasca Glacier, Canada, the mean cross-sectional velocity equals the mean surface velocity (Raymond, 1971). Here we used an intermediate value assuming that the depth-averaged velocity is 90% of the surface velocity."

L230: Why did you calculate the velocity field only for 2016/2017 season? I would recommend showing a map of the velocity field.

L249-257: A plot of the emergence velocity is strongly recommended.

L250: The ± sign is not needed as σ is always positive. What is the reason for such a high standard deviation?

L277: What is the frontal ablation flux of both glaciers? I think it is important to include it in your analysis as this is the term that closes the mass budget, otherwise, how would you explain the increased thinning and acceleration of ice flow during land-terminating to lake-terminating transition? Please refer to the relevant discussion made by Pronk et al. (2021). There is an abundance of icebergs in the Thorthormi lake as shown in Fig. 1,

suggesting a significant frontal ablation flux.

References: There are many errors, missing pagination or volume numbers. Please update references that changed from early access to the final publication stage.

Figure 1: Please add a description of the black bounding box in panel (b). As far as I'm concerned, the panel labels should be in brackets: (a), not a)

Figure 2: This Figure is very similar to Fig. 4 in Tsutaki et al (2019), I guess the reason is to make them easy to compare, please add a reference. The glacier area change increases already 3-4 years before the detachment from the moraine (panel c), this is probably due to the development of lateral lakes - please clarify this in the text

Figure 3: There is a large scatter in 2011-2018 data, the points diverge into two populations - a positive and a negative one, why?

Figure 4: Please add the uncertainties of 2016/17 and 2017 (ITS_LIVE) data series.

Additional references:

Pronk JB, Bolch T, King O, Wouters B & Benn DI (2021) Contrasting surface velocities between lake- and land-terminating glaciers in the Himalayan region, The Cryosphere, 15(12), 5577-5599 . https://doi.org/10.5194/tc-15-5577-2021