

Answer to reviewers “Generalized sliding law applied to the surge dynamics of Shisper Glacier and constrained by timeseries correlation of optical satellite images” by Flavien Beaud et al.”

We would like to thank the three reviewers for their time and constructive comments. Before addressing each reviewer’s comments independently, we summarize our understanding of the main criticisms and how we plan to address them in the revisions.

We are glad to see that the reviewers made an overall positive assessment of the manuscript. The section that draws substantial criticism is one sub-section of the discussion, section 6.4. In this section we assess whether the proposed generalized sliding relationship can encompass the data presented. Reviewer 1 and 2 both express concern that the uncertainties related to the bedrock topography, ice surface elevation and basal stress estimations are too large to trust the assessment of the generalized sliding relationship. Our goal in that section is to present a first order assessment that the proposed generalized sliding law could explain the observed data, and that it is possible to constrain sliding-law parameters in cases where estimates of surface and bedrock topography are available. We note that the concerns raised by the reviewers are the very reason why we avoid quantifying the sliding-law parameters. We also agree that the data scatter originates from both heterogeneities in bed properties and measurement uncertainties, but the signal is large enough so that our qualitative assessment holds.

Reviewer 3 (D. Benn) suggests some re-framing of the sliding law that would make the role of effective pressure (N) explicit. We explain below that we didn’t follow this advice because it would then require choosing between a soft or a rigid bed from the onset and the formulation would then lose generality. We, however, propose changes to the text that better describe the importance of effective pressure in the generalized law and should better substantiate our interpretation of the surge data.

We also further explain how the bed data estimates from Farinotti et al. (2019) has little effect on our main conclusion. We propose to add figures to the supplementary material to show how using different surface DEMs or bed elevation estimates affect the sliding-law relationship. These figures (see new figures) show that while the set of parameters that best fits the dataset might change, the proposed sliding law could still explain the observations. Given the difficulty to assign realistic uncertainties to the bedrock elevation, we cannot perform a quantitative analysis of the uncertainties on the sliding-law parameters.

Below are the answers to the specific comments for each reviewer and afterwards, the proposed changes to the text. Reviewers' comments are in black, our answers in blue, and proposed changes in green.

Answer to Reviewer 1

Review for:

“Generalized sliding law applied to the surge dynamics of Shisper Glacier and constrained by timeseries correlation of optical satellite images” Beaud et al.

Summary:

The authors present a study with the overall goal to use satellite observations of a surging glacier to constrain the form of a generalized sliding law. The manuscript is overall very well written, especially the first half, and the figures are informative and of high quality. The manuscript has three main parts: First, the authors derive a new generalized form the traction relationship which combines rate-strengthening with weakening applicable to both hard and deformable beds. The second part presents satellite velocity and elevation observations of a glacier surge captured on Shisper Glacier. Finally, the last part uses these observations to constrain parameters within their generalized sliding law presented in the first section.

The goal of the study is very worthy. We have much work to do to better understand basal physics and I am generally enthusiastic about the first two sections. I quite like the generalized relationship derived in the manuscript, especially when compared to less physically based generalized sliding laws. However, I still find little advantage for transient simulations where defining u_t still requires aprior knowledge of the bed conditions. For these sections, I include some suggestions to improve these sections detailed in the line-by-line.

Thank you. It is unclear what the reviewer refers to as simulations. We make several approximations (discussed late by the reviewer) but there is no numerical modeling involved in the current study.

However, I have fundamental concerns about the methods/data used to constrain the traction relationships presented in the last section.

Thank you for your overall positive comments on the manuscript.

- Investigating traction relationships require the traction and velocity to be very well constrained. In this regard, the basal shear stress is the biggest concern. The methods to invert for the topography presented by Farrinetti 2019 use simplified physics that assume all motion is derived from ice deformation. Given the observations presented in the paper and the hypothesis that the glacier responds to melt suggests that this is not the case. Thus, while the variations in topography derived from this method

likely reflect variations in the bed topography, the actual thickness of the glacier in the paper is largely uncertain. On top of this is the fact that the glacier is surge type, so inverting for the bed topography will also have a large dependency on what velocity field or surface elevation data set is used for the topography inversion. The authors do not note what data they use for the inversion, although this is not the main issue here.

We did not determine the bed topography ourselves, we used the product of Farinotti et al., 2019 and all the details about the model and uncertainties can be found therein. We will make this point clearer in the revision. The main product is one estimation of bed elevation based on the composite of 3 different models for Shisper glacier.

We agree that the uncertainties are large and difficult to estimate, hence our choice of a qualitative interpretation. We tested different bed and surface topography models, the results show that the signal is large enough that the main conclusions hold regardless of the bed elevation model or surface elevation model used. The proposed changes should make this point clearer in the manuscript.

The reviewer's statement about the methods presented in Farinotti et al. (2019) is not exactly correct. Two out of the three models (models 1-3 in Farinotti & 2019) include a parametrization of basal sliding while assuming simplified physics and the 3rd is mostly empirical. These models have been applied to marine terminating glaciers and their validity is thus not limited to a deformation-dominated flow scenario.

- Estimating the basal shear stress using the driving stress for a glacier undergoing large transient forcing is insufficient. In the case of a surging glacier, sharp and variable gradients observed in the velocity field indicate higher order stresses will undoubtedly play a role in the basal traction field. This requires more sophisticated inversion methods such as the SSA or Full Stokes which take into account higher-order stresses by using a more complete formulation of the momentum balance. However, these methods are only as good as the data that constrain the inversion, in which case there are still problems, one of which I have outlined above, another is knowing the ice rheology, and the last and probably the most important I will outline below.

Using the driving stress to estimate the basal shear stress will lead to an overestimation of basal shear stress (see Minchew 2016, Thogersen 2019, etc.). That means that if we can show rate independent or rate-weakening behavior, that would only be further confirmed by a better quantification of basal shear stress.

Proposed changes L434 to 435 from:

We then plot an estimation of the relationship τ_b versus u_{ex} for our dataset (Fig. 8).

To

We choose to use the driving stress (τ_d) as an approximation for the basal stress (τ_b). While this assumption is coarse, it ensures that we overestimate the basal stress at large sliding velocities (see Minchew 2016, Thogersen 2019), making it more difficult to observe rate-independent or rate-weakening behavior. We then plot the relationship τ_b versus u_{ex} for our dataset (Fig. 8).

- A basic feature of a surge is large transient surface geometry changes (i.e. Kamb et al. 1985) which will have a significant impact on the stress field. Evidence of large surface geometry changes is presented in the paper, where 100s m ice thickness changes can be observed through the three elevation data sets presented. However, comparing the velocity field which represents a snapshot for a specific time period during a surge to an elevation dataset averaged overtime, we have no idea and really no way of determining whether the stress field is consistent with the velocity field at that point in time.

That is correct. There are figures, including new ones, in the supplement showing the different results with different surface elevations. We have added the figures showing the τ_d vs u_b relationship with the different bed elevations estimated by Farinotti et al, 2019.

The plots for all the bed and surface elevation models available show similar patterns and, importantly the rate-independent or rate-weakening behavior. We thus argue that our quantitative conclusions are robust

We have also added some discussion about the relative effect of different variables on sliding in the newly rewritten discussion subsection (section 6.5 shown at end of response). In summary, changes in effective pressure are expected to be at least as large and up to an order of magnitude larger than changes in driving stress, but can occur over much shorter timescales.

With the combined effect of these three sources of uncertainty for the traction field, all of which as presented are nearly impossible to assess, I do not see how conclusions regarding traction relationships can be made unless other high resolution elevation data sets could be found that match the time period of the observed velocity data and better inversion methods are used.

We made it clearer in the revision that we did not attempt a formal and robust inversion for the sliding-law parameters due to the uncertainties that the reviewer identified. We are proposing a framework that could allow quantification, and point to what future study should do to take this to the next level.

A revised manuscript would need to address this prior to be considered for publication. The challenges for the last section are considerable. However, the data and interpretation are interesting and in theory I do like the idea of trying to use surge behavior (with better data) to populate a traction curve (although there are a lot of things about surges (i.e. heavy crevassing) that make inverting for the traction field difficult.) A revised manuscript might want to focus on the surge behavior where a discussion on the potential for constraining traction relationships using surge glaciers would surely be interesting.

Thank you for your enthusiasm about the general goal of the study. The revised manuscript will make it clearer that the section of concern is a first order assessment and by no means an attempt at a robust quantification, and that it's use is promising where better data is available.

Specific Comments

Line 20:21: This generalization is not necessarily true. There are direct observations at several locations in Greenland rebuke this paradigm (i.e. Ryser 2014, Maier 2019 – measurements slow flow, Lüthi 2001, Doyle 2018 – measurements in fast flow).

Thank you. We will change the wording from “fast” ice flow to sliding-dominated ice flow throughout the manuscript and regarding these lines we will make the following changes at L. 21-23 from:

Ice flow velocities exceeding one meter per day are considered 'fast', and correspond to an ice flow regime that is dominated by basal sliding rather than ice deformation. Fast ice flow at tidewater glaciers is responsible for the majority of mass loss from the Greenland Ice Sheet [...]

To:

Sliding-dominated ice flow at tidewater glaciers is responsible for the majority of mass loss from the Greenland Ice Sheet [...]

Line 28-30: This is not likely generalizable everywhere. See (Maier 2021) for traction analysis that suggests some fast flowing regions generally obey rate-strengthening.

The focus of this study is on the sliding-dominated ice flow, not just 'fast' ice flow. Thanks to the reviewer comment, we now corrected the text to reflect that.

Proposed changes L. 27-28, from:

Fast ice flow velocities cannot be adequately explained by traditional Weertman-type sliding relationships [...]

To:

Large sliding-dominated ice flow velocities cannot generally be explained by traditional Weertman-type sliding relationships [...]

Line 31: Strange phrasing here.

L. 31 reads: "The idea that bed shear stress is bounded for large sliding velocities was proposed in early physical glaciology work [...]"

It is unclear to us what is strange in this wording.

Line 111-114: This description is a bit confusing. I am pretty sure skin friction is the same as viscous drag for a non-turbulent boundary layer. This needs to be much better defined. Right now I think you are actually referring to solid friction (i.e. generated from debris-bed interactions)? You need make it clear that this is what you are referring to, as this is the only thing that makes sense for the bottom panel of the hard bed figures in Figure 1.

Skin friction can be viscous drag at the boundary layer scale, but it is form drag at the macroscale. We will replace 'viscous drag' here by 'form drag' as it is more accurate for our current description.

Also while the case is made that there is a physical transition regime for till beds, can the same be made for hard beds? Based on your plots on Figure 1 the transition regime seems arbitrarily drawn after rate-weakening begins.

The presence of that transition is what was shown by Iken early on and validated by Schoof (2005) and Gagliardini et al. (2007). The beginning of the transition zone is defined by u_t in both rigid and deformable beds, while the end is indeed arbitrarily drawn as a concept. We will make that clearer in the figure caption.

I would think the transition regime occurs between the start of cavitation and Iken's bound.

That is true. We modified figure 1 and its caption to show that the transition regime will be different for different values of q . [MODIFY FIGURE]

Line 100: There is probably a more direct way to say this. Also see Helanow et al 2021 for the latest on numerically derived sliding laws over realistic beds. This might help your case that there is no need to explicitly model rate-weakening.

L100-103 read: "The bed of a glacier can only produce a finite amount of resistance to flow that is determined by its properties (CITATIONS) challenging the validity of Weertman-type relationships which imply an unbounded resistance to flow."

This sentence is meant to be general across all bed types, and we purposefully consider the possibility of rate-weakening behavior, as we find our results to be supportive of that behavior. The paper mentioned is focused on hard beds.

Figure 1: Ok the skin friction here is much better defined in a conceptual sense, but you are talking about solid friction, and this is not included in the sliding law of Gagliardini 2007. Iverson 2003 provides some of the only direct measurements of solid friction, however, it has not been incorporated nicely into sliding laws nicely yet in a theoretical sense. Some nice experimental work on solid friction was also done recently by Thompson et al. 2020. These papers might help better formulate your "hard bed skin friction" regime.

We are not talking about solid friction, we are talking about skin friction as ice is treated as a fluid and deforming till can be considered as such as well. Skin friction is the viscous drag at the scale of boundary layer, between rock and ice. We will clarify that in the legend of figure 1 and will further replace viscous drag in the text by form drag, which here is the macroscopic viscous drag due to bed obstacles. The terminology is the same as in Minchew and Joughin (2020).

135: I like the concept of the generalized law you propose Eq 6., but the utility for me is somewhat lost. To model transients you still need to know the effective pressure and thus all the bed specific parameters in Eq 5 right? I think explaining the benefit of your generalization would be a nice thing to add here.

The interest of having a generalized sliding relationship of all glaciers is that, while analyzing observations of surface velocities, one can use one single equation without having to make an educated guess as to what the bed type might be. We don't propose a new mechanism, merely a unification of the existing ones. The possibility of single equation is the title of the reviewer highlight of Zoet and Iverson (2020) by Minchew and Joughin (2020). There is no modeling done in the current study, but we lay a road map for future modeling studies to improve and constrain the parameters.

177-181: What is the uncertainty on the bed and thickness? This is fundamentally important to estimating the basal shear stress.

We refer to Farinotti et al. (2019) for the specifics regarding the methods. The final product is a composite of the different models used that yield a solution. Creating that composite yields the best approximation. They didn't provide uncertainties but they provided solutions obtained with the 3 different models. We tested these different solutions and found that in all cases the data could be reconciled with the proposed friction law.

236-240: Using a PCA to reconstruct a timeseries needs much more explanation. Why do you do need to do this? What is the advantage? Is the data really noisy?

We will clarify the text in that paragraph as it appears that the text is misleading. The PCA is used to construct the spatio-temporal pattern of surface velocity most consistent with the entire dataset. The advantage is that it reduces the noise while preserving the spatial resolution (contrary to a convolutional filter), and produces more consistent velocity maps. For completeness the timeseries of velocity before the reconstruction with PCA have been shown in the supplement. Sources of noise and the effect on data is discussed in the results section.

Changes 238-239 from:

To identify these correlations, we use a PCA and reconstruct the time series with the limited number of components needed to recover the data variance.

To:

To identify these correlations, we use a PCA and reconstruct displacement maps with the limited number of components needed to recover the data variance. This process reduces the noise level while preserving the inherent spatial resolution of the data.

Figure 4: Interesting figure. I would possibly try to regroup this to show surge and nonsurge behavior to make it to really emphasize the difference between the two states. If there is any way to consolidate into less panels this might help make things clearer. Maybe bold font for the inset text boxes? Really hard to read.

The point of this figure was to show different velocity patterns in chronological order, and to show all the important patterns necessary to understand the dynamic phases of glacier surging. The text inside the figure is already in bold or medium font to ensure better viewing

270-275: Fall speed up is quite interesting. Any idea why? Stress increase from snowfall?

It is likely a Fall-speed up as observed in other glaciers linked to changes in subglacial hydrology changes. We describe this in the discussion (last paragraph of 6.2 Surge trigger in original MS). Also, because of lack of data, we cannot go too deep into the interpretation.

283: You can also define based on propagating surge bulge seen in the strain field? To me the surge is hard to identify as a propagating velocity wave in Fig. 4 or 5.

It is, in theory, possible, but would still rely on an arbitrary definition. We tried to qualify a surge bulge evolution and surge propagation velocity, but the data is still not at a high enough spatio-temporal resolution to do so.

Fig 5: Also an interesting figure. What is your reference for along flow distance (i.e. what side is the terminus?)? It would also be cool if you put your interpretation of the 'surge front' at different locations here (maybe with some accompanied interpretive text). It isn't super obvious where this is from your velocity maps, and also might make for interesting discussion (maybe near lines 297-300).

We will add kilometer markers on Figure 2 to show the locations We will clarify that the left side of the plot is the top of the glacier.]

We don't think that a detailed mapping of the terminus is necessarily relevant for the current study, as it wouldn't change the interpretation. The advance of the terminus is plotted in the third panel of Figure 5. This mapping has also been done in different manners in a few studies already (See other papers on Shisper).

311: Did you visually check for artifacts?

Not systematically. The processing is done automatically by the algorithm, that is one of the purposes of its design.

315: Can you indicate the location of these regions on Fig. 5?

Yes, the figure will be updated to show the different zones.

345: Can the fact that these glaciers were recently connected explain some of the changes in geometry?

This is a good question. The rapid demise of the Mochowar is clearly due to inherited ice geometry. The expected changes would be limited to the dead ice zone of Shisper left after

the surge. We expect that effect to be very local, having minimal impact on the Shisper's geometric evolution.

358-360: Sentence unclear.

We propose to change the text from (L. 357-360):

We do not have enough information to discuss the differences between the two surges, but it suggest that the fast flow part of a surge cycle can vary over time. The possible difference between surges of a single glacier could challenge the idea that glaciers in different climatic regions have different surging mechanisms (e.g. Murray et al., 2003; Quincey et al., 2015), however suggest that a single mechanism has different manifestations.

To

We do not have enough information to discuss the differences between the two surges, but we suggest that the fast flow part of a surge cycle can vary over time. The possible differences between surge dynamics at a single glacier could suggest that surges follow a single mechanism with different manifestations, rather than different surging mechanisms linked to climatic regions (e.g. Murray et al., 2003; Quincey et al., 2015).

367: So surge here is inferred to only happen in conjunction with melt forcing?

Generally, yes. Although, increase melt is not the only factor, the Fall speed-ups are associated with decreasing surface melt.

386-387: As stated without additional evidence this is highly speculative.

These lines refer to the statement that our data is limited in temporal resolution and that we do not capture hourly or daily velocity changes with the Kamb (1985) citation. This citation is meant to imply that hourly and daily velocity changes can happen during a surge.

412-419: It would be interesting to know when the lake reaches flotation, as this could also induce unstable behavior similar to a surge.

Yes, it would indeed. However, the data quality does not allow us to obtain that information. We also explain the possible influence of the lake formation on the glacier dynamics.

Remainder of manuscript: I have commented enough on the remainder of the manuscript at the beginning of the review. In short, it is hard to do this analysis if the traction field is not

confidently known. While I suspect that some of your conclusions, i.e. there could be a wide range of traction parameters in a small spatial domain, could be true, but its hard to see how you could differentiate a wide parameter space from errors. A few remaining comments:

Table 3: Gillet-Chaulet 2016, De Rydt 2016, and Maier 2021 all have parameters that you can add to this list.

Thank you for these suggestions. While these papers hold interesting information, none provide estimations for the parameters listed in the table with a similar context. The parameters presented in these papers are thus not directly relevant for this table.

Figure 8: Excess velocity is difficult to understand and conceptualize how it relates to a traction relationship. Can you populate a traction curve by just looking at changes in velocity with out regards to the original velocity? I think this would need an illustration of some sort.

The excess velocity is a simple way to ensure that most the signal used is due to sliding, and not ice deformation. While it does not provide an accurate quantification of sliding velocity, removing the “background” velocity ensure that most the velocity changes observed are attributable to sliding. Thus, that the shape of the relationship will be adequate, although it could be offset along the velocity axis.

The reviewer proposes to look at changes in velocity. That change must be calculated with respect to a reference, which, by definition, will also be arbitrary. Here the reference is the background velocity. The equation showing how the excess velocity is calculated should provide a clear definition as to what it represents.

What sort of area does each dot represent? This is important when determining how independent each grid cell is.

The bin plot bins the data by range of stress / velocity combination. As described in the caption, each axis is divided into 40 bins of stress and velocity and the color represents the numbers of data points that falls in each bin. Each data point is a pixel on a velocity map.

485: The plots presented are also mostly examining spatial variations.

We do not entirely agree with this assessment. Although our figures show temporal changes, the studies mentioned at that line are indeed mostly spatial, which we say in the next line.

Appendix: This is really difficult to understand.

We will add a more detailed description of the content.

Answer to reviewer 2

This study develops an imagery processing pipeline to improve coverage and quality of velocity observations (derived by combining Sentinel-2 and Landsat 8 imagery) of a surging glacier (Shisper Glacier in the Hunza Valley) and uses these detailed observations to 1) characterize the conditioning and trigger of the surge and 2) use the range of velocities observed during the surge cycle to validate a generalized sliding relationship. The velocity processing timeline appears robust, generating realistic velocity fields, and was certainly a large task in and of itself to achieve. The manuscript is well referenced and well-written, and I appreciate the detail taken to explore multiple facets of observed and/or inferred surge characteristics. Beyond the pipeline and interpretation of surge observations, the remainder of the manuscript is somewhat weaker, and could be improved if some of my comments below are addressed or considered. Mainly, discussion surrounding known uncertainties (ice thickness) is lacking, as are quantified evaluations of how well the relationship fits within bounds or as parameters/DEMs vary, which weakens the main argument that the generalized relationship works and is physically sound and useful for understanding surge dynamics. This work is important and can potentially offer the community new tools and insight into glacier sliding, but I think several points of clarification and additional statistics need to be included prior to meeting publication standards.

Major comments:

The paper is quite long and does not read as three discreet parts as mentioned in the introduction (although the abstract describes the paper as having two parts). My impression is that much of the orthorectified imagery processing and velocity map development would be better suited for the supplement, with the main manuscript body focused on describing the surge and suspected drivers, and then applying those observations to test the generalized sliding relationship. However, the main tasks of the pipeline could be summarized in the methods with mention of how this pipeline has significantly improved velocity estimates. However, I will leave this up to the discretion of the authors and editor.

We reframe the abstract and introduction wording to show that the study has two main goals (highlighted in abstract) but saying there are 4 parts might be more accurate, (1) sliding theory, (2) remote sensing methods, (3) remotes sensing results, (4) discussion of results in light of sliding theory.

Change L. 6-7 in the abstract from

The present study has two parts:

to

The present study has two main goals:

Changes to the introduction that reflect better paper structure and more careful working about the assessment of sliding relationship: L. 89 -94 from

This paper is composed of three parts. First, we show that rigid and deformable bed theories can be combined into a generalized sliding relationship applicable for any glacier environment and dynamic evolution. Then, we present the remote sensing method, show the results for Shisper and Mochowar glaciers, and use the measurements to characterize the surge of Shisper glacier. Finally, we use the data set collected for Shisper glacier to validate and constrain the generalized sliding relationship and contextualize how the generalized sliding relationship can improve our understanding of surge dynamics.

To:

This paper is composed of four parts: (1) we show that rigid and deformable bed theories can be combined into a generalized sliding relationship applicable for any glacier environment and dynamic evolution. (2) We present the remote sensing method and workflow. (3) we apply this workflow to Shisper and Mochowar glaciers and characterize the surge of Shisper glacier. (4) We use the data set for Shisper glacier to perform a first-order assessment of the generalized sliding relationship and contextualize how this theory can improve our understanding of surge dynamics, and more generally, basal sliding.

NOTE: “validate and constrained” changed to “perform first-order assessment”.

We originally thought about splitting this study into two separate papers, one with the remote sensing methods and results, the other with the generalized sliding law and its assessment. However, because of the uncertainty in the assessment, we were concerned it would be too weak for a paper of its own. We believe the image processing is novel and deserves its own part in the main text. We would thus prefer to keep the structure as is.

My main critique of the paper is that the uncertainties surrounding estimated driving stresses are not thoroughly evaluated (although they are mentioned), making it difficult to assess the accuracy/performance of the generalized sliding relationship. I think more quantitative metrics need to be used to illustrate the sensitivity of results to tuning parameters. Uncertainty in ice thickness stem from both (1) surface elevation uncertainty (DEMs) or (2) uncertainty in the bed topography.

We have made clearer in the revision that we are not trying to assess the accuracy of the sliding relationship. Our goal here is merely to show widespread rate-independent or weakening behavior and that sliding parameters are spatially variable. Then we suggest that this method applied to a site with better data can lead to an actual evaluation of the accuracy and performance of this relationship.

Changes made to introduction reflect that. We believe that consistently referring to our work a “first-order assessment” characterizes the qualitative and coarse nature of what we can achieve with the current dataset.

-With regards to (1), excess velocity vs driving stress relationship is said to be mostly insensitive to DEM selected, with results for SRTM and 2019 DEM shown separately in the supplement. However, these comparisons are only qualitatively given. What % of observations fall outside the bounds during these cases compared to the SRTM/2019 DEM mixed assessment? Is a certain quadrant of the glacier more prone to observations that fall outside of these bounds? Several statistics or quantitative values here would be useful.

What is said in the conclusions is that the shape of the relationship remains similar (i.e. rate-independent and rate-weakening behaviors are present) and that a range of parameters is needed to encompass the data. We believe these conclusions can be made qualitatively and are insensitive to the aforementioned uncertainties.

We do not claim the relationship remains the same, and the figures in the supplement show that the best parameter set should be different. As mentioned earlier, we are keeping the analysis qualitative on purpose because the data is not good enough to produce a quantitative validation. We further believe that a more thorough analysis with the current dataset could lead to misleading interpretations, and we prefer to suggest that that can be done in further studies with improved dataset.

-With regards to (2), the text on page 180 reads “*To constrain the bedrock topography of Shisper glacier, we used the three different bed elevation models proposed in Farinotti et al. (2019) and averaged the three results, as suggested by Farinotti et al. (2017).*” More information here is warranted considering how large an impact the ice thickness imparts on driving stress. What was the range in modeled ice thicknesses compared to mean thickness? Which 3 models were used and why? Were the velocities used for the bed thickness inversion taken from before, after, or during the surge? I do think it is worth calculating changes in effective pressure vs velocity relationship using two end member modeled ice thickness fields in order to show (and quantify) the sensitivity of the relationship to unknown bed topography.

We acknowledge that we didn’t explain clearly what bed topography was used. The main product from Farinotti 2019 is a composite ice thickness estimation based on 3 different models. The models and their differences are described in details in Farinotti 2017, and the final data product and its creation described in Farinotti 2019. We have tested all the models proposed in that study, but focus on their main product which is the composite model.

Proposed text change at L.181-182 to replace:

To constrain the bedrock topography of Shisper glacier, we used the three different bed elevation models proposed in Farinotti et al. (2019) and averaged the three results, as suggested by Farinotti et al. (2017).

with:

To constrain the bed topography of Shisper glacier, we use the data products from Farinotti et al. (2019). For Shisper glacier, the ice thickness was estimated as the composite of three different models, as detailed in Farinotti et al. (2017). The specific DEM used is a combination of ASTER and SRTM DEMs for which the timing of acquisition is not specifically known. It is likely in the mid-2000s, after the last surge of Shisper glacier.

To show the effect of using different bed topographies, we have added figures to the supplementary material showing the velocity vs driving stress relationship for all three beds available. We have also added a figure in the sup mat showing the different bed elevations resulting from the 3 models and the composite bed elevation.

However, we don't believe we are able to calculate changes in effective pressure, we can merely qualify them.

How were the upper and lower values of σ_{max} (maximum resistive stress) selected? The upper bound of 500 kPa falls well outside of previously published literature as per Table 3. It seems that this value was selected in order for the upper bound to “cap” the observations and encompass the majority of observed variability. However, how can results showing observations generally fall within curated bounds created using parameters tuned to fit the observations a robust way to validate the generalized relationship? Perhaps I am overlooking something, but this seems like circular reasoning to me, and some further clarification would also aid readers.

This is correct. The parameters were indeed chosen to encompass the data shown, and the data fits within these bounds. This would not be the case if we used a Weertman-type sliding relationship. At lines L 453 to 457 we describe the choice of parameters as likely non-unique and our selection qualitative. Again, we are not looking for a robust validation, we are trying to show a qualitative fit, which can be done visually.

Perhaps some of the text is misleading on L. 461-462 as this was meant for p and q only. We propose to change

This range of value is within the bounds of proposed parameters in the literature (Gagliardini et al., 2007; Iverson, 2010; Zoet and Iverson, 2015, 2020).

To

This range of values for p and q is within the respective bounds of proposed parameters in the literature, and sigma_max and u_t appear realistic (Gagliardini et al., 2007; Iverson, 2010; Zoet and Iverson, 2015, 2020). Only laboratory or numerical experiments are available to compare the latter two parameters hindering a direct comparison.

The reason for the large driving stresses is explained L 443-444:

"The ice fall and the front of the terminus show particularly high driving stresses because of surface slopes greater than 30° (Fig. 9). "

Minor comments

Section 5.4 – mass balance of Mochowar glacier

The inclusion of this section is confusing without a companion section for Shisper glacier. The most important elements of this section can probably be integrated more seamlessly into sections 5.1 and 5.3.

Section 5.3 describes the mass balance change at Shisper, we believe this is the companion section.

I am interested in the lack of evident fall speed up in 2017 (based on Figure 5), preceding the onset of the surge in winter 2017. Can you comment on how its absence relates to the broader picture of hydrology-driven surging?

This is indeed intriguing. It is important to clarify that the lack of evidence in the current data set for a fall speed up in 2017, doesn't necessarily mean it didn't occur. We already discuss the fact that the nature of the current dataset means that we will miss some aspects of ice flow that occur at shorter timescales than time difference between the pictures (see L70 71: "*Glacier velocities derived from remote sensing will therefore consistently miss sub-monthly fluctuations, in particular those at the daily or sub-daily timescales which can be significant*"). Also see lines 380-389 for further discussion.

Table S1 – it seems that in 2016 and 2017, the time separation between pairs can be quite large, reducing the temporal resolution of summer to autumnal velocity measurements. How might this impact the ability to constrain the timing and magnitude of a “fall speed up”?

That is correct. It can prevent us from seeing velocity speed-ups.

Section 6.2 Surge Trigger

I agree with the authors that there is sufficient evidence that surging at Shisper glacier is hydrology, rather than thermally, driven. However, I would like to see the manuscript comment on some reasons why the surge initiated when it did (as in, why specifically in late 2017, early 2018 rather than an earlier year). Is this simply due to increasing magnitudes of spring speed up in 2016 and 2017?

This is a good question. We have combined two sections of the discussion and added a discussion of the enthalpy / mass budget model as also recommended by reviewer 3.

Line 240, regarding the data cube, D. I am having trouble following how images are grouped according to an image target date. Are all images that fall within a specific temporal window of the target date included in the stack?

Sentence L 241-242 changed from:

Velocity maps are then stacked on the same data cube D according to the target image date, data cube that is decomposed in principal components.

To

Velocity maps are then stacked on the same data cube D according to the older image date. When a velocity map overlaps with the preceding one, we only keep the older velocity map for the time span between the two maps older images. The data cube that is decomposed in principal components.

Line 490 – correct spelling for “Greeland”

Thanks, corrected.

There are many incomplete/missing sections in the supplement. These will be filled in the next iteration of the manuscript?

Yes, we will clarify the supplement.

Answer to reviewer 3 (Douglas Benn)

This is an intriguing paper that is rich in both data and ideas. Among other things, it represents a bold and innovative attempt to use remote sensing observations to calibrate a proposed 'generalized sliding law'. There are some shortcomings in the implementation of this idea and the discussion of the results, but with some additional work the paper should become a very original, worthwhile and thought-provoking addition to the literature.

Thank you.

Following a well-written introduction, the paper provides an admirably clear exposition of recent sliding law literature, which culminates in the proposal of a 'generalized sliding law'. The proposed solution is elegant, and neatly highlights the fundamental similarity of existing hard-bed and soft-bed sliding laws. However, the proposed law subsumes effective pressure N into both the threshold sliding velocity U_t and maximum resistive stress σ_{max} , meaning that these two main components of the $U_b - T_b$ relationship are inherently subject to large variations, particularly on glaciers that exhibit large and rapid velocity variations (e.g. surging glaciers). This is reflected in the very large spread of values in Figure 8, which the authors interpret more or less entirely as the result of variations in N . Therefore, I feel that in its present form, the generalized law is not particularly useful.

Thank you again for these positive comments. Regarding the “usefulness” of the current relationship, we believe it is very useful to have one general relationship that can be used in any case rather than having to choose one based on assumptions on bed properties. It is indeed unfortunate that N does stand out more clearly, however, the complexity of the model at play is not a choice and retains that of previous studies.

It would be better to retain N as an explicit variable in the sliding law by replacing U_t with something like $a.N$ and σ_{max} with $b.N$, where a and b are 'constants' largely determined by bed properties (C , A_s , $\tan\phi$ etc.). Although a and b will be spatially variable, this approach might allow temporal variations in N to be approximated if τ_b and U_b are known, potentially extracting considerably more structure from the data gathered by the authors.

Being able to do so would be ideal and would indeed clarify the interpretation. While it is possible to do so for $\sigma_{max} = N \tan \phi$ or NC , it is not possible for u_t . While u_t is linearly dependent on N for a deformable bed and can be expressed as aN , where a is a spatially variable parameter accounting for till properties, N is raised at the power p (Eq. 5) for a rigid bed, and p is comprised between 3 and 5 in the current study. It is thus not possible to write the current generalized sliding relationship while isolating N . In the discussion we go into

details as to what could cause changes in either σ_{\max} and u_t and identify them as being most likely related to N .

What propose to clarify in section 2 that the parameters (except N) used to calculate σ_{\max} and u_t likely change spatially, but they are less likely to change in time. Thus, most temporal changes in σ_{\max} and u_t are likely the results of changes in N . We also clarify that σ_{\max} and u_t have very different effect on general shape of relationship.

L. 134 Changes to equation 5 to better reflect the role of N in determining threshold velocity over a deformable bed.

$$u_t = \begin{cases} C^p N^p A_s \\ f(N, \text{bed properties}) \end{cases}$$

Changed to

$u_t = \begin{cases} C^p N^p A_s \\ C_d N \end{cases}$, where C_d is a spatially variable parameter accounting for bed properties (see Eq. 2 in Zoet and Iverson, 2020),

That way N is expressed explicitly in σ_{\max} and u_t .

To add on line 136 after Eq.6 (generalized sliding relationship):

From Eqs. 4–6 N stands out as the variable responsible for temporal changes in σ_{\max} and u_t . The other parameters (C , A_s , C_d and φ) have been shown to change spatially (Gagliardini et al., 2007; Zoet and Iverson, 2020) but are not expected to vary significantly over time spans relevant for a glacier surge, i.e. a decade. The fact that N is raised to the power $p > 1$ in Eq. 5, however, precludes from writing Eq. 6 for the general case explicitly as a function of N .

The data on the surge of Shisper glacier are excellent and very interesting. The combination of Sentinel and Landsat data provides a dense and high-quality velocity series, allowing the evolution of the surge to be interrogated in detail. The description of the processing work flow should prove useful to other workers in the field. Given the quality of the data, however, the discussion of the underlying processes is disappointing. The discussion of surge mechanisms presents an out-dated binary choice between thermal and hydraulic switch mechanisms, and by dismissing the thermal mechanism, the authors conclude that the hydraulic mechanism as the only other option. In its widely accepted form, however, the hydraulic switch idea is problematical. First, no convincing mechanism has ever been proposed for why drainage systems should spontaneously switch from channelised to distributed forms (the detailed analysis of Kamb 1987 focuses on the opposite switch, which satisfactorily explains surge termination but not onset; and Fowler's model of the hydraulic

switch simply incorporates an heuristic function designed to have the desired effect). Second, the hydraulic switch idea ignores the crucial issue of where the water comes from: i.e. the influence of surface-to-bed drainage and basal melting over surge cycles. Enthalpy balance theory (Benn et al. 2019) addresses these problems and unites surge mechanisms in a single framework, which potentially provides a more complete explanation of the sequence of events observed on Shisper Glacier. In particular, the exponential speed-up of Shisper in 2018 is consistent with frictional heating - velocity feedbacks, and the subsequent peaks and troughs in velocity appear to reflect competition between surface water inputs and drainage against a background of high frictional heating, all of these being associated with fluctuations in N . Overall, the pattern of surge evolution is remarkably similar to that observed during a recent surge of Morsnevbreen, Svalbard (paper cited below), and possibly for the same reasons. I suggest the authors investigate this possibility and weave it into their discussion of the Shisper Glacier surge.

We propose to combine sections “6.2 Surge trigger” and “6.6 Outlook on surge behavior”, into one “6.5 Outlook on surge behavior” section (see proposed section below). We now include a discussion of how the enthalpy / mass budget model can help interpret certain aspect of the surge and the added value of the generalized sliding relationship to explain surge behavior. We still answer to each of the reviewers point independently as to motivate the ideas presented in the re-written section.

Out-dated binary choice of surge mechanism:

It is difficult to write a paper that describes surging without acknowledging predominant theories in the field, which is what we do. The enthalpy model is relatively recent (late 2019), and while it can produce an explanation as to why glaciers surge, the specific trigger mechanism is lacking, because of its lumped nature. In this paper, we focused the discussion on trigger and surge evolution, effect that are not captured by the lumped model

Dismissal of thermal mechanism:

The mechanism that we dismiss here is the ‘thermal switch’ from a frozen bed to a bed at the pressure melting point as a trigger for the surge. This mechanism is still popular and invoked in the recent literature (e.g., Farinotti et al., 2020). In that sense we consider shear heating a potential source of meltwater, not a thermal switch. The contribution of the shear heating on meltwater production was indeed not discussed but has now been added.

Lack of existence of hydraulic switch:

We are not aware of studies suggesting that the hydraulic switch is problematic. Kamb et al. 1987, indeed suggests a hydraulic switch to terminate the surge, although that paper is a model of subglacial drainage only and is not cited in our paper. The paper that we refer to is

Kamb et al. (1985). In that paper, the authors advance the idea of the drainage system closing at the end of the melt season leading to an increase in water pressure and thus velocities.

There is field evidence for such switch, which is not in essence “spontaneous” as suggested, but gradual (that takes weeks to month). When water supply tappers off, the channelized drainage system closes and leads to an increase in water pressures at the bed. This mechanism is illustrated in Fig.3 of Iken and Truffer (1997), where velocities show a peak in the summer followed by a decrease until the early winter, then rise again until the next summer. That rise is not associated with surface meltwater production. Our observations similarly point to a mechanism that leads to a Fall speed-up. We suspect that, in accordance with Kamb et al. (1985)’s idea, it could be due to the closing of an efficient, channelized drainage system. We think that this mechanism took place at Shisper prior to the surge, as shown in the data, and could have initiated the slow onset of the surge.

Origin of the water:

The Fall speed-up idea is centered on the fact that less water exits the glacier as the efficient drainage system shuts down. The temperature reanalysis data shows that the mean surface temperature at the elevation of the main trunk remains above 0degC at the time of the Fall speed-ups and the slow surge onset. We thus expect a significant amount of meltwater to be produced at the bed in addition to basal melt due to geothermal heat flux and shear heating.

Note that we updated Fig. 5 to now show the mean daily temperature at the average elevation of the main trunk. Previously it was showing the maximum daily temperature at the mean elevation of the re-analysis grid, which was >4000m a.s.l.

ADD NEW FIG 5

Basal melting is a somewhat continuous process and can initiate a gradual increase in sliding velocity, but it is unlikely to trigger sharp changes in velocities. With a gradual increase in water input, the subglacial drainage network would adjust to new water input and prevent a step change in effective pressures and sliding velocities (See Flowers 2008, Hydrological Processes, DOI: 10.1002/hyp.7095). It is also important to note that if the background water input remains too high, an efficient drainage network would be sustained, and surface velocities changes would be reduced. Here, we show a clear correlation between hydraulic events and sliding velocities.

In quantifying the contribution of basal melt, before the surge onset, it is small fraction of the water input (See added figure at the end of the answer). During the surge, it may become more significant and is likely to contribute to sustaining high velocities. However, all the large velocities changes observed here can be linked directly to hydraulic events similar to those predating the surge. Either a Fall or spring speed-ups or the formation of the lake.

Therefore, we believe that the enthalpy model can be essential in understanding the gradual increase in spring speed up amplitude and the fact that the glacier reaches an unstable equilibrium. In light of the data presented here and other observations of surges and the generalized sliding law, we believe that our interpretation that changes in N associated with hydraulic events drive the surge and its fluctuations holds. The basal melt production is likely to contribute to the surge amplitude, but we do not believe there is enough evidence here to suggest that it drives surge evolution.

Enthalpy / mass budget model integration with sliding rel to explain surging:

We have added this in the new proposed paragraph in the discussion.

Figure 7 shows a conceptual representation of σ_{\max} alongside the observed velocity fluctuations of Shisper Glacier. If the general sliding law is reframed with N as an explicit variable, it should be possible to replace this conceptual curve with estimates of N - potentially a much more interesting and valuable outcome. Figure 8 shows that the relationship between driving stress and velocity is extremely noisy, and this noise is attributed in the Discussion largely in terms of variations in N . If this is true, we should expect to see some structure in calculated values of N in time and space. Uncertainties will of course persist (e.g. variation in a and b as defined above; errors in the ice thickness derived from the Farinotti models), but if the authors' arguments are correct, some structure should be apparent. I urge the authors to explore this possibility in detail.

As stated earlier, we cannot reframe the sliding relationship as an explicit function of N , which makes inferences of N difficult. We believe it is best to leave such quantification to further studies with better data. As pointed out by the other reviewers the uncertainty surrounding ice thickness and surface elevation data makes variable parameter quantification difficult. The importance of N , is however, now more detailed in the reframed discussion.

The existing discussion of the importance of N in surging is not new. N is an integral component of the hard-bed and soft-bed sliding laws from which the 'generalised law' is derived, so it cannot be claimed that the generalised law 'highlights the importance of effective pressure'. However, if the authors can pull estimates of N from their excellent dataset and show how it varies during quiescence and surge, that would give the paper something really unique and valuable.

It is not new indeed, as we acknowledge. What is new is a unified framework that allows to show it more clearly. Changes in the text proposed for section 2, and the new discussion

paragraph, make the role of N clearer. This also departs from, for example, the enthalpy / mass budget approach that does not explicitly account for N. It is implicitly present in the water budget though. In many surge studies, the emphasis is often put onto the driving stress increase / decrease to explain surge onset / termination, as short-term mechanisms. What we find here is that these, similarly to the enthalpy model, are likely necessary to put the glacier in a surge-likely stage, but the short-term events responsible for the surge are probably associated with abrupt changes in N.

Proposed changes to the manuscript: combining sub-sections 6.2 Surge trigger and 6.6 Outlook on surge behavior. The new section is the following.

6.5 Outlook on surge behavior

Traditionally, glacier surges are divided in two main categories, surges triggered by glacier hydrology and those triggered by a change in basal thermal regime (e.g. Meier and Post, 1969; Clarke et al., 1984; Jiskoot et al., 2000; Murray et al., 2000, 2003; Sevestre et al., 2015a). The thermal trigger consists in an event that thaws a glacier bed that was previously frozen (e.g. Clarke et al., 1984; Murray et al., 2000). The hydraulic trigger refers to an increased basal water pressure reducing bed resistance (e.g. Kamb et al., 1985; Murray et al., 2003; Sevestre et al., 2015b). A slow build-up and termination (e.g. Murray et al., 2003) is a widely used criterion to identify a thermal trigger (e.g. Quincey et al., 2015). By contrast, an hydraulic trigger is expected to produce a more sudden velocity variations. While it remains a broadly recognized hypothesis (e.g. Farinotti et al., 2020), there is little direct evidence to support the thermal trigger of cyclic surges (Clarke et al., 1984; Frappé and Clarke, 2007; Sevestre et al., 2015b).

Neither trigger mechanism provides an explanation as to why certain glaciers are surge-type and others are not. A global glacier analysis shows surge-type glaciers are only found within certain climatic conditions (Sevestre et al., 2015a). Benn et al. (2019) propose a theoretical model of glacier surge based on enthalpy and mass budgets that captures this climatic condition and can capture whether a glacier is stable (non-surge-type) or unstable (surge-type). In this model, enthalpy is gained by adding heat or water to the system. As ice cannot be warmer than 0degC, water content is necessary to track total heat content. Mass is gained by ice accumulation and lost by melt and flow. The enthalpy and mass budgets are intricately linked. For example, an increase in water content tends to yield faster ice flow and an increase in heat production, thus to more ice melt. The interplay between the two budgets is described in detail in Benn et al. (2019). An important element of that model is that basal shear heating can play an important role in the surge cycle.

At Shisper, the increase in spring speed-up amplitude together with the pre- and post- surge differences in driving stress are consistent with expected enthalpy and mass budgets. The gradual increase in spring speed-up amplitude suggests that the glacier is in an increasing energy state and increasingly sensitive to water pressure changes. This state of higher enthalpy appears to be happening at the same time as mass is gained.

Once the glacier is in an unstable equilibrium any perturbation can start the surge, for Shisper, we infer that that perturbation is caused by glacier hydrology. Our data shows a pre-surge history of both Fall and spring speed-ups, that appear to be linked to the glacier's hydrology (Fig. 5), and thereafter the surge dynamics seems modulated by hydraulic events.

Theories of subglacial water drainage also suggest that subglacial water ways can adapt relatively rapidly to changes input (days to week), which would prevent a relatively slow increase in water input to trigger a sharp speed-up (Flower 2008 DOI: 10.1002/hyp.7095; Werder et al., 2013, doi:10.1002/jgrf.20146; Schoof 2010, doi:10.1038/nature09618). If we estimate the basal shear heating over the current data set, we find that while it is non-negligible (new supplement figure), it produces significantly less melt than the expected surface melting if the mean daily temperature is above 2 degC. We thus suggest that the basal shear heating likely plays an important role to sustain relatively high water pressures and the high enthalpy state of the glacier leading up to, and during the surge. In summary, hydraulic event triggers are necessary to drive each phase of the surge.

From Eqs. 4–6 we can further conclude that changes in N drive the different phases of the surge, rather than changes in driving stress or bed properties. Changes in bed properties such as till strength, particle or obstacle size, can be significant in space, but can be ignored over the timescale of a surge. Driving stresses change significantly during the surge (Fig. 9), with drops of up to 200kPa between 2000 and 2019. Yet, such changes remain smaller than the expected fluctuations in N . Sub-daily changes in N for Shisper glacier are expected to be at least on the order of 100kPa (~ 10 m of water) as observed for small valley glaciers (e.g., Iken, 1981; Rada and Schoof, 2018), and up to 10^3 kPa as observed during the surge of Variegated glacier (Kamb et al., 1985). For example, the significant mass wastage between the surge onset and Phase 3 is still not significant enough to prevent Phase 3 of the surge.

The generalized sliding relationship highlights the importance of effective pressure in controlling sliding throughout the surge. While we cannot readily express the generalized sliding relationship as a function of N , we can discuss its relative importance on σ_{\max} and u_t . Both σ_{\max} and u_t are controlled by a combination of N , bed properties and ice rheology at the bed (Eqs. 4 and 5).

Till strength for a given effective pressure shows little fluctuations (Iverson, 2010) and the particle size distribution is not expected to change significantly over the span of a surge. The re-arrangement of the orientation of large clast could change till properties, yet that process has not been studied in detail. Over a rigid bed, variations in bed resistance arise from changes in bed geometry and ice flow-law coefficient (within parameter A_s). Considering that, for Shisper, the ice at the bed is at the pressure-melting point before the surge, the parameter A_s can be assumed constant throughout the study period (Cuffey and Paterson, 2010). Furthermore, once the transition or skin friction regimes are reached, the effect of changes in driving stress become secondary as long as it is greater than the basal shear stress (Fig. 1). We thus suggest that bed properties have a significant impact on the spatial distribution σ_{\max} and u_t , and the temporal variations originate from changes in N almost uniquely.

The effective pressure, N , which depends on the ice thickness and basal fluid pressure, might be seen as a state variable that controls both the onset of unstable sliding, i.e. the transition and skin friction regimes, but also allows the recovery from fast sliding to occur (Fig. 7b). This means that both the surge initiation and termination can be hydraulically triggered. The idea that enhanced driving stresses lead to initiation or that draining of glacier ice and resulting decrease in driving stresses can cause the surge to terminate would require rates and amplitudes of driving stress changes that are unrealistic. Changes in glacier surface akin to those we observe over two years (2017 and 2019 DEMs) would be necessary to bring the sliding regime into the transition regime or terminate the skin friction regime, under constant hydraulic conditions. Expecting these changes to occur over the timespan of surge initiation or termination is unrealistic.

The generalized sliding relationship also offers a possible explanation for differences in surge dynamics, for example whether the velocity increase is gradual or sudden. For bed conditions where the threshold velocity is relatively large and $q = 1$, reaching skin friction will be gradual and the transition regime will span a large range of sliding velocities. This would result in a gradual increase in velocities towards the surge apex. Conversely, a relatively low threshold velocity and large q , would mean that the transition regime only span a small range of velocities, and the skin friction regime is highly rate-weakening. The surge apex would be reached quickly, and surge velocities could remain particularly high until a significant water drainage event occurs.

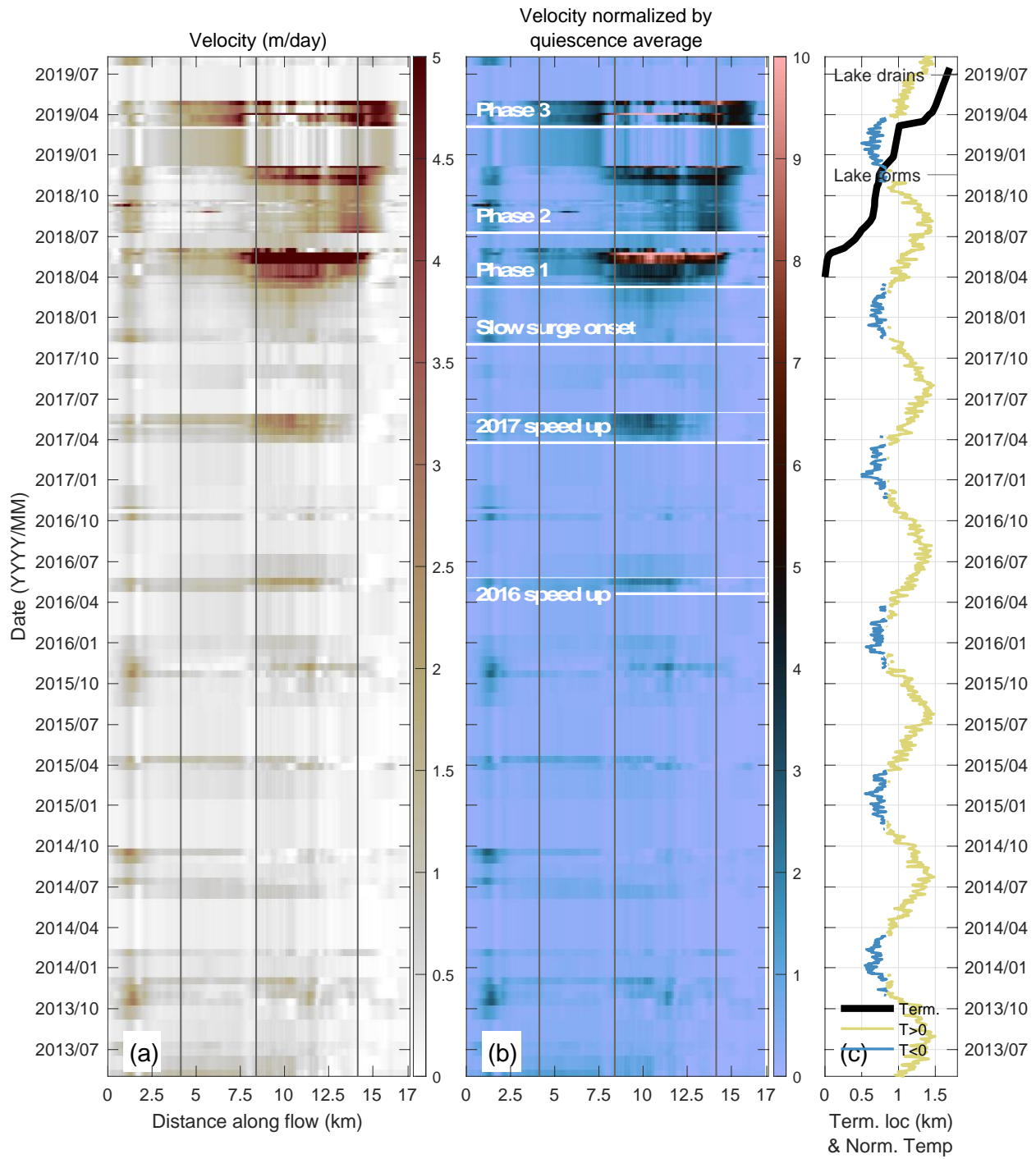


Figure 1: updated figure 5 showing vertical lines that delimitate the different zones of the glacier. The left-hand side represents the top of the glacier. The temperature timeseries has also been updated to show the mean daily temperature at the altitude of the main trunk.

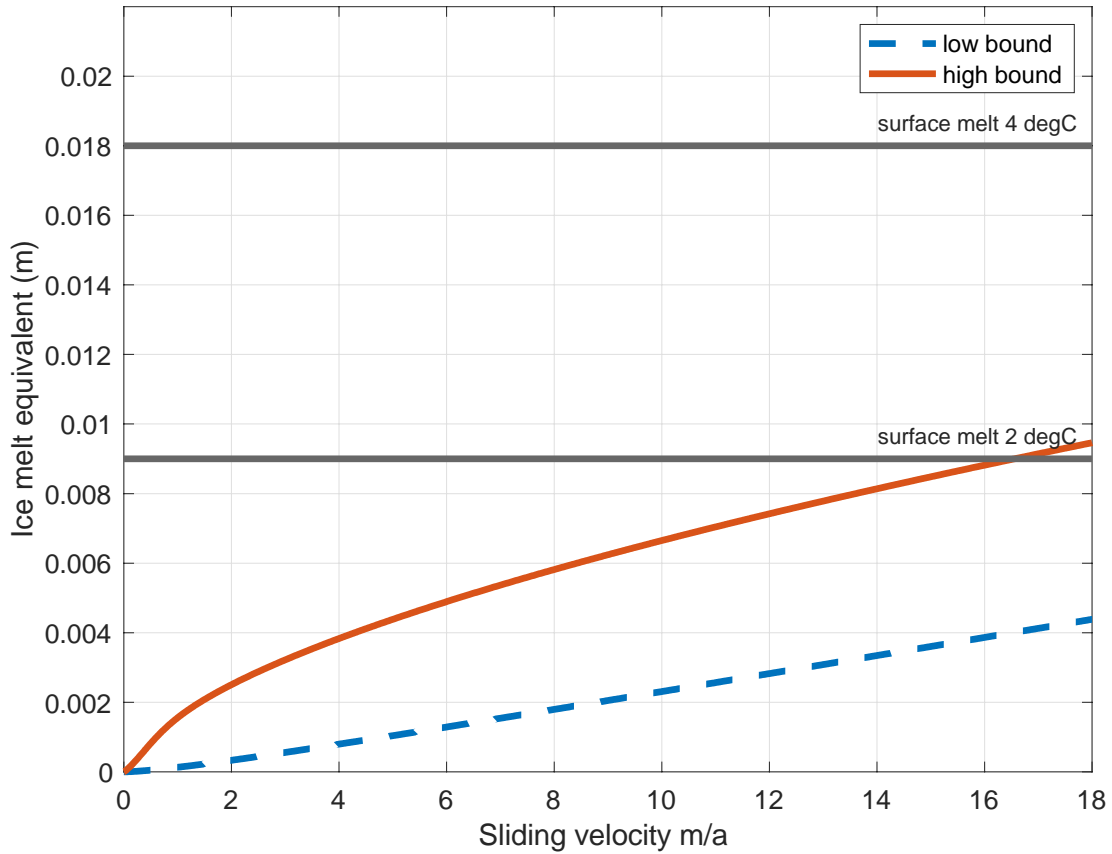


Figure 2: TO BE ADDED TO SUPPLEMENT. Calculation of ice melt in ice-thickness equivalent over a given area calculated as a function of shear heating with the high and low bound we find for the generalized sliding relationship and the expected surface melt for surface temperature of 2 and 4 degrees Celsius. These temperatures are what we observe in the re-analysis data around the times of Fall speed-ups for the main trunk zone. We used a degree day factor of 4.5, which is realistic for the Chinese Karakoram (Zhang et al. 2006, Annals of Glaciology)

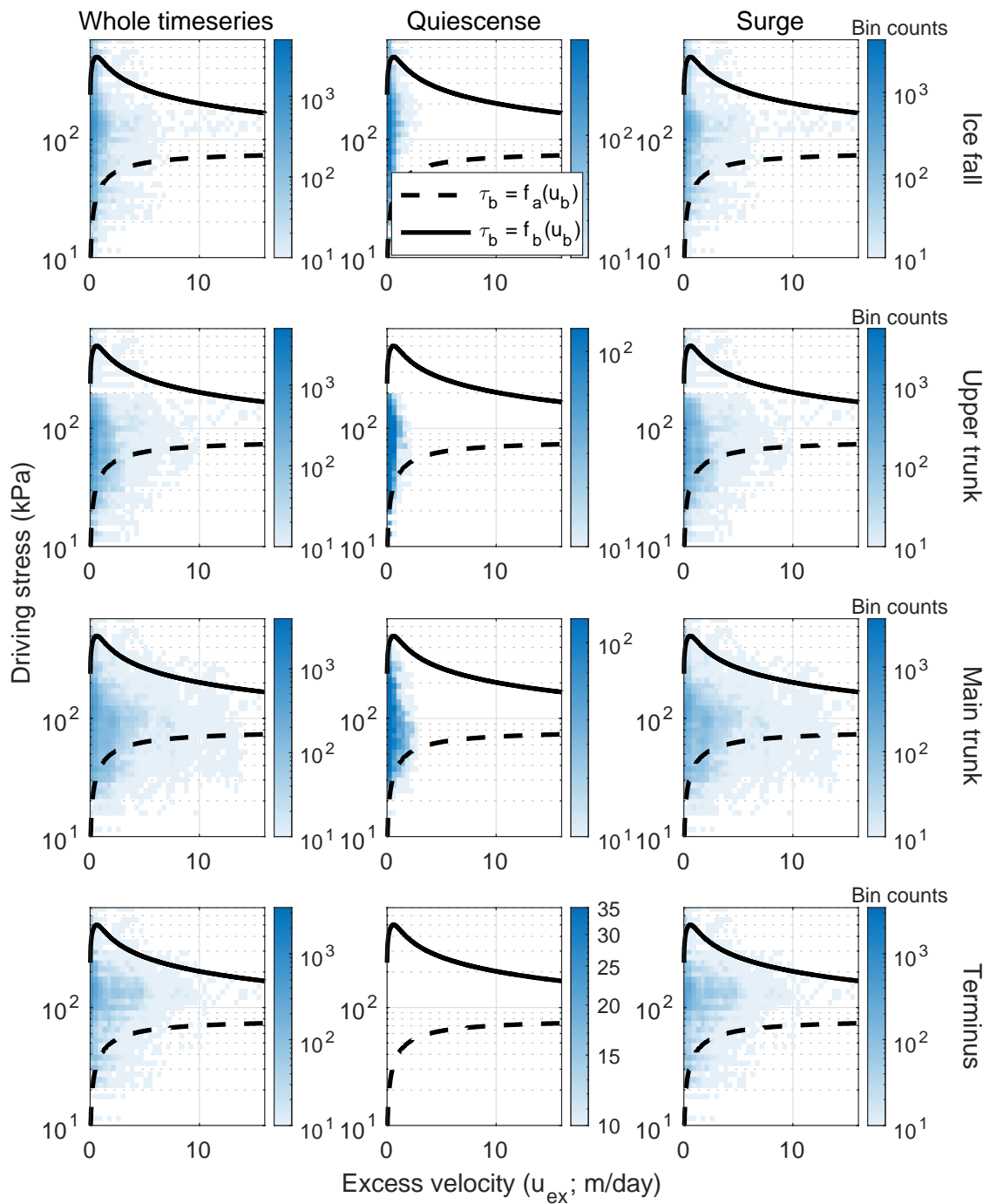


Figure 3: reproduction of Fig. 8 with the model 1 of Farinotti et al. (2019). The parameters for drawing the sliding relationship are the same as used in the main manuscript.

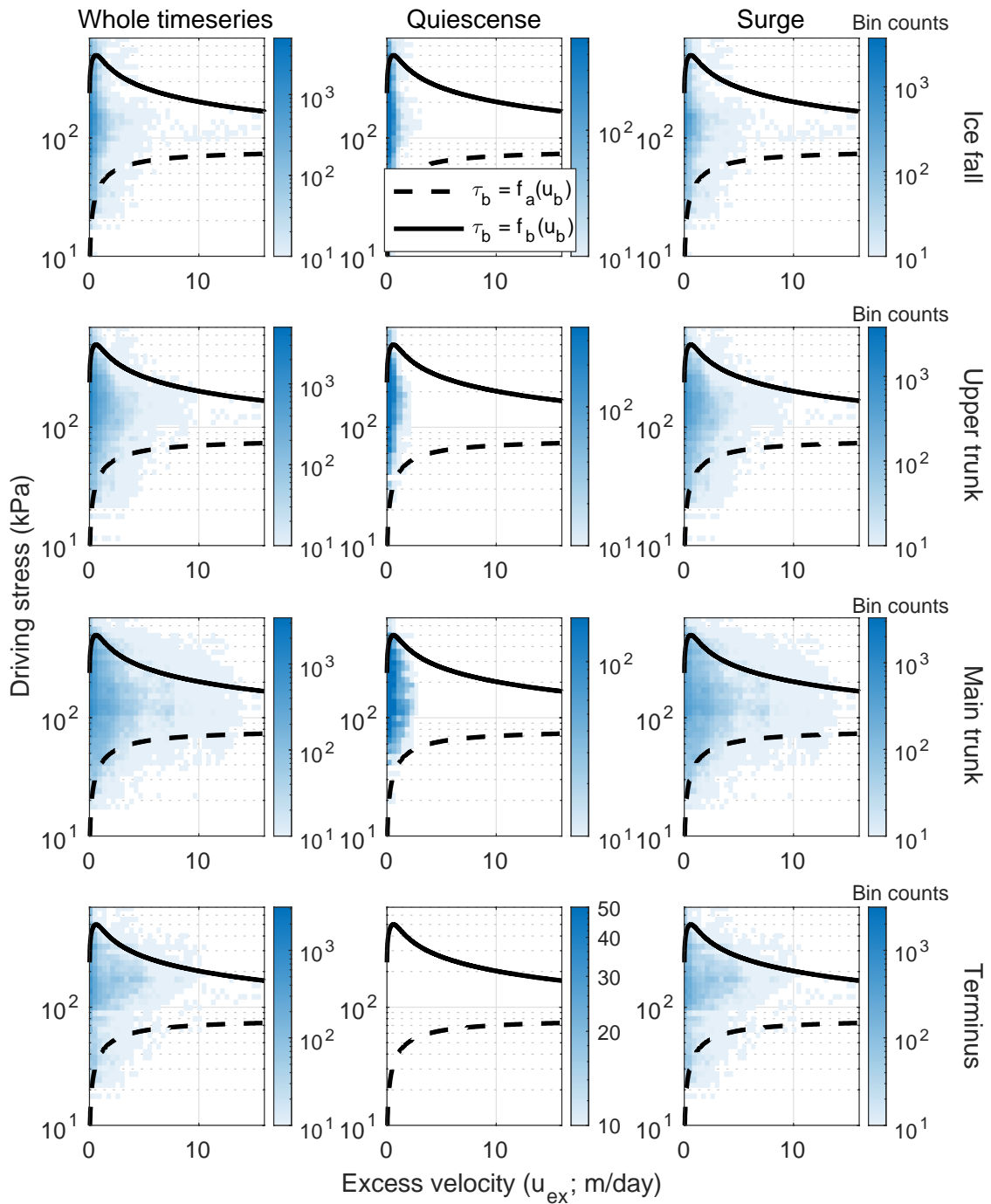


Figure 4: reproduction of Fig. 8 with the model 2 of Farinotti et al. (2019). The parameters for drawing the sliding relationship are the same as used in the main manuscript.

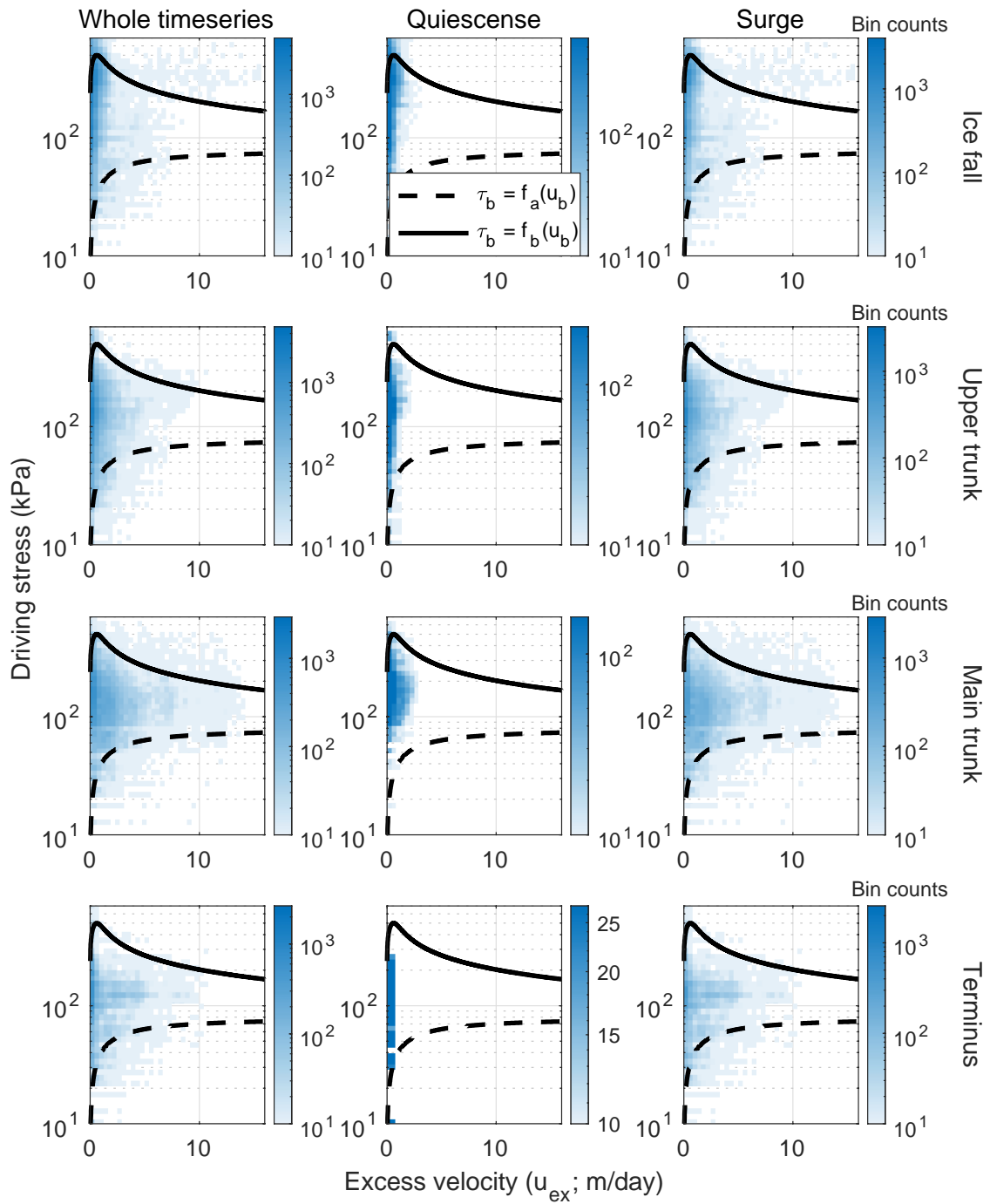


Figure 5: reproduction of Fig. 8 with the model 3 of Farinotti et al. (2019). The parameters for drawing the sliding relationship are the same as used in the main manuscript.