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

We would like to thank the two reviewers and the editor for their feedback on the manuscript and giving us the opportunity to revise and resubmit it for further consideration. The main point of concern was related to the uncertainty surrounding the estimation of resistive stresses to assess the sliding relationship. We have addressed this concern in two main ways:

(1) by interpreting the driving stress as an indirect corroboration of the sliding relationship, rather than the net sum of resistive stresses and

(2) by removing the numbers associated with the sliding relationship parameters.

Throughout the paper we have updated the idea of an assessment of the sliding relationship to state that we use the surge dataset to substantiate the sliding relationship. We believe that helps make it clearer that the analysis is qualitative only.

To remove the uncertainty associated with using driving stress as a proxy for the net sum of resistive stresses, we rephrased our argument as follows. Considering the force balance acting on a glacier, if the velocity increases significantly while the driving stress remains relatively steady, the resistive stress must decrease to accommodate for the velocity increase. We thus use the driving stress to make a qualitative argument rather than an estimation of resistive stress. Section 6.3 (Towards a unified glacier sliding relationship) has been rewritten to reflect these changes.

In section 6.3 (Implications for glacier sliding), we removed the assessment of sliding parameters and the discussion about their possible values and range. These statements were also removed from the conclusion in accordance with the comments from Reviewer #1.

Both reviewer suggested a title update and we propose to change from:
Generalized sliding law applied to the surge dynamics of Shisper Glacier and constrained by timeseries correlation of optical satellite images

To

Surge dynamics of Shisper Glacier revealed by time series correlation of optical satellite images and their utility to substantiate a generalized sliding law

Our detailed answers and manuscript modifications (green) are detailed below, in line with the reviewers’ comments (black). Note that the comments from Reviewer #1 come as a response from our answers from the first round of revision which are still present.

Reviewer #1

Review of revisions made for “Generalized sliding law applied to the surge dynamics of Shisper Glacier and constrained by timeseries correlation of optical satellite images”

I have read the revised manuscript, edits, as well as the authors response to my comments and the other reviewers. In the revised draft, some nice improvements are made in the discussion about the surge, however the main criticisms with regards to the bed friction are largely unaddressed. Much of the manuscript is publishable and interesting, but to reiterate my comments and that of another reviewer, the paper is trying to do a lot, there is plenty of material and nice data to discuss about the surge dynamics, but it gets overwhelmed especially in the discussion about friction changes which are poorly constrained. The difficulty of investigating friction relationships stems from the fact friction sliding velocity are difficult to estimate, even with good data and assumptions. In this regard, not even a relatively low bar is met and accordingly very few conclusions can be made in this aspect of the data analysis. Further modification will be necessary before publication. I address some of the friction related comments here and then I propose some modifications. I will leave the other aspects (i.e. the surge behavior) to the other reviewers since Doug Benn is far more knowledgeable than I with regards to surge dynamics.

Author Comment:

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.

My Comment:
The uncertainty is not quantified in the manuscript and a qualitative understanding still requires knowing that your signal is above the noise, which is not demonstrated here. The interpretation is also not qualitative, rate-weakening and parameter bounds are quantitative aspects of the data. As far, as the Farinotti goes, you are correct that several of the parameterizations include sliding. However, these inversions schemes are designed to calculate world-wide ice volumes using simplified inversions tuned on a regional basis and are thus subject to high uncertainty for individual glaciers.

Author Comment
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.

My Comment:
The friction field is not quantified to the degree needed to claim rate-weakening behavior. Further, the driving stress is not a high-end member estimate basal shear stress. This is especially true during large transient changes. Here, the global force balance must be maintained. This means regions where the friction is reduced will be accommodated by regions or lateral margins where the friction is increased through stress transfer. This is why a more sophisticated inversion for the basal shear stress that includes the full momentum balance is required to look at friction variations during a glacier surge.

We have removed our estimations of resistive stress and sliding relationship parameters, and reframed the argument. The global force balance indeed needs to be maintained. Since the driving stress overall decreases throughout the surge (Figure 9), at the exception of the terminus, the resistance must weaken during that time to accommodate the velocity increase. In addition, the importance of longitudinal and lateral stress coupling in the ice is enhanced as velocities increase (Blatter, 1995; Pattyn, 2002; Pattyn et al., 2008). This reduces the importance of the resistance at the bed and valley walls, further validating our findings.

This statement about the driving stress is generally true, however it is 1) not specifically what we were saying, and 2) only valid for relatively low sliding velocities. Our argument is specific to large sliding velocities, not in all flow conditions. This is backed up by several lines of evidence we could find in the literature where sliding speeds in excess of 0.3-0.4 m/day concur with driving stress in excess of the resistive stress. (Habermann & al 2013; Valot& al 2017; Hoffman and Price 2014; Minchew & al 2016; Thogersen & al, 2019). It is, in fact, well established that longitudinal stress gradients are essential to maintain the force balance at high sliding velocities, not the basal or wall resistive stresses, otherwise the shallow ice approximation would be valid for steep and fast sliding glaciers.

The authors are clearly is in favor of integrating the unified friction theory and surge observations, here are some changes I would suggest for the manuscript to be publishable close to its current form: Lines are in reference to the track changes document:

Lines: 475-495, 514-516, 608-610, Table 3 – Any discussion where it is claimed a range of parameters is found by bracketing the scatter. With the three sources of uncertainty which are unquantitified and also have the potential to be huge, this is not demonstrated, not even qualitatively as the authors claim. These lines and related discussion should be removed. As well as the last paragraph of the conclusion.

We have removed all quantifications of sliding relationship parameters.

Lines: 455-460 – This should be upfront after the second sentence. It also needs to directly acknowledge all three sources of uncertainty, and roughly how they could influence the friction field, the fact that they have the potential to be large, but you are proceeding because you are mainly using this as a proof of concept.

We have re-framed the argument using the force balance and we now focus on driving stress rather than resistive stress. If velocity increases significantly while the driving stress doesn't, the net resistance must decrease. The uncertainty in the data presented is thus now only around the driving stress, more specifically changes therein. Here net changes in driving stress during the surge are captured by the changes between the DEMs, the ice thickness estimation is only important for absolute values of driving stresses.

We believe the reframing addresses the reviewer's comment as it makes our qualitative goal more clear.

Lines: 453-455 – Not necessarily true – see comments above.

We have removed the notion that the driving stress is used as a proxy for resistive stresses here.

Changes made at l.445-451 of track changed manuscript:

From:

The mean quiescence velocity (uquiesc) is calculated by time averaging the entire velocity field for each velocity map until the surge onset in November 2017. We choose to use the driving stress (τd) as an approximation for the basal stress (τb). While this assumption is coarse, it ensures that we overestimate the basal stress at large sliding velocities (see Minchew et al., 2016; Thøgersen et al., 2019), making it more difficult to observe rate-independent or rate-weakening behavior. We then plot the relationship τb versus uex for our dataset (Fig. 8). Equation 6 enables us to identify 4 free parameters ut, σmax, p q that we can tune to bracket the data.

to

The mean quiescence velocity (uquiesc) is calculated by time averaging the entire velocity field for each velocity map until the surge onset in November 2017. We expect this choice to lead an overestimation of the mean quiescence velocity as it encompasses the enhanced speed-ups of 2015 and 2016. This underestimation of the sliding signal renders our assessment more conservative.

An idea for discussion that incorporates the unified sliding framework: a discussion on how areas with high driving stress prior to the surge seem to be the regions that accelerate the most during the surge. This is interesting, and less hampered by uncertainty. You could easily relate this to your bed friction model, i.e. what are the conditions that need to be satisfied for this to occur? This discussion is made simpler in your unified framework. Keep in mind a friction change is likely associated transients during the surge which would not be captured with the driving stress approximation.

We were not able to address this comment as it is unclear to us how the reviewer came to that conclusion. The area the most active is between km 10 and 12, yet it is where driving stresses are relatively low (Fig. 9). The highest driving stresses are observed at the ice fall which sees the least acceleration during the surge compared to other areas of the glacier.

Section 6.3 “Towards a unified glacier sliding relationship”: Could put the last three sentences of the conclusions right after 475, which would emphasize the section heading.

A similar statement added in the last paragraph as the section was significantly rewritten.

Section 6.4 – I would try to shorten the content into 1 paragraph and stick into section 6.3. The paper is already very long, and a long discussion about the general context of friction relationships and other peoples friction relationships is not needed here.

Putting our research into the context of previous work and showing how it fits together appears to us as an important part of developing a generalized sliding relationship. The evidence we present here is mostly substantial, hence the necessity of building on and using the context from previous studies.

Title: Would possibly revisit depending on the revisions.

Title changed. See introductory comment.

Reviewer #2

Review of the article entitled Generalized sliding law applied to the surge dynamics of Shisper Glacier and constrained by timeseries correlation of optical satellite images

1 General comments

This is an interesting and well documented study that attempts to deffine and apply a generalised sliding law to remotely sensed velocity of a glacier surge. The paper contains a wealth of data and present very clearly both the thought process and history behind the proposed sliding law and the specifics of the treatment of the remotely sensed velocities. That is very commendable but makes in the end for quite a long paper in which it feels that some of the messages are a bit diluted. This has already been a comment from the first round of review and the authors decided to maintain the structure of their paper as they feel that the sliding law part would not hold as a single study. My opinion on that is that splitting the paper would allow to get more in the details of the processes and limitations of the method while making the overall message of both part of the study clearer. If the author still decide to stick with the present structure I would urge them to review the title of their study as it seems that this sliding law is not really applied to the surge dynamics itself but more that the surge dynamics are used to infer the validity of said friction law.

I find the remote sensing part of the manuscript along with the description of the surge mechanisms a great addition to the literature, and the clarifications of the author following recommendations of the preceding round of review are satisfying in my opinion.

Thank you for your overall positive assessment of our revised manuscript.

I have more issues with the part pertaining to the sliding law and its application.

- I am not completely convinced that the comments of the author relating to the shortcoming of the method are completely clear in the manuscript. The authors made a great effort in explaining the reasoning behind their choice of bedrock geometry and approximations used for the basal shear stress in the review answer but it feels that those are not that clear in the manuscript. Perhaps adding a section stating more clearly the goals of the study and the reasoning behind the chosen approximations might clarify the message here.

As suggested by reviewer #1, we have removed numbers associated with parameters and reframed the way we make our arguments in a clearly qualitative manner. In the process, we have removed the explanations for the aforementioned approximations as the driving stress is only indirectly used to infer rate-independent stress. We believe that these changes simplify the reasoning as the justifications to use driving stress as a proxy for resistive stresses are not necessary anymore.

- D. Benn commented during the first round on the shadowing of the effective pressure in the chosen formulation of the friction law. The authors made the reason of this choice quite clear in the manuscript and I can see the use of the friction law as it is but in my opinion it presents some limitations that should be more clearly presented. In its current form, it seems difficult to apply this law in a coupled approach where effective pressure would be computed, in that sens it seems that the term of generalised might not be well suited and that those limitations should be clarified.

Added text Just before S.3:

"*This highlights the variables of interest to use the generalized relationship in data inversion efforts or to refine rate-and-state approaches (e.g. Thøgersen et al., 2019). It can complicate the use of the relationship in numerical models where the direct use of N might be necessary, although existing models that readily solve for water pressure, and thus calculate N, will already have chosen a type of substrate as dictated by water flow in different media (see Flowers, 2015, for a review).*"

The law is generalized in the sense that it can be applied to any glacier bed. As mentioned in the answer to D. Benn's comment, we did not make either of the existing laws more complicated than they already were. Rather, we show that the generalized formulation reduces to the previously published individual sliding laws under appropriate assumptions. Numerical models computing N already have to make a choice in term of substrate to determine which equations to use (till or hard bed), and will thus be able to choose the appropriate expression for the threshold velocity and maximum resistive stress.

- Regarding the analysis of the driving stress vs. excess velocity plots, there are a few points on which I would need some clarification.
    - The excess velocity itself is computed using the quiescent phase velocity as a reference, I wonder why the authors elected to use this velocity rather than the mean winter velocity. It feels to me that the mean quiescence velocity is roughly doubled the mean winter velocity and that difference would mostly be due increase sliding during summer.

Added L439-451 of the track changed manuscript:

We expect this choice to lead to an overestimation of the mean quiescence velocity as it encompasses the enhanced speed-ups of 2015 and 2016, meaning the excess velocity is likely underestimated. This underestimation of the sliding signal renders our assessment more conservative.

- Figure 8 and associated sup figures only use a division between quiescence and surge periods, I wonder why the different phases of the surge were not considered here and if it could yield more information.

We explored ways to plot the data with its temporal evolution, but found that the interpretation was too challenging, especially since we lack evolving DEM throughout the surge over the whole glacier. We are also aware of uncertainty in bed data and wanted to ensure we did not over-interpret the data.

Added l. 458-459 of the tracked changes manuscript:
We chose to lump the different phases of the surge together because we lack data on surface changes at a required temporal resolution during the event.

- On the design of Figure 8 I wonder if there could be some improvements to give a better idea of the fit of the curve. I would like to see the Whole timeseries panel without the over-imposed sliding laws to get a more unbiased view of the data points. It might also be useful to use the quiescence phase panels to zoom-in on the lower velocities and have a better idea of the fit of both curves in this range of velocities.

We removed the plotted relationship in the panel showing the whole timeseries to remove that possible bias. We kept the plotted lines in the other panels as we believe it is useful for the interpretation, but we made the line thinner and color lighter to make them less important.

As asked by the other reviewer, we also removed the idea of "fit" and associated legend and table with numbers but kept the lines to show the types of behavior.

We prefer to keep all panels with the same X-axis for the velocities. Zooming in would indeed give a better idea of the trend during the quiescence, but since we focus on the relatively large velocities here, we believe it is more appropriate for the current story. The part of the data set plotted in the supplement in log-log space in figure S4.9.

- On the discussion regarding the initiation of the surge I would like to get more information on the proposed mechanism, the author state that the perturbation is caused by glacier hydrology but the surge acceleration is happening after the last fall acceleration when we would expect the hydraulic system to be in a rather dormant state. I also think that missing to the discussion between surface and shear heating generated water is the difference in the temporal production of both those sources, one being seasonal and the other evolving more smoothly through time.

This has been clarified.

Also, as stated in the manuscript the surge onset is in November which coincide with the observed Fall speed-ups, not later.

On l.549-568 of the tracked-changed manuscript the text now reads:

Once the glacier is in an unstable equilibrium any perturbation can start the surge. For Shisper, we infer that that perturbation is caused by surface melt-dominated hydrology due to its periodic nature, rather than by a relatively steady meltwater production due to shear heating. Our data shows a pre-surge history of both Fall and spring speed-ups, that appears to be linked to the glacier's hydrology (Fig. 5), and thereafter the surge dynamics seems modulated by hydraulic events. Spring speed-ups are typically explained by an increase in water input overwhelming a mostly distributed subglacial drainage system (e.g. Müller and Iken, 1973; Iken and Truffer, 1997). On the other hand, the Fall speed-up is typically explained by the closure of a channelized drainage system that leads an overall increase in water pressure although the input does not necessarily increase, and that mechanism has previously been proposed to explain surge initiation (e.g. Kamb, 1987; Abe and Furuya, 2015). The presence of Fall speed-ups prior to the surge and its slow initiation showing a coinciding timing suggest that said Fall speed-up is responsible for the surge onset. The first main phase of the surge is then triggered by the following spring speed-up.

2 Specific comments

The version of the supplementary material that as been uploaded is a track changed version and might then not be the final version, however, I noted a few issues with that. In the answer to reviewers, the authors state that they added a figure in the sup mat showing the different bed elevations resulting from the 3 models and the composite bed elevation. I could not find this figure and I think it would be a nice addition to the paper.

Thanks for pointing out that omission, the figure is back where it belongs.

- The first introduction of the supplementary material seems to miss a few words but it can also be issues with the track changed format. Fixed
- There is no reference to the lake volume presented here from the text. Fixed
- Figure S4.2 might be misleading, the seasonality of the surface melt should be emphasised here, as an example, during the surge initiation in the middle of winter, the shear heating melt would actually be the largest source of water for the glacier.

Clarification added l. 50-575 of the tracked-change manuscript:

If we estimate the basal shear heating over the current data set, we find that while it is non-negligible (Fig. S4.2), it produces significantly less melt than the expected surface melting if the mean daily temperature is above 2◦Celsius.

Now reads:

If we estimate the basal shear heating over the current data set, we find that while it is non-negligible (Fig. S4.2), it produces significantly less melt than the expected surface melting if the mean daily temperature is above 2◦Celsius. This is commensurate with the surface temperature at the time of the surge onset in the main trunk.

The temperature of 2degC chosen to estimate surface melt in Fig. S4.2 is commensurate with the re-analysis data at the time of surge initiation (and other Fall speed-ups). This can also be seen in Fig. 5 where the surge onset corresponds to temperatures above 0degC. Our estimate thus show that at the time of onset, the surface melt is expected to be larger than basal melt in the main trunk of the glacier.

Reference to the temporal nature of the two forcing has been clarified in the caption as well.

- In Figure S4.3 and following, the caption should be placed as in the main manuscript. Fixed

Bellow is a list of more specific and technical comments throughout the manuscript given with line numbers:

- Line 63: Typically has an extra l Fixed
- Line 76: Isn't it as well as ? Fixed
- Line 196: ortho-recti cation is misspelled. Fixed
- Line 234: On this line and following shouldn't the window be w? Fixed
- Line 248: This sentence is not very clear to me, I would suggest: When a velocity map overlaps with the preceding one, we only keep the newer image in the overlapping period.

We changed:

"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."

to

"When velocity maps overlap, we use the velocity map with the older starting image for the timespan of the overlap."

- Line 263: The last reference to figure 4 is missing its panel (e). Fixed
- Figure 5: If possible it would be nice to add some kind of hashing for the periods in which the confidence in the data is lower. The principal component analysis takes the signal to noise ratio into account to produce the final velocity maps.
- Line 288: The altitude taken for the temperature given here is different than the

one stated in the caption of Figure 5. Fixed

- Line 327: It seems that the two sections on mass balance (5.3 and 5.4) could be merged together. Merged.

References in comments but not in the manuscript:

Blatter, H., 1995. Velocity and stress fields in grounded glaciers: a simple algorithm for including deviatoric stress gradients. J. Glaciol. 41, 333–344.

Pattyn, F., 2002. Transient glacier response with a higher-order numerical ice-flow model. J. Glaciol. 48, 467–477.