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 define 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 he 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.

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.

- 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.

- 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.
  - 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.
  - 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.

- 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.

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.

- The first introduction of the supplementary material seems to miss a few words but it can also be issues with the track changed format.
- There is no reference to the lake volume presented here from the text.
- 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.
- In Figure S4.3 and following, the caption should be placed as in the main manuscript.

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

- Line 63: Typically has an extra "l".
- Line 76: Isn't it "as well as"?
- Line 196: ortho-rectification is misspelled.
- Line 234: On this line and following shouldn't the window be w?
- 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.".
- Line 263: The last reference to figure 4 is missing its panel (e).
- 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.
- Line 288: The altitude taken for the temperature given here is different than the one stated in the caption of Figure 5.
- Line 327: It seems that the two sections on mass balance (5.3 and 5.4) could be merged together.

- Line 358: Not sure why this comment on velocity is in the mass balance section.
- Line 364: The comparison to Arolla glacier does not make a lot of sense to me.
- Figure 7: The grid in panel (a) should be pushed to the background