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.

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.

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.

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

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.

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.

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.

Title: Would possibly revisit depending on the revisions.