Response to Reviewer 1

THe authors would like to thank the reviewer for their careful reading and comments on our submitted manuscript.

In response to this feedback, we have made revisions to our manuscript following the reviewer's comments.

We now respond to the reviewer's comments point by point.

General Comments

1. There is one essential study that deals with controls of surges in HMA I am surprised you have left out of your Introduction but more importantly of your Discussion, namely (Barrand and Murray 2006). I am no co-author on that, so no bias, but they specifically tried to find controls, for the same variables that you investigated found similar results but also looked at other potential drivers. Added to that I am wondering, whether or why not you have considered to look at bed topography (simply via (Farinotti et al. 2019))? For some surging glaciers a particular shape of the valley/bed coincides with the transition from reservoir to receiving zone. That is not a request to do it, I am just wondering why you haven't expanded to other variables, like (Barrand and Murray 2006) also have done.

> We thank the reviewer for bringing this very important point forward.
> While we agree with the reviewer's comments, we wish to point out that the study from Barrand and Murray (2006) is mentionned in the Introduction. We further discussed our findings with the results of Barrand and Murray (2006) in Section 4.3.
> In comparison to the present work, Barrand and Murray propose to study two additional glacier attributes: glacier complexity and debris cover.
> Barrand and Murray (2006) numerically derive glacier complexity from the glacier permiter and area while other methods (Kienholz et al., 2013 for example) rely on semi-automatic approaches to identify the different glacier branches.
> Debris cover mapping similarly requires the use of advanced, problem-specific, algorithmic techniques.
> Here, we decided to keep our exploratory data analysis to attributes already existing in the RGI V6.0 and the velocity time series used.
> We nonetheless acknowledge that further investigations of surge-inducing parameters are necessary, and hope our inventory serves as a baseline for new studies.

2. The only really strong relation you find between variables is between glacier length and surge area (L215ff, and then Discussion). I am wondering however whether you did these comparisons also for the baseline 'non-surging' glaciers? Because I would assume that relation just holding true for any glacier, the longer it is the bigger it is and hence also the larger potential "surge area" it has. And

then that wouldn’t really be a signal from the surging glaciers per se. Or am I missing something here?

> The "surge affected area" we refer to in the manuscript corresponds to the maximum surface area showing signs of surge activity (mostly increase in surface elevation) for each glacier, over the whole study period.
> As non-surge type glaciers do not display unstable behavior, they present no "surge-affected area".
> We here demonstrate that the relationship between a glacier's size, and the surface area destabilized during its most intense surge follows a power law.
> We thank the reviewer for bringing this very important point forward, and have made this clearer in the manuscript.

3. L230ff/L240ff: Since you do not address that further in the Discussion, I am curious why you think (a) there seems to be general smaller mass loss for surging glaciers but then later you show that they substantially loose mass at the end of the surge or just after it and (b) why you think that rapid onset of mass loss is? Just because ice mass was transported to lower elevations and hence melts faster? Or are there dynamic reasons at play that disguise some redistribution of mass as mass loss?

> The reasons for this sudden onset of mass loss are still partly understood.
> The study of Bhattacharya et al., (2021) however demonstrated similar increased mass loss for glaciers in the Ak-Shirak range following a period of synchronous surging, without any clear change in climate forcing.
> There is, to our knowledge, no evidence of dynamic thinning in the years following the termination of a surge. The down-glacier transfer of a significant volume of ice to lower elevation, and subsequent ice stagnation, appear to be reasonable hypotheses to explain the increased melt rate described in this manuscript.

4. L309ff: I think a major shortcoming of your study – that is naturally just stemming from the data we have access to – is that the period you investigate is shorter than some surge cycles. Khurdopin cycles have been around 20 years, Muchchuhar Glacier next to Shisper even much longer. So in a way you may miss some within that period. So when you compare to inventories that go further back I would have expected them to catch some surges which you may have failed to catch. But your numbers are consistently higher. That would suggest that your numbers are still too low (because you missed some of the currently quiescent ones). Considering that for example (Bhambri et al. 2017)’s data and maybe others as well are accessible, wouldn’t it be prudent to compare your inventories and see which glaciers you agree on and where they find some you didn’t and vice versa? (Bhambri et al. 2017) has a very similar number but you still say yours is ‘more accurate’. From the evidence you provide I find that difficult to be sure of.

> We agree with the reviewer on the importance of this point and thank them for bringing it forward. The use of the terms "more accurate" was misleading and has been replaced in the manuscript.
> This section of the discussion specifically aims at comparing the propsoed HMA-wide

> inventory to already existing regional ones.
> As stated in the manuscript, our study differs from that of Bhambri et al., (2017), over several major points. First, Bhambri et al., (2017) do not consider surface elevaiton change datasets; this hampers one's ability to identify ice mass redistribution whether it is build up (reservoir zone) or increased mass loss (ablation zone) during the quiescent phase (see Fig 1.B South and Central Rimo examples in the manuscript) or anomalous mass gain in the ablation zone (see Fig 1.A).
> Then, the identification of surges from Landsat scenes yields more space to interpretation than a primary scan of the quantified surface elevation changes (on which either surge or quiescence related signals are usually blatant) of all glaciers in the studied area ,and is thus more likely to lead to unobsvered surges.
> Finally, our study relies on the use of the HIMAP regions proposed in Bolch et al. (2019) to define the different mountain ranges (called HIMAP regions in the manuscript). The HIMAP definition of the Karakoram differs to that used in Bhambri et al, (2017), and covers a greater area.
> We further agree with the reviewer that the number of surge-type glaciers documented is likely underestimated due to the sampling rates of the surface velocity and elevation change datasets and the considered time period, as discussed in the manuscript.

5. As you are definitely aware, more recently a number of glaciers have been found to ‘detach’ rapidly (Kääb et al. 2021; Leinss et al. 2020). Have you made sure that these detachments are not classified as a surge by you? Even maybe for yet undetected detachments?

> In the present work, we identified surge-type glaciers based on three distinct criteria.
> These criteria imply the use of datasets (surface elevation changes from DEM differences and mean yearly surface velocity) averaged over diverse time periods.
> Signals resulting from sudden phenomena like the rapid detachments of low-angle glaciers are thus likely to be averaged-out by the dominant geophyscal signal.
> As an example, no glacier studied in the works of Kaab et al., (2021) is in our present inventory.
> Furthemore, the results form Kaab et al., (2021) explicitly describe sudden detachments on low-angle glaciers as the most dynamic end of the wide spectrum of surge-like instabilities thus further demonstrating that the various physical phenomena acting on surge-like glacier instabilities are still partly understood.

Minor Comments

1. L54: A study that actually looks at economic and infrastructure impacts of a surge, and that same surge, would be (Muhammad et al. 2021). I am a co-author on that study and I also do not think it is at all essential to cite here. I leave it up to you in case you find it helpful to make your point.

> We have added the proposed reference as it is relevant for our manuscript, and thank the reviewer for bringing it to our knowledge.

2.L146: “Consortium et al. 2017” reads funny – I know citing the updated RGI is a bit strange but the official format is below, so I would at least go with ‘RGI Consortium 2017’

> We have fixed this.

3. L155 and throughout: capitalize ‘Glacier’ when associated to a specific glacier. You do that sometimes, sometimes not. Same for ‘Tibetan Plateau’ on L181

> This has been fixed.