Review of ESSD-2022- 238 by Guo et al.

General comments

The authors have investigated glacier elevation changes in High Mountain Asia (HMA) derived from digital elevation models (DEMs) acquired over a period of 50 years to obtain information about surge type glaciers. For this purpose they make use of the usually very typical elevation change pattern that result from the mass transfer of a reservoir zone to the glacier terminus during a surge and the subsequent down-wasting of the tongue and build-up of new mass after a surge. Thereby, the observed elevation changes usually reach several dozens of metres so that the obtained signal is generally much higher than DEM uncertainties. The new assessment is then analysed in terms of topographic glacier characteristics, to extract possible differences between surge-type and other glaciers.

(1) As a general feedback to the terminology, I would say this (and several of the recently published previous studies) presents an inventory of surging glaciers rather than surge-type glaciers. The authors look at real surges that happened rather than investigating surface features (looped moraines etc.) that hint to possible (unobserved) previous surges. This differentiation of terminology should be clearly stated and applied here as it could explain a part of the differences among different inventories.

(2) I think this is about the third or fourth study creating an inventory of surge-type glaciers in HMA, so there is obviously a need for such information. It also seems that it is important which team identified the largest number of 'new' surges. However, as for the previous studies using the RGI6 to mark these glaciers, there is a severe backdrop of the inventory presented here: The authors have not separated the surging glaciers from their trunk glaciers they are connected to in RGI6. This might only be an issue for a smaller portion of glaciers (a few dozens?), but it has to be done. Now the really huge glaciers Baltoro, Biafo, Fedchenko and Siachen are surge-type glaciers although they are not. They are just connected to often much smaller tributaries that have surged. To study these, they have to be separated, maybe just with a straight line to start with (and in the accumulation area). As the authors correctly describe (L325ff), it is clear that in many cases a separation in the ablation region will be difficult, as some surging (tributary) glaciers might contribute substantially to the flow of a trunk glacier or even form a joint tongue with other tributaries. I suggest skipping these complicated cases and focus on the clear ones, e.g. separate Bivachny from Fedchenko, Drenmang and Chiring from Panmah, Maedan from Chiring and Panmah from Choktoi with a straight line.

(3) This separation is not only important for glaciologic studies, but also because the authors analyse here topographic characteristics, size being one of it. When the very large glaciers listed above are in the sample instead of the 10 to 100 times smaller glaciers that really surged, it is not surprising that the result is always that the surge-type glaciers are (on average) larger and longer than the other glaciers. Although this general result might not change after separation, the proper separation is mandatory for a sound assessment of related characteristics. So there is no way around it. A different issue but leading to the same bias is the use of RGI6 for the analysis. There are many smaller glaciers missing in RGI6 and a large number has been manually digitized to the effect that partly huge areas with rock outcrops are included in the glacier outline. This leads to glacier extents that can be too large by 50% or more. Please use for the analysis the outlines provided by the GAMDAM2 inventory (doi.org/10.5194/tc-13-2043-2019).

(4) I had a look at the outlines presented here and compared them to the inventories by Bhambri et al. (2017) for the Karakoram, Goerlich et al. (2020) for the Pamir and Guillet et al. (2022) for entire HMA. Of course, there are differences between the inventories due to different methodological approaches (elevation changes, velocities, length changes), time frame considered or inclusion of surge-type rather than surging glaciers (see above). However, it is argued that the inventory presented here should be more complete as it considers a longer time frame for the analyses. As described in Section 6.3 and confirmed by an overlay of datasets, many glaciers that have been identified as surging in the Guillet inventory are not included here. The authors explain this difference by possibly diluted elevation differences when observed over a longer period of time. I can live with this explanation, but how can this inventory then be more 'complete'? Completeness is mentioned as a key argument for this new survey at many places (L9, 11, 44, 66 and 73). But why should I favour this inventory over the other when obviously so many 'verified' surging glaciers are missing here?

To get the dataset presented here more complete, the authors should at least perform a back check for the glaciers classified as surging in the Guillet et al. inventory but not in this one. A comparison to very high-resolution imagery (e.g. ESRI Basemap) sometimes shows impressively surging glaciers or distorted moraines at these locations. Also a visual inspection of the KH-9 images might reveal that glaciers have surged back then. Some more details about why these glaciers have been missed would be helpful to provide a better insight into the limits of the various approaches. Vice versa, glaciers identified as surge-type here but not by Guillet at al. should also be double-checked. When mass balances are slightly positive and surge marks are missing, it is also possible that glaciers just advance.

(5) Related to the above, the criteria presented for surge identification in Section 4.3 seem to be unbalanced. For example, terminus thickening is included as criterion I-1 but thinning is only in class IV-1, i.e. strong (post-surge) thinning does not have the same weight as thickening. Given that glaciers showing only thickening might just advance rather than surge (creating false positives), I would even argue that strong thinning (see L35) is the better indicator and should at least also be listed in category I. For example, in Figure 2a one can see the strong down-wasting of both Western Kunlun glaciers (just below the last zero of the '2000' annotation) and in Figure 2b the re-advance of its eastern branch, whereas the western branch is still thinning. Indeed, this part is now also surging again and the assignment 'non surge-type' is obviously wrong.

Finally, I suggest removing the climatic interpretation in L342 to 366. We have recently learned that ESSD will refocus on publishing dataset descriptions. This part (although interesting) goes clearly far beyond this. Maybe you can just cite here some studies looking at the interpretation in more detail. Below is a condensed summary of the general points above.

1 Please use proper terminology, distinguishing surging from surge-type
2 Please separate tributaries connected to a larger trunk glacier to the extent possible.
3 Please use GAMDAM2 instead of RGI6
4 Please double-check and correct the present inventory with Gilles et al. and satellite images
5 Please shift class IV-1 'only down-wasting' to class I-4 and re-check then classes I-1 & I-4

Specific comments

L35: 'melts fast' this is certainly correct in a relative sense, but still it might take 20 to 30 years or even longer. So maybe write 'relatively fast'.

L41: 'However, a glacier surge' (or use plural: glacier surges are')

L43: When speaking of contemporary glaciers, I suggest writing 'glacier hazards' or maybe even better 'glacier-related hazards'

L44: Glacier-related hazards are certainly an important point for studying surge-type glaciers, but an inventory has no prognostic characteristics and does thus not help to solve the problem. By knowing where these glaciers are, one does not know where the next hazard will occur. I would thus better argue here with the importance of having such an inventory when studying climate change impacts on glaciers, e.g. mass balances or length changes. For such studies it is of high importance to distinguish the two samples.

L67/70: Both studies identified surging glaciers (i.e. surges were observed) rather than surge-type glaciers (which might have surged in the past). Please adjust the terminology.

L73: I would write 'In this study'

L77/78: Such analyses is not allowed in ESSD.

L124: Please use GAMDAM2 instead, RGI6 has quite a lot of issues in this region.

L172: Please also explain in this section how glaciers that are just advancing were distinguished from glaciers that are surging (when criterion I-1 is used).

L182: Please shift the class IV-1 'Strong thinning only' to class I (e.g. as I-4) and get the currently surging west Kunlun Glacier included.

L218: Please revise Section 5.2 after small (part-time) tributary glaciers have been separated from the much larger trunk glaciers. Neither Fedchenko, nor Siachen, Baltoro or Biafo (and several others) are of surge type.

L242: See comment to L218, this applies also to Section 5.3

L296: See comment to L218

L298: This might change a bit when separating small surge-type tributaries from their trunk glaciers. Please get at least the largest ones out of the sample.

L332: Yes, fully agreed, it can be difficult. But it has to be done anyway. It makes no sense to present a topographic analysis for the glacier complex when it is 100 times larger than the glacier that is actually surging.

L356/7: I would not draw such a conclusion when surge cycles are longer than the observation window. This observation can still be by chance.

L396/397: Please clearly separate surge-type glaciers from glaciers with observed surges to get the numbers correctly interpreted.

L400/1: I do not understand this calculation: When Goerlich et al. reports 176 surging (not surge type!) glaciers and the present inventory includes 156 of them, why is this 126 more rather than 20 less? Please better explain what has been calculated and compared here.

L426: Please provide the final selection of glaciers also as point data and in shape file format.

Figures

Figs. 2 & 5: The orange to red colours are too close to properly separate them. Please use more different colours.

Fig. 3: The circles can be a it smaller to see more details

Fig. 5: Please revise after glaciers have been separated. Fedchenko, Biafo, Baltoro and Siachen (among many others) are not surge-type glaciers. This is highly misleading information that can not be used for anything.

Figs. 6 and 7: Please use more distinct colours, they are too close.

Fig. 9: Make circles smaller to see something. The foreground data could also be dots only.