

Comment on **essd-2022-238**

Gregoire Guillet (Referee)

Referee comment on "A new inventory of High Mountain Asia surging glaciers derived from multiple elevation datasets since the 1970s" by Lei Guo et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2022-238-RC1>, 2022

General comments:

The manuscript presents a new inventory of surge-type glaciers in High Mountain Asia, derived from glacier surface elevation changes computed from various DEM sources, between the 1970's and 2010's.

The manuscript tackles an important topic which is the identification of surging behavior over a large spatial and temporal scale using remotely sensed glacier observables and thus aims at proposing an updated inventory by incorporating historical data absent from other studies.

This problem is of significant importance to the community and the proposed paper is of overall good quality.

However, there are some major shortcomings that need to be addressed:

- The authors here rely solely on anomalous surface elevation change pattern to identify surging behavior. This can lead to false identifications of glaciers present similarly altered surface elevation change signal (See specific comments for more). Some of the widely known shortcomings of the datasets used in this study, as well as the existing corrections (SRTM C-Band penetration correction) are not accounted for in this study, which may lead to further false positive identifications.
- The authors rightfully propose a classification that assigns a level of certainty over the potential surge behavior of each glacier. This is a valid approach since the authors only have one identification criterion and I commend them for doing so. However, when analyzing, discussing and presenting their results, the authors seem to forget that surge-type behavior is uncertain for some glaciers and consistently mention 1015 surge-type glaciers while "only" 704 present indications of surge-type behavior.
- Finally, some of the Introduction and Discussion lack context and an adequate description of the state of knowledge of what are glacier surges and the processes that govern glacier instabilities.

I want to restate my support for this manuscript and the work it presents. Given its current state however, I suggest that the authors make major additions and changes to both their methodology and results before this manuscript can be considered for publication

As an example, adding more than one identification criterion would strengthen the confidence in the presented results. The authors could for example use available satellite images to visually investigate changes in surface and geomorphological features like

crevasses, supraglacial ponds or looped moraines.

Specific comments:

L15-16. This statement is misleading as your classification is based on the confidence degree you have over certain surge events - all the glaciers in your inventory have different level of confidence.

This directly differs from the methodology used in Guillet et al, which I assume is the work you refer to, which used different identification criterion to investigate surges.

L23. This relationship was actually first described in the enthalpy balance theory of glacier surges proposed by Benn et al 2019. Please rephrase this statement.

L27. Isn't that just because of a sampling bias, as a longer observational period allows to identify more surges ?

L32. Please describe what do those phases entail and how do they differ.

L34. While I agree that the physics governing the unstable flow exhibited by surge-type glaciers requires a better understanding, I also think that speaking of 'enigma' disregards the substantial efforts made in the recent years to further our understanding of glacier surges.

The authors should here at least refer to the works of Sevestre and Benn (2015), Benn et al (2019), or Thøgersen et al. (2019) (among others) in order to provide an up-to-date synthesis of the state of knowledge on the physics of surge-type glaciers.

L35. Please be more specific in this statement. 'Fast' is very vague without a reference. This sentence is not easy to read and could benefit from being segmented and more detailed.

L46. Not all surging glaciers show terminal advance. I suggest reading through the works of Paul et al. (2017) and Steiner et al. (2018, already cited in your work).

L58. Contrasting elevation change signal is indeed a powerful tool to identify surge-type glaciers when it is associated to other remotely sensed observations (surface velocity, changes in crevasse pattern etc.).

However the statement made here is misleading as both Lv et al (2019) and Guillet et al (2022) also changes in surface velocity to identify surges.

Furthermore, Viay and Braun (2017) focus on the early 21st century - a period of 12 years - not necessarily what could be called a 'long temporal scale'.

L92: As mentioned, the NASADEM used in this study originates from a reprocessing of the C-band SRTM.

This data is however known for suffering from important radar penetration.

This is more than likely to create spurious elevation change signal in the upper reaches of glaciers which can then lead to false identification of build-up phases, for example.

This has to be addressed here, since the NASADEM is extensively used throughout this study.

Sec 4.3: This is my main concern with this manuscript.

Surge-type glaciers are mainly identified using only surface elevation changes.

To me, this approach is a bit hazardous, as many processes can cause altered glacier surface elevation changes compared to what would be deemed as "normal" or "standard". I am here typically thinking about glaciers affected by landslides for example (Hewitt, 2009, Van Wyk de Vries et al., 2022) for example.

This needs to be further investigated, including the possibility of using additional criteria to validate the identified surges. Such criteria would typically be morphological (Looped moraine or changes in crevasse patterns) as I doubt of the availability of glacier surface velocity datasets for the 1970s-2000 period.

L208: Again, this statement is extremely misleading. Glaciers that are considered as "Possibly" or "Probably" surge-type cannot be considered as such. There is no clear surge signal in the elevation change of those glaciers over the studied time periods, otherwise they would be qualified as "verified". In total, you have identified 704 surge-type glaciers at most.

L240: Please show examples of these glaciers as well as their elevation change signal for all considered periods.

Sec 6.2: This section is very qualitative and provides very few quantitative information. Please provide estimates of the quantities you are referencing.

L308: You mention randomness, where I believe you mean variability

I do not understand the point made between L311 and 314. Please clarify.

You further mention that glacier median elevation is "irrelevant" for other topographic parameters.

Again this statement is pretty hard to understand, even though I assume you here mean "correlated".

A glacier's median elevation is however very correlated to its elevation range. Similarly glacier area is most likely correlated to glacier elevation range (as glaciers are relatively elongated features) so I do not really understand the point of this statement. If I misunderstood your point here please correct me and clarify.

Sec 6.3: This section is a bit problematic at the moment for several reasons. First, the authors propose an inventory based on only one identification criterion and compare it to the one of Guillet et al 2022, which only comprises glaciers for which two criteria of active surges could be observed. Apart from the intrinsic methodological difference, this comparison makes little sense, as the inventory proposed here also comprises glaciers for which surging behavior is uncertain. This comparison should only target 704 glaciers in the inventory proposed here - which then lowers the difference with Guillet et al (2022) to 38. This in turn leads to several questions, most notably on the length so-called cycle of surging glaciers (assuming that there are no false positives in any of the inventories). Second, the authors mention that with 349 glaciers more (1015-666), they only observe a small (4%) increase in the glacierized area covered by surge-type glaciers in HMA and that, hence, the newly identified ones are relatively small. This is in stark contrast with previous studies documenting surge-type glaciers as systematically bigger than non-surge type glaciers (Jiskoot et al., 2011, Sevestre and Benn, 2015, to cite a few) as well as what is predicted by the enthalpy balance theory (Benn et al., 2019) and needs further investigation.

Hewitt, K. "Rock avalanches that travel onto glaciers and related developments, Karakoram Himalaya, Inner Asia." *Geomorphology* 103, no. 1 (2009): 66-79.

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