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Comment on egusphere-2022-765

Anonymous Referee #2

Referee comment on "Surging glaciers in High Mountain Asia between 1986 and 2021" by Meiping Sun et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-765-RC2, 2022

This study by Sun et al. analyses the distribution and frequency of surges in High Mountain Asia (HMA) by means of optical remote sensing data. First, they identify evidence of glacier surges by investigating manually estimated terminus advances in series of Landsat optical images from 1986 until 2021. These evidences are further consolidated with the use of elevation change maps derived from ASTER DEMs and ice velocity obtained from the ITS LIVE data set (the latter possibly being used only for a few test cases). Then they analyze the occurrence of surges over time, both at decadal and monthly scales. They describe more in-depth the surges of two glacier complexes. Finally, they analyze the periodicity of surges for a set of 36 glaciers for which at least two surges are observed over the study period.

The theme of glacier surges in HMA is highly topical, as proved by the wealth of recent studies on the matter, which are well referenced in this study (in particular Table 2). The topic of surge identification and inventory has been largely addressed with several regional inventories now existing (as listed in the present study). I do not think that this study adds a lot of additional information on the matter, considering that the presented inventory is most likely incomplete (see comment about section 3.2). On the other hand, this study provides observations of terminus changes down to bi-monthly sampling over the entire region, over a period of 35 years, which I believe is unique and is undoubtedly a useful resource for studying glacier surges. I would like to congratulate the authors for this tedious work. However, I feel that the method used to analyze this dataset is flawed by a sampling bias and cannot be used to draw any significant conclusion (see comment on section 4.2). In summary, I believe this study would either require extensive additional work to improve the analysis, or could be transferred to a science data journal with an open access to the set of terminus positions.

Specific comments:

- Article structure

Overall, the article is well written, except in a few places that would require rephrasing (see technical corrections). However, I have two concerns on the general structure. The first is that the paper tends to discuss topics that are very indirectly relevant for that study, in particular in the introduction, data and discussions sections (see below). This article should be shortened to focus only on the topic of glacier surges. The second is that some methods are described in the results and some results in the discussion, so the structure need to be re-organized. The number of references is also very high and many of those are probably not needed, or are sometimes not appropriate (see technical corrections).

- Section 1 - Introduction

The goal of the introduction is to provide the context for the topics addressed in this study. The topic of temperature increase (L 21-24) or glacier mass balance (L 25-31) is very indirectly related to that study and I do not think this part is needed. At the very least, it could be shortened to 1-2 sentence stating that temperatures are rising and glaciers are retreating, with a few main references and without providing such detailed numbers. Finally, the introduction discusses the periodic nature of surges (which is later addressed in that study).

- Section 2 – Study area

Again, this section dwells on subjects that are not relevant with regard to the rest of the paper. For example, L 69-81 discusses the provinces covered by HMA, the climate setting or the river basin in HMA, which are barely discussed in the rest of the paper. The river

basins are actually used, but I do not believe that the analysis with river basins is relevant (see comment on section 4.1).

- Section 3.1. – Data sources

The details on how RGI was initiated (L 90-94) or about the SRTM DEM (L 118-121) can be removed/shortened, as these are very standard products in the community. I would instead point to the right references (e.g., Farr et al. (2007) for SRTM is missing).

In section 3.1.1. the long discussion about the pros and cons of individual inventories leaves an unclear message on what inventory is used in what region. For example, L 102 "In this study, the GAMDAM dataset is used as a reference for glaciers in southeastern Tibet and other regions in HMA.". What are "other regions"? Please clarify precisely where each inventory is being used. Maybe replace figure 1 with a map of outlines used, coloured by inventory source? The choice of the inventory is important, particularly how they take into account glacier complexes, when discussing the typical area of glacier surges or total area affected by surges.

In section 3.1.2, you state L 113 "the image fusion of visible band and panchromatic band was carried out". Was the imagery provided pan-sharpened or did you carry-out the processing yourself? If the latter, please provide a reference to the methodology used.

- Section 3.2 – Methods

The methods section starts with a rather long description of methods used in previous studies (L136-140) that are not used in that study. I would remove that part as it's not needed. If the focus on the paper was on the methodology (which is not the case here), this would otherwise belong to the introduction, not the methods.

There are also several equations describing how uncertainty is calculated (equations 1 to 4) but all results are provided without any uncertainty values, so it's unclear how these equations are used. Additionally, these uncertainty estimates are very crude, since as

highlighted by the authors, they only "consider the error resultant from spatial resolution of remote sensing images" (L 147) and neglect errors related to poor image coregistration or subjective delineation. I would also like to point out that equations 3 and 4 are not appropriate for calculating mean elevation uncertainties, as they assume fully uncorrelated errors between pixels, which is incorrect. I refer you to Rolstad et al. (2009) or Hugonnet et al. (2022) for more rigorous estimations of the uncertainty.

Section 3.2.4 is the most important aspect of this work and yet it's probably one of the shortest.

First, the methodology is too vague and subjective to ensure reproducibility of the results. The first criteria of front advance by 150 m/a is very objective, but the second and third criteria are very subjective. For example, "a significant surface elevation decrease" (L 188) is too vague, what does significant means for you? Please provide a typical "significant" change. I also have a hard time imagining a glacier that advances by over 150 m/a and that displays neither surface characteristics typical of a surge (criteria 2) nor large elevation changes (criteria 3). If several glaciers that passed criteria 1 but were excluded of the inventory based on criteria 2 and 3, please describe them and explain why.

Second, the methodology rules out any glacier that does not display a front advance of 150 m or more over one year. However, there are evidence of surging glaciers that do not display such a large terminus advance such as Hispar (Paul et al. 2017) or Khurdopin (Steiner et al., 2018). Under these conditions, it is clear that the inventory provided here cannot be complete.

Third, the ice velocity observations are mentioned several times as a basis for the surge identification (L 194, 366-367) but according to section 3.2.4 this is not used in the identification, i.e. a glacier with negligible terminus advance but strong velocity changes have not been included. It seems that the velocity has been only used for the study cases in section 4.3, so it should be clarified.

In summary, the authors should clarify what is the aim of this study: 1) Provide a complete inventory of glacier surges or 2) study a subset of surging glaciers (as in Vale et al., 2021). In case of 1, then it is not sufficient to only consider the criteria of front terminus advance and the identification should also consider glaciers with negligible terminus advance but with significant changes in velocity or changes in elevation, as is the case e.g., in Guillet et al. (2022). Case 2 would be perfectly fine, but then it should be made clear that you are only considering a subset of surging glaciers. In that case, statements like "which made our results more accurate" (L 367) should be avoided. Then the comparison with previous studies (section 5.1) is a bit irrelevant, since it is clear that some surges will be missing from that inventory. At best, the authors should focus on surges not previously identified and provide an explanation as to why these were identified here and not previously (surge happened outside of the study period of previous studies, lack of observations etc).

Finally, it is crucial for transparency that the authors provide the list of glaciers considered here, in the form of a table, with the criteria used to identify it as surge type (e.g. maximum advance rate, surface features, min/max elevation change observed etc.).

- Section 4.1.

This part is not particularly novel, as the distribution of surges has been addressed in previous, more complete inventories such as Guillet et al. (2022). An issue with the analysis of surging glacier area in this section and also table 1 and figure 3 is that glacier's area is extracted from glacier inventories like RGI6 which consider entire glacier complexes. This is frequent that surges occur only on branches of such glacier complexes (e.g., your study case of Musta glacier). Therefore, this analysis (and many other previous studies) overestimates the total area of glaciers affected by surges and the typical size of surge type glaciers. I recommend this is clearly stated in the results. Ideally, since the authors have undertaken the tedious work of manually editing glaciers terminus, they could consider the area of surging glacier branches only. If that is already the case, then this should be highlighted, as it is unclear in the present article.

Also, I do not think that the analysis of glacier distribution with river basin (L 206-211 and Table 1) is relevant, because the impact of surges is not so much on water resources but rather on local hazards. Therefore, an analysis by range is largely sufficient.

- Section 4.2

The availability of up to bi-monthly observations of glacier terminus changes for the entire region is probably the most interesting asset of this study, so this section should be crucial. However, I have a lot of concerns regarding the analysis done here.

The analysis starts with the number of advances observed over the study period. But the authors never described what they consider an "advance". Is it an advance over a fixed period of time (e.g., over a year, over a month), or between two available consecutive images? I believe this is the latter, but it is unclear. If that is the case, this is a huge flaw given that their observations are on an irregular temporal sampling, which in turn is dictated by the availability of images (if there were twice as many images available, there

would probably be twice as many advances).

In particular figures 4 and 5 are clearly influenced by this sampling bias. The lower number of available images before 2000 (on figure 2, TM images that are the only ones available prior to 1999 make about 1/3 of the observations for a 14-year period) is clearly reflected in Figure 4, and so this figure does not provide any scientifically relevant result. Similarly, the right panel in that figure is most likely biased by the better quality of images in the summer months, as highlighted by the authors at L 228-230. Since the authors seem to be aware of that sampling bias, I am puzzled as to why the authors conducted such an analysis.

In brief, the authors should spend more time trying to find a way to analyse this dataset that is not influenced by the image availability. An option might be simply to divide the number of advances by the number of available images, for each time period (year, month) and for each region. They can test the reliability of their analysis by taking a subset of observation with no temporal gap, and randomly removing some observations.

- Section 4.3

This more in-depth analysis of specific surges is more interesting, that the previous sections. I only have a few minor comments detailed further down. However, the analysis of two glaciers is not sufficient to claim a regional study as highlighted in the title, abstract and conclusions.

- Section 5 – Discussion

As stated before, section 5.1. should be revised depending on the aims of the present study. The comparison with previous studies only makes sense when providing an exhaustive inventory, otherwise, it should only focus on newly identified surges. In particular, the authors should be really cautious when claiming that their results are better than previous studies.

Section 5.2 is more novel and interesting as, to my knowledge, few studies have analysed surge periodicity for such a large sample of glaciers. It does however belong to the results section. In this subsection in particular, the authors should be cautious about the terminology used. They often mix the terms "surge cycle", "surge period", "surging phase" which makes it difficult to understand the results. They should include a small statement at the beginning to clearly define the various terms and stick to a single term.

Finally, from L 385-397, the authors mix their own results with results from previous studies and it is incredibly difficult to understand what is a new result, and what was already published. I think this would be clearer if this section was split between the results (where the new results are presented) and the discussion (where the comparison with previous studies is discussed).

Section 5.3 is absolutely not related to the present work, it does not discuss any result obtained here and should be removed.

Section 5.4 starts with a rather long discussion on the literature and it is difficult to relate it to the results of the present study, which is only brought up at L 460 (so after 40 lines). Additionally, this second part is very unclear, seems to be a list of contradictory statements and does not bring much of a discussion. Hence, I would recommend to remove it. For example, the authors discuss two types of surges: 1) with a short surging period and 2) with a long surging period, but the meaning of short/long is not defined. Also, because type 2 is exactly the opposite of type 1, it is not very specific. It is like saying, there are two types of people in the world, short people and tall people. I do not see what we learn from that. Finally, the first type includes two situations, the second one being glacier surges that last several years, which is in direct contradiction with the type it belongs to i.e., "surging periods lasts for a short time". Additionally, this paragraph discusses "acceleration and deceleration" (L 464) that have been barely described in the results, apart from two study cases.

- Conclusions

The conclusions need to be updated accordingly to the previous comments.

Technical corrections:

- L 6: The statement about the Karakoram anomaly is unrelated to this work and should be removed.

- L 12: The analysis with river basin should be removed, as discussed in the specific comments.

- L 14: "these surging glaciers advanced at least 2802 times" as discussed, this needs to be clarified. On what time scale are these advances considered? It does not make sense if it is on an irregular temporal sampling…

- L 17: "The mechanism of surging glaciers in HMA is more complex, which is different from Svalbard and Alaska glaciers." This statement is not really backup up by a thorough analysis, so please remove.

- L 20: On the topic of water resources, I believe there are more appropriate references than the Shi & Liu reference, e.g. the IPCC special report on ocean and cryosphere (chapter 2), Huss & Hock (2018) or Immerzeel et al (2020).

- L 21: I don't think the references to Shi & Liu (2000) or Kargel et al. (2014), i.e GLIMS are relevant here.

- L 26-29: you do not need to cite these precise numbers, especially if they are taken from the mentioned references.

- L 32: "also known as exceptional or catastrophic advances, galloping glaciers or pulsatory glaciers". I think terms like exceptional or catastrophic advances would not apply only to surges, so I would remove.

- L 35: "occurs at quasiperiodic multi-year intervals" I would state that many surges have been observed to have a quasiperiodic cycle, but I do not think it should be considered in the definition of a surge (otherwise the vast majority of glaciers in your study and previous studies would not classify as surge-type because we do not have long enough observations to validate a periodic behaviour).

- L 35-38: split this sentence as it is too long.

- L 46: I was unable to find the references Xie et al 2009 (the DOI is invalid) or Liu et al. 2017. Anyway, none of these references seem to focus on glacier surges in HMA, so please remove. A reference to Sevestre and Benn (cited In the previous sentence) is sufficient.

- L 105: “most abundant” I would rather insist on the fact that it is the longest record. Not sure about data volume, as modern fully systematic missions like Sentinel may already have acquired more data than the Landsat archive.

- L 110: “orbits” - replace by “paths and rows”. Can you also please specify wHICH Landsat collection was used (as it influences the number and quality of images).

- equation 1: It is strange to use A for a length, as it is often used for an area.

- L 155: “Glacier terminal”. Replace with “terminus”.

- L 156: I do not understand what you mean by “statistical standard”. Wy would advance be calculated with reference to the maximum length? If so, that would always be a retreat… I thought the advance would be calculated by comparing the length between two images. Please clarify in the methods.

- Equation 2: epsilon is already used in equation 1 for another variable. Please rename.

- Equations 3 and 4. See my earlier comment. These equations highly underestimate uncertainty as they assume full independence of each elevation observation, which is not the case.

- L 171: “MED is the average elevation difference”. This is confusing, “MED” would be used for the median, but you state average. Please clarify.

- L 178: “a maximum advance distance of more than 150 m/a” a distance cannot be expressed in m/a, please rephrase.

- Figure 3: What is the meaning of the inner/outer disks? It seems that some subregions of the Bolch et al. (2019) outlines have been merged, e.g. Northern, central and Eastern Tien Shan. Please explicit which regions are considered. Also refer to the outlines source

in the figure caption.

- L 221: “the annual times” does not mean anything. I think you mean “occurrences”?

- L 228: “the higher frequency of glacier surge found in July, August, September, and October is strongly correlated to the quality of the images which are better in the vast majority of HMA in these four months”. Exactly, so you need to take into account this sampling bias in your analysis, otherwise it does not make any sense…

- Figure 7: “termious” -> “terminus”. You need to divide the distance/area by the time span between your images since it is not constant.

- Figure 8 and 10: the “jet” colormap has been repeatedly criticized because it leads to misinterpretation of the data, plus it is not appropriate for colour-blind people. Please use a more appropriate colormap. See for example https://doi.org/10.1371/journal.pone.0199239.

- L 294: “the two glaciers had different degrees of thinning and thickening before and after the surge (Fig. 10).” You only show thinning/thickening during the surge. Your statement would imply that you have a dh map before and after the surge. Please clarify.

- L 295: “The average thinning of the accumulation zone”. You need to explain how you delineated the accumulation and receiving zones. You could for example show the contours on your different figures.

- L 301: “streamflow” I believe you mean “ice flow” or “ice velocity”.

- L 303: “while the end velocity of branch glacier A2 has increased since 2006”. What about the high speed before 2001? Is this a real signal? Why don’t you discuss it?

- L 304: Here and at many other places, you use the term “the end” which is ambiguous. Please use the term “terminus” consistently throughout the text to make it easier to understand.

- L 301-307: The description of the velocities is very qualitative, but it does not bring a lot of value compared to the figures.

- Table 2: You should clarify the studies that aims at a complete inventory (e.g. Guillet et al, 2022, RGI, Sevestre and Benn (2015)….) and those that only analyse a subset within a given region (e.g. Vale et al. (2021)). Please also add a line to separate the different studies. Because the column "data source" extends over several lines, it is difficult to find to which study it is linked.

- L 352: "leading to large errors". This claim calls for more details. If there are limitations with this previous study, please describe them more clearly.

- L 366: "Meanwhile, glacier surface elevation and flow velocity were used as supporting evidence for surging glacier identification, which made our results more accurate." According to the methodology, velocity was not used in the identification. Plus, you did not consider any glacier that did not display a terminus advance of 150 m/a or more, so you cannot state that your results are more accurate. At best, they are complementary. Please remove that sentence.

- L 369: The Xie and Liu (2010) reference seem to be a general class book on glaciology not specifically focused on surges. You can certainly find a more appropriate reference here…

- L 381: Before you discuss the duration of a surging phase, you need to explain how you define it. Is it during the entire advance period? Above which threshold is an advance considered?

- L 382: "Most glaciers in the Pamirs have a surge cycle of 9-20 years" Why most? If not all, why some are excluded here?

- L 383: "surge period" the term can be confused with "surging phase". I recommend using "surge cycle" and first of all to define the terminology at the beginning of the section.

- L 384: "that of the Bivachny Glacier is approximately 15-20 years (Xie and Liu, 2010; Zhang et al., 2018)." Are these numbers not your results? This is confusing as you are mixing with your own results. Please restructure.

- L 384: "The surge phase of glaciers in the Karakoram region is generally short, ranging from several months to five years." It is roughly the same as for Pamir, so why state it is short?

- L 386: “The surging glaciers in the Kunlun Mountains have surge periods ranging

from 1-24 years”. On figure 12, I see short surge phases, but not 1 year surge cycles. Clarify.

I believe you mean surge phase, not surge periods.

- section 5.3.: Although I recommend to remove the whole section, here are two comments: L 402-404 the temperature trends are quite different from the ones quoted in the introduction. It is weird to have different values and not explain why. L 414-417 these statements would need to be backed up with references and detailed more.

- L 463: “the surge lasts for several years and occurs in every month” I do not understand where this statement comes from. You do not discuss the timing of the surges in the paper, only that of advances. A surge could be consistently initiated at a specific time of year and continue long enough that advances happen all year round.

- L 477: “covering an area of 10974.25 km2, accounting for 11.25% of the total area of glaciers in HMA” See my concern about these numbers being overestimated if considering glacier complexes. If keeping this number, the possible overestimation should be at least mentioned here.

- L 481: This whole statement needs to be revised with a proper analysis of the advances that taken into account differences in sampling.

- L 486: You should mention that the surge cycle has been estimated for a subset of 36 glaciers.

- L 490: “The mechanism of surging glaciers in HMA is more complex, having characteristics that are not exactly the same as those of Svalbard and Alaska surging glaciers.” Remove as it is not sufficiently supported by your results.

- Data availability: I recommend the data to be made available publicly (with a possible embargo period if needed).

Additional references:

- Farr, T.G., Rosen, P.A., Caro, E., Crippen, R., Duren, R., Hensley, S., Kobrick, M., Paller, M., Rodriguez, E., Roth, L., Seal, D., Shaffer, S., Shimada, J., Umland, J., Werner, M., Oskin, M., Burbank, D., Alsdorf, D., 2007. The Shuttle Radar Topography Mission. Reviews of Geophysics 45. https://doi.org/10.1029/2005RG000183
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