Reply to Dr. Andres Rivera (https://doi.org/10.5194/egusphere-2024-2662-RC1)

General comment:

> *This is a short, interesting and well written contribution about ice flow velocities of small glaciers in the tropical Andes of southern Peru and northern Bolivia. The authors have processed a great number of Landsat and Sentinel images from 2013 to 2022 following procedures and methods already applied to bigger glaciers and ice caps elsewhere. The main assets of this manuscript is the positive application of this methodology for detecting velocities of small glaciers in a region with limited data. They were able to detect very slow motions in regions with steep slopes and limited cloud free conditions. They have also detected some seasonal variations without clear inter annual trends. Even if the method is not novel, the main contribution is its application in small glaciers. It will be very nice if an outcome of this paper is building a freely available database with the resulting velocities in the region.*

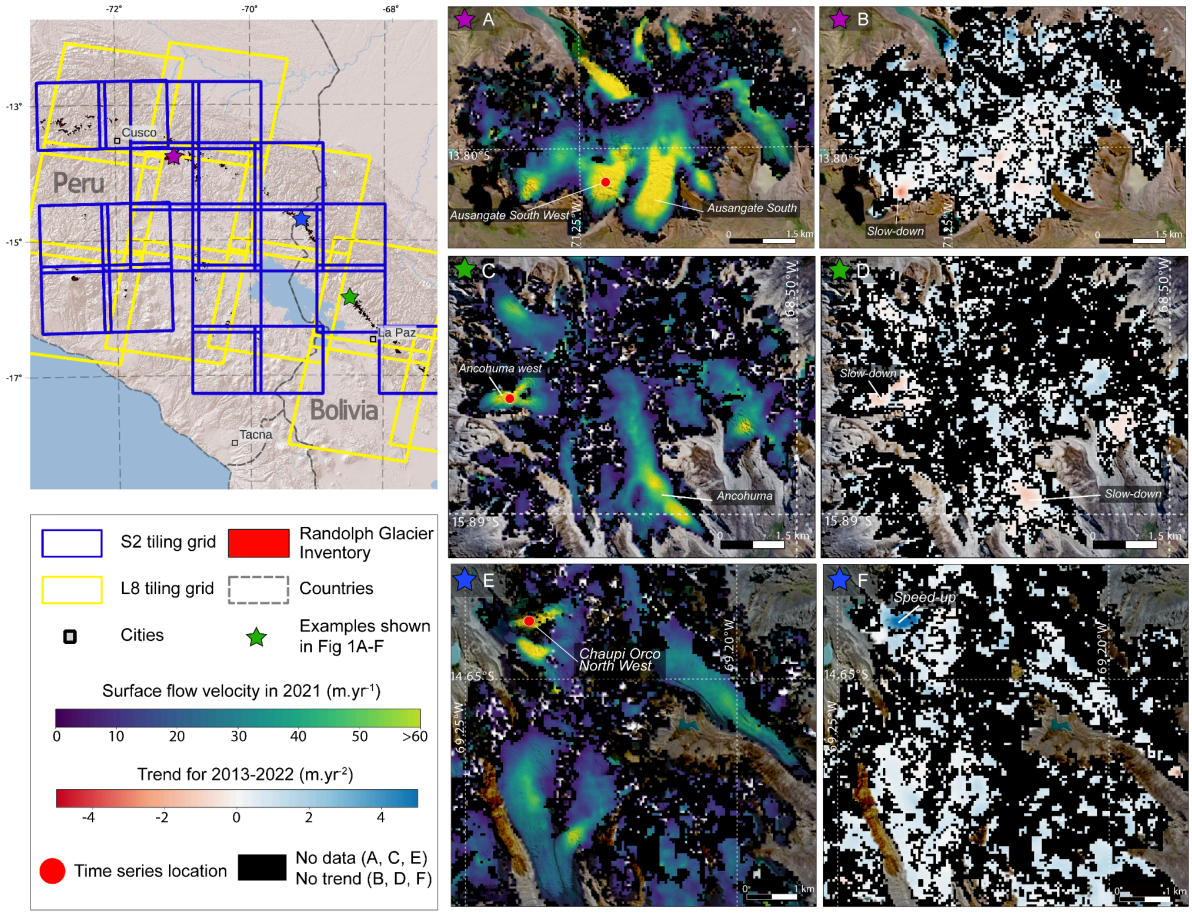
Authors: We would like to thank Dr. Andres Rivera for his insightful comments which allowed us to improve the manuscript.

Specific comment #1:

> *I'm having few comments and suggestions aiming to improve the text. My main concern is related to the quality of the figures, especially Nº1 that requires some improvements. For somebody not very familiar with the study area, it is difficult to locate Figure 1 a, c and e. There are very small points with colors at main Figure 1 that I presume are the insets locations. Maybe adding a bigger symbol for each inset box to Figure 1 could help. Each box must have coordinates. The resolution of the boxes is quite low and very limited details could be seen. Maybe having a box of only the main glaciers and not for the whole mountain center? The time series locations are not visible and I struggled to sea each start¡¡ The Figures b, d and e are extremely noisy and looks like there is no trend at all¡¡ By the way, in the text it is used m/yr^2 and in the figure says m. yr^{-2}. I suggest using only one form in the whole text. In Figure 1 (main) it is mentioned the Randolph glacier inventory (RGI), but nothing is said in the text. I suggest adding a reference and a phrase about the RGI used polygons and why not using the national inventory. I presume Ames's inventory from 1989 is outdated but you can have a look and compare your outlines with the Peru's national glacier inventory available at:*
>
> *https://sinia.minam.gob.pe/documentos/inventario-nacional-glaciares-lagunas-origen-glaciar-2023*

Authors: We have improved the resolution of Figure 1 according to Reviewer #1 and #2 comments. We have chosen to show the location map, which provides an insight into the exact boxes locations (now stars) and also added coordinates on all subfigures. Concerning the image quality, the first draft was submitted with a compressed version of the Figure, we now provide Fig. 1 in full resolution which should solve the issue of image quality and details. We have also zoomed on the specific glacier locations, with a Sentinel-2 mosaic background, which also contributes in enhancing the figure quality. We have worked toward making this map clearer, first by increasing the size of symbols for boxes and changing them to "stars", as these are more clearly visible (also added inside the legend and on the subplots A-F). In Figure b, d and e we have adjusted the colorbar to make trends more visible (when they are significant, i.e. not in black). It is however difficult since the glaciers' speed has been in general fairly stable throughout the entire mountain range. We still think it is important to keep as black, regions where trends are not statistically significant with *respect to the error in ice velocity, in order to not mislead readers and future product users. You can find the modified figure below (other changes have been made thanks to reviewer #2):*

We also added in the section 2.1. of the Methods, that the RGI_V6 is used as a reference for the glacier outlines. Thank you for pointing this out.



Specific comment #2:

> *Regarding the seasonal comparison shown in Figure 2 is very interesting especially for such small glaciers. Some comments: The name of each analyzed glacier is visible in figure 1, but the numbers in figure 2 are a mean for the ablation zone? or are just one spot on the glacier? Increasing the size of Figure 1a, b and C can help also for identifying the location of your series in Figure 2.*

Authors: It is in fact the velocity time series at the location of the circle on figure 1A,C,E. The average velocity of a 3x3 pixels window is considered at this location. We have now increased the size of the circle on Figure 1, along with more zooming on the time series location. We have clarified the legend of Figure 2 accordingly.

Specific comment #3:

> *As a general approach to the problem, I'm missing a brief but informative discussion of the literature available about glacier changes in the region, the relationship with ENSO and some trends justifying the analysis that has been done. How the resulting ice*

> *velocities are improving our understanding of ongoing and forecasted glacier behavior in the region? This is a brief communication, so not space for a detailed overview, but a phrase or two about this will set up the context.*

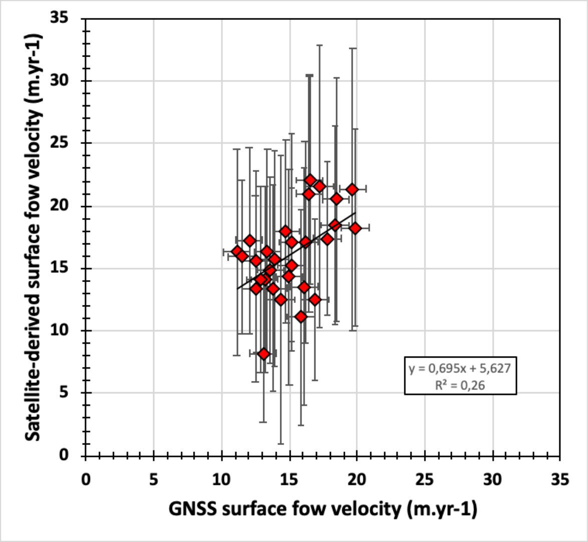
Authors: Following the reviewer comment, we added a short paragraph in the Introduction regarding the overall context related to glaciological studies in the tropical Andes and the interest to focus on glacier dynamics. You can know read:

"In the tropical Andes, many studies have been dedicated to surface-area or volume changes over the last decades-to-centuries (e.g., Hastenrath and Ames, 1995; Rabatel et al., 2005, 2013; Basantes-Serrano et al., 2022) and to surface mass balance processes and their relationships with climate conditions and particularly the ENSO (e.g., Kaser, 2001; Francou et al., 2003; Rabatel et al., 2013), but the spatio-temporal variability of glacier flow in the tropical region has never been explored in details. This leaves a significant knowledge gap in our understanding of glacier dynamics and their response to changes in surface processes, thermal regime or subglacial hydrology under tropical conditions, with direct consequences on ice flow modeling capacities."

Specific comment #4:

> *How the obtained results are compared to field and remotely sensed data? There is one mention to GNSS data at Zongo, but nothing is said about the compared values. There is an agreement with some results in the Alps, but there is nothing else in the Andes? There are more works in the region, not based on GNSS, but using remotely sensed imagery that can be compared. See for example Kos et al 2021 https://doi.org/10.3390/rs13142694.*

Authors: The comparison with the Zongo d-GNSS measurements are mentioned in section 3.1 of the results. It is worth noting that the GNSS measurements are conducted on the ablation stake network located in the ablation zone which consists of a gentle slope tongue with glacier surface flow velocities ranging between 5 and 25 m.yr^{-1}. Such low flow velocities are reaching the limits of the satellite-derived flow velocity detection. Hence, the comparison with these data is limited. This is the reason why we decided to mention a more thorough comparison, using the same method, and same satellite data over the Alps, which provides a better level of understanding of uncertainties related to satellite-derived surface flow velocity. We have clarified the sentence in that sense in section 3.1. We also decided to show the scatterplot of the comparison between satellite-derived and in situ GNSS surface flow velocity on Zongo glacier in the Supplementary Material as follow:



Concerning the work of Kos et al., 2021, their study focuses on the Cordillera Blanca, which was used as an example in Millan et al. 2019 but not considered in the current study. However, Kos et al. have compared their mapping with Millan et al., 2019 showing good consistencies with the ice velocity mapping: "Daily surface velocities averaged over 3.2 years ranged between 0.01 and 0.47 m/day, where both their spatial distribution and magnitude are consistent with results obtained from Sentinel-2 data".

Specific comment #5:

> *Two of the co-authors published a nice compilation of ice velocities in Cordillera Blanca (Millan et al., 2019), but it seems to me there are no overlapping areas with this study for comparison purposes. In this sense, this brief communication is not about the whole tropical Andes of Peru and Bolivia, but from a limited region of Southern Peru and Northern Bolivia. Maybe this could be said from the early beginning to avoid misunderstandings regarding the extension of the study area.*
>
> *There are previous works in Peru and Bolivia about velocities of debris-covered glaciers (e.g. Hubbard and Clemmens, 2008 https://doi.org/10.3189/002214308785837057). The measured values are extremely low but similar to your minimum velocities. Maybe it is worth mentioning these efforts and how your method could be applied (or not) to these glacier types.*

Authors:

You are right, the current study does not cover the entire tropical Andes. We now state it clearly in the Abstract as follows: "We present the first analysis of glacier dynamics in some mountain ranges of the tropical Andes of southern Peru and Bolivia using satellite data from 2013 to 2022." And also at the end of the Introduction as follows: "Therefore, we propose to reconstruct and analyse, the evolution of the dynamics of glaciers located in tropical mountain ranges in the Andes of southern Peru and Bolivia from the years 2013 to the present, building on previous mapping from Millan et al. (2019, 2022)".

Regarding the debris-covered glaciers. Very few glaciers have a debris cover in the cordilleras we focus on in our study. Other places in the Tropical Andes, like the Cordillera Blanca (where the study by Hubbard and Clemmens was conducted) have much more debris-covered glaciers. On such glaciers, although the velocities are low, the nature of the surface (i.e. the presence of debris) is well adapted to the correlation algorithm and because of the slow movements, image pairs with long temporal baselines (e.g., 1 year or more) can be used. In addition, changes in contrast (due for example to changes between snow and ice surface states) are not an issue on the debris-covered part. Therefore, an application on such glaciers should be deserved. We added this point at the end of the discussion, you can now read:

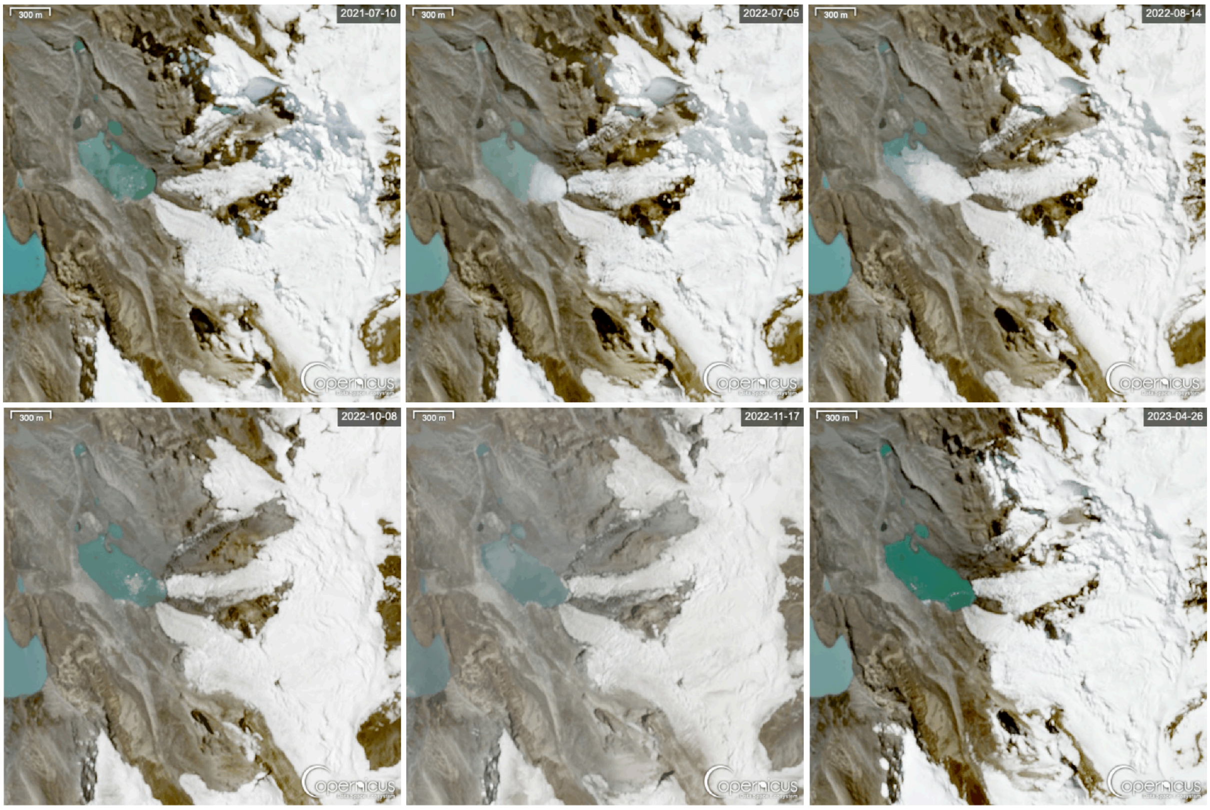
"Finally, it should be noted that glaciers in the mountain ranges studied here do not present a debris cover. In other cordilleras of the tropical Andes, such as Peru's Cordillera Blanca, debris-covered glaciers are more numerous, and in situ velocity measurements have been made on some of them (e.g., Hubbard and Clemmens, 2008). Although these debris-covered glacier tongues have low velocities, generally less than a few tens of metres per year, their surface texture (linked to the debris) and temporal persistence mean that correlation algorithms using long temporal baselines (e.g., a year or more) can allow to retrieve consistent velocity values, as shown by Cusicanqui et al. (2024) using Landsat data on rock glaciers in Chile."

Specific comment #6:

> *Regarding Chaupi Orco North West: A possible surging event is not totally absent in the tropics as mentioned by Basantes-Serrano et al., (2022), but they indicated that one possibility explaining this process is subglacial geothermal heating increase as the glacier is on top of a at Antizana volcano (even if has no activity in the last 4 centuries). Is Chaupi on a volcano? Could it be related to geothermal activity increase at the glacier bed?*
>
> *The velocity changes at Chaupi during the possible surge event is in the order of 100 m from a mean of 60-70 m/yr (roughly speaking) between 2013-2021 to a maximum of 165*

m/yr in May 2022. If you see the seasonal variations in the other 3 glaciers, there is a gap (explained in the text due to the lack of images) where there are jumps of near 90-100 m/yr between August/November and March/April. Could it be possible that the limited data at Chaupi is precluding to see the seasonal "jump" and looks like there is a sudden increase in 2021-2022?

Authors: Thanks for the comment. It could be a possibility. However, as you can note on Figure 2, the surface velocities at Chaupi Orco are increasing during the dry period in 2021 (between April-May and October 2021), when a lot of observations are available giving confidence on this increase. At this period of the year, the velocity of the other glaciers is, on the contrary, seasonally decreasing. Therefore, the behavior of Chaupi Orco for this period is definitely unusual. In addition, this glacier is a lake terminating glacier and looking at the Sentinel-2 images, one can note an unusual presence of icebergs at the lake surface during the "surge-type" event, which goes hand-to-hand with an increase in ice flux as illustrated below. We have added a new figure in the supplementary material (Figure S2) with images before, during and after the event as follow:



Specific comment #7:

In synthesis I think this is a valuable contribution that deserves to be published.

Authors: Again we would like to thank Andres Rivera for contributing to the manuscript improvement.

Reply to Whyjay Zheng (https://doi.org/10.5194/egusphere-2024-2662-RC2)

General comment:

I am thankful that the authors took a step into the ice flow velocity measurements using the optical feature tracking technique for the tropical glaciers in the Andes. As the authors stated in the preprint, this region suffers from suboptimal cloud conditions for the optical images to be useful. The authors ventured into this challenge and presented the aggregated annual velocity map for many small glaciers, some even with seasonal time series. Despite having a non-trivial number of pixel voids (e.g., Figure 1c), I still think the workflow presented here is state-of-the-art.

This work is concisely well written, and the results are worth sharing with the glaciology community. I have listed my specific comments below to hopefully improve the communication between the authors and future readers.

Authors reply:

We would like to thank Dr. Whyjay Zheng for his insightful comments which allowed us to improve the manuscript.

Major comment #1:

Justification of the processing methods. The processing methods used in this work are basically the same as the three papers mentioned in L38. Therefore, the authors skipped many details when they described their workflow. I checked the three references, and it seems that methods, including the feature tracking parameters and annual map extraction, are fine-tuned for the Alps. Millan et al.'s 2019 paper uses a Peruvian region for one of the test cases, but it does not quite convince me that the parameter set is good enough for the tropical Andes glaciers because the derived velocity map does not have the same quality as the Alps glaciers in the same paper. In addition, highly imbalanced data availability during monsoon and non-monsoon seasons may also affect the robustness of the linear regression for the annual map and lead to extra uncertainty. This is also different from the case in the Alps. Could you explain why you think the same workflow applies to the tropical Andes, if there are reasons other than convenience? I think it is worth sharing any thoughts under the hood even if the authors do not aim for the best tracking parameters and processing workflow (cf. to what is suggested in Millan et al.'s paper, "The size of the sub-images may also be sub-optimal for correlation, but seems more appropriate…"). Lastly, for your future reference, I'd like to share my team's recent work with the authors (https://doi.org/10.5194/tc-17-4063-2023), which aims to help the fine-tuning process of a feature-tracking workflow.

Authors: We don't know what the reviewer means by "not the same quality" when he refers to the results obtained in Millan et al. (2019) on the glaciers in the Alps and those illustrated in the Peruvian Cordillera Blanca. However, it must be noted that all the parameters of the correlation process in the workflow, i.e. window size, searching distance and sampling step were not optimized on the basis of the glaciers in the Alps only, but rather defined by simultaneous tests on a wide variety of glaciers in the Alps, the Andes and in New Zealand. Specifically, the case of the Mont-Blanc area shows that we are both able to map large and fast glaciers, along with glaciers that are even smaller than in the Andes and on steeper terrains (e.g., Planpincieux glacier), and were validated with a larger network of GNSS measurements which strengthens our analysis. This was clarified in the Introduction (section 1) and in sub-section 3.1. of the Results (see also our reply to a comment from reviewer 1).

We also thank the reviewer for the reference recommendation. It could be interesting to test the GLAFT package in the future, but the ground truth data are likely too limited (in number and in surface flow velocity representativeness) to obtain interesting results.

Major comment #2:

> *Time series. Section 2.2 is titled "Time series extraction." I see multiple annual maps are a type of time series, but with Figure 2, I was a bit misled in the beginning and was excited to search for the seasonal/monthly signals for all glaciers until I realized the time series with a high temporal sampling rate, as in Figure 2, is only for a few glaciers. With the current text structure, I would change the title of section 2.2 to "Annual maps production and selected seasonal signal extraction" or something similar to reflect the presented results. Also, I am curious to know if the authors have a map showing where (or for what pixels) the LOWESS method for the seasonal signal is applicable. This information may be valuable to the community.*

Authors: We have changed the title of the section 2.2 by "2.2. Annual maps production and seasonal signal extraction" and the one of Figure 2, which now reads as: "Time series of surface flow velocities for selected glaciers shown in Figure 1…".

We have not produced a map to show the pixels where it is possible to use the LOWESS method. To produce such a map, we would likely have to run the LOWESS method everywhere to see if it gives results or not, which would take a huge amount of computational resources. For this work we decided to look at the time series only on the glaciers with the highest velocities to be sure that we were looking at seasonal trends and not correlation errors (median velocities > 60 m/yr), which corresponds to the precision error of the smallest cycle of 5 days (Millan et al., 2019). This choice limits the number of observable glaciers.

Finally, it must be noted that we even have hesitated to remove the LOWESS fit from Figure 2 since the scarcity of the data during the monsoon season makes it highly difficult to perform.

Minor comment #1:

> *I do not see a Code availability and Data availability section in the preprint. For this work, I think it is essential to provide readers with guidelines about how to get the new data or at least how to reproduce the results.*

Authors: Thanks for this comment. Yes, our plan is to share the data and code for the data analysis. We have now added a section "Data and code availability". For that we will use the website of the French National Glacier Monitoring Service called GLACIOCLIM, where data of other papers are already shared.

Refer to: https://glacioclim.osug.fr/-Acces-a-des-donnees-elaborees- .

Minor comment #2:

> *For the colormap used for surface flow velocity (Figure 1), please ensure it adheres best to the TC suggestions described here:*
> *https://www.the-cryosphere.net/submission.html#figurestables.*
> *I recommend a perceptually uniform colormap.*

Authors: We have now changed the surface flow velocity colormaps to a colorblind-proof and perceptually uniform colormap.

Minor comment #3:

> *Supplementary figures S1 and S2 lack key information to help readers understand what is presented, such as the satellite/instrument names and scale. Did the authors review both descending and ascending tracking for both locations? Is the loss of coherence caused by the terrain effect or the climatic conditions?*

Authors: We have updated the captions of these figures (now S3 and S4) to provide a more detailed description of the Sentinel-1 SAR interferometry measurements to the reader. Indeed, both ascending and descending satellite tracks were investigated. It is difficult to confidently state the predominant cause of coherence loss, however we find that temporal decorrelation due to precipitation and surface melt/refreezing events to be the most likely causes (exacerbated by the fact that almost no 6-day image pairs exist). Additionally, as the Reviewer suggests, some areas are also affected by layover/shadowing artifacts due to steep terrain, hindering reliable velocity retrievals. Note that we have included a different interferogram in Figure S3a, to present another partially coherent retrieval (from a descending track over Ancohuma).

The Figure captions now read:

Figure S3: Examples of Sentinel-1 Synthetic Aperture Radar (SAR) interferograms constructed over: (a) the Ancohuma region in Bolivia (ascending track #76, images from March 13 and 25 2021); and (b) the Ausangate region in Peru (descending track #127, images from December 11 and 23 2020). We processed all available Sentinel-1 Interferometric Wide swath images from four different tracks (6 to 12 days apart), covering the various areas of interest with both ascending and descending tracks over the three years 2019-2021. Generally, interferometric coherence is completely lost over all areas of interest whatever the considering time windows and whatever the period of the year (with very few exceptions, see Figure S4). This temporal decorrelation is likely due to heavy precipitation and surface melt/refreezing events, amplified by the fact that very few 6-day image pairs are available in the region. Additionally, some areas are affected by radar layover/shadowing effects, due to the steep mountainous terrain.

Figure S4: Examples of Sentinel-1 Synthetic Aperture Radar (SAR) interferograms constructed over: (a) the Ancohuma region in Bolivia (descending track #54, images from June 16 and 28 2021); and (b) the Ausangate region in Peru (descending track #127, images from July 28 and August 1 2019). These examples are among the most coherent retrievals of the entire processed image archive, and show partially coherent phase values over small parts of the areas of interest. This coverage, however, did not allow us to extract meaningful velocity estimates.

Minor comment #4:

> *L60: Is it possible to specify what trend maps use a Gaussian filter and what others use a median filter?*

Authors: Thanks for this comment. Trend maps were filtered using a 5x5 median filter, and not a combination of gaussian and median filter as mentioned previously. The text has been adapted accordingly.

Minor comment #5:

> *L75-80: I do not quite understand what the authors want to achieve by comparing the Bolivia case with the Alps case. Zongo Glacier has a higher coefficient of variation (σ/μ ~1) than Rabatel et al.'s Alps test (σ/μ ~0.1), which makes the argument that "D-GNSS measurement falls within the level of error" much less powerful.*

Authors: d-GNSS measurements in the Zongo glacier are made in areas where the surface flow velocity are ranging between 5-25 m/yr, which is within the precision range of the satellite derived surface flow velocities (Millan et al., 2019). Therefore, we chose to supplement the validation of our method (which was the same used over the Alps) with the example of the Mont-Blanc area. Indeed, glaciers in this area provide a much larger network of GNSS measurements, in regions where glacier speed is higher, and better reflect the typical "high speed" observed in the Tropical Andes (~50-150 m/yr). We have modified the text accordingly (also with respect to a comment from reviewer #1) at L80-86.
In addition, because this comment is also in line with one of the Reviewer #1, we now shortly elaborate in the text on the Zongo Glacier in situ data, and present the scatterplot of the comparison of in situ and satellite-derived velocities in a new Figure S1.

Minor comment #6:

> *Figure 1: Panel D (and maybe B and F as well) contains lots of non-trend pixels, which block the view of the trend color. At this figure resolution, I can barely see if there is any area with color other than yellow (0).*

Authors: Everything was light yellow because no significant trends were detected over most of the area during the entire study period, and this is well illustrated with the largely dominating yellow color. When small trends can be observed, it remains difficult to see because of the little glacier size. Therefore, we have added inside notes where small trends can be observed on Figure 1B,D,F, ("Slow-down" and "Speed-up") and we made zoom (which was also requested by the Reviewer #1).

Minor comment #7:

> *The authors mentioned the glacier flow response to climate change at least twice in the manuscript (L16-17 & L140-141). Do we have any in the results? Does the surge signal count? It might be worth adding a sentence or two to address these questions.*

Authors reply: This mention was a rather general statement and we have no argument in this study to dig into this direction. We modified the sentence to be more precise and in line with the arguments mentioned in the discussion to relate the seasonal changes and the surface melt. You can now read in the Introduction: "dynamics and their response to changes in surface processes, thermal regime or subglacial hydrology"; and the Discussion "for understanding their dynamical response to changes in surface melt and subglacial hydrology".
The surge signal is most likely not a sign of climate change.