Response to comments by second reviewer – Anonymous Referee

We sincerely thank the Anonymous referee for the thoughtful comments and constructive suggestions, which have greatly contributed to improving the clarity and overall quality of our manuscript. Below, we provide a point-by-point response to each comment.

General comments

GC-1. Cusicanqui et al. estimate rock glacier kinematics on annual to decadal time scales from medium-resolution Landsat imagery. Assessing the applicability of Landsat imagery for this purpose is important because Landsat images are more widely available than higher-resolution images, while the lower spatial resolution of 15 m (panchromatic) raises questions about the suitability for measuring rock glacier kinematics on subdecadal time scales. To appraise the applicability, the authors compare the Landsat-derived motion estimates to independent observations derived from GNSS and high-resolution images, as well as to an InSAR inventory. The study raises and addresses a question of substantial interest to readers of The Cryosphere. Its novelty lies in its being the first to use Landsat imagery for estimating rock glacier kinematics. The methods and data interpretation are, for the most part, sound, and the results are valuable to the community.

Response > We thank the reviewer for the positive comments about our study.

GC-2. While the manuscript clearly advances the field, I have concerns about the extent to which the presented data support the authors' conclusions about the observational uncertainty. Furthermore, I have found the manuscript difficult to follow because of inconsistencies in content and terminology and, at times, a writing style I consider to be verbose and vague. As I agree with Jan Henrik Blöthe's three general comments, I will not comment further on these aspects, focusing instead on my concerns about the uncertainty analysis and the presentation.
Uncertainty: My main content-related concern pertains to the accuracy assessment. There is currently no figure or table that shows aggregated accuracy metrics, making it difficult for the reader to appraise the evidence for claims found in the abstract and conclusion. I believe it would be helpful to reorganize the accuracy assessment, introducing relevant equations in the methods and presenting the estimates in the results (including a figure with metrics such as the root mean square deviation with respect to independent estimates, the NMAD over stable areas, or the estimated deviation between temporal changes). In the abstract and conclusions, these specific metrics can be reported, while clearly distinguishing observations from subjective interpretation.

Response > We thank the reviewer for highlighting this point. Regarding the uncertainty, as suggested by the reviewer, we have reorganized the manuscript by adding a specific section showing the reported uncertainties; in this Section 5.4, we have now added a new figure showing the variation in time of NMAD on both components EW and NS, as well as Table showing all the statistics for both L7/8 and VHR datasets, on stable and mobile areas (i.e. differences with GNSS observations). Now you can read:

[...]

5.4 Reported uncertainties

The horizontal accuracy assessment at annual and selected periods in this study is summarized in Table 2. For the L7/8 dataset, the average NMAD of surface displacement over stable areas obtained is 1.8 m in EW and NS components. Uncertainties are greater in recent years due to the cumulative error of time series (Table 2). The average median value over stable areas corresponds roughly to 1/10 of L7/8 pixel size. For annual surface velocities, the average NMAD is 1.18 m a^{-1}. Applying this NMAD value as a threshold to filter statistically representative rock glaciers velocities (Table 2), identified only nine PMAs with average velocity above 1.2 m a^{-1} (Fig. 8). As expected, annual uncertainties are too high to reliably detect statistically significant changes in velocity at annual scale. At decadal time spans, uncertainties decrease significantly (Table 2). The NMAD is 0.11 and 0.13 m a^{-1}, for 2000-2014 and 2013-2024 periods, respectively. Again, applying the average NMAD value for both periods as a filter of PMAs, 150 PMAs are above this threshold, being good candidates to depict velocity changes. Finally, as the 'Top 50 average velocity' of all PMA is 0.3 m a^{-1}, at decadal scale and uncertainties encompasses 43% of the overall average velocity.

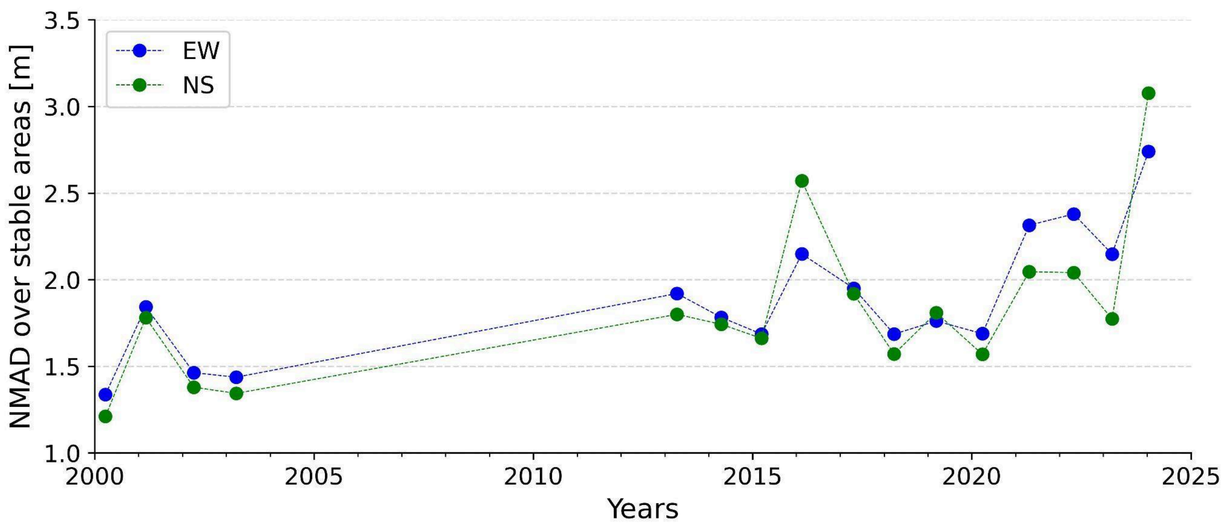

Figure 8. Annual NMAD values for the east-west (EW) and north-south (NS) components over stable areas for the L7/8 dataset. Figure S4 presents the stable area map of the study area used for NMAD computing.

Table 2. Accuracy assessment of surface displacement and surface velocity maps at annual and decadal time span. Spatial statistics were computed over a stable area of 53% for L7/8 (n pix = 4 810045), 55 % (n pix = 10 593 874) and 47% (n pix = 3 522 115) for Tapado complex and Largo rock glacier VHR dataset. [(a)] Values between brackets represent the range (min and max) values over a stable area for each component. [(b)] Difference velocity between GNSS and pseudo-GCPs vs surface velocity fields, computed using the same time period. [(c)] VHR dataset was splitted in two sub periods trying to fit the same time span as for the L7/8 dataset.

		STABLE AREAS								MOVING AREAS	
L7/8 dataset		Annual surface displacement [m] [a]		Decadal velocity $[m\ a^{-1}]$				24-year velocity $[m\ a^{-1}]$		Difference in velocity $[m\ a^{-1}]$ [b]	
				2000-2014		2013-2024		2000-2024		2010-2022	
		E-W	N-S	E-W	N-S	E-W	N-S	E-W	N-S	Tapado	Largo
	Mean	[-0.16, 0.45]	[-0.70, 0.20]	-0.009	-0.009	0.032	-0.021	0.004	-0.008	0.183	1.359
	Median	[-0.36, 0.32]	[-0.82, 0.34]	-0.006	-0.014	0.017	-0.023	0.004	-0.015	0.157	1.224
	Std	[2.25, 5.93]	[2.37, 6.03]	0.275	0.298	0.255	0.283	0.136	0.141	0.236	0.837
	Nmad	[1.33, 2.74]	[1.21, 3.07]	0.150	0.148	0.148	0.120	0.093	0.084	0.240	1.001
VHR dataset		Multi annual surface displacement [m] [a]		Decadal velocity $[m\ a^{-1}]$ [c]				20-year velocity $[m\ a^{-1}]$		20-year velocity $[m\ a^{-1}]$	
				2000-2014		2012-2020		2000-2020		2010-2022	
		E-W	N-S	E-W	N-S	E-W	N-S	E-W	N-S	Tapado	Largo
	Mean	[-0.05, 0.11]	[0.19, 0.06]	0.010	-0.012	-0.002	-0.006	0.005	0.002	0.011	0.377
	Median	[-0.23, 0.0]	[-0.23, 0.06]	-0.011	-0.020	-0.002	0.000	-0.012	-0.012	0.006	0.206
	Std	[0.35, 1.34]	[0.31, 1.16]	0.120	0.097	0.049	0.054	0.078	0.065	0.047	0.307
	Nmad	[0.11, 0.36]	[0.28, 1.00]	0.030	0.078	0.030	0.010	0.020	0.048	0.036	0.133

[...]

Regarding the relevant equations as NMAD, as this is a classic statistical metric for uncertainty quantification in geosciences, we believe that adding the equation to the manuscript would not add any useful information. However, to ensure the reader well understands the uncertainties of the L7/8 dataset, we rearrange some text from “Section 5.2 Regional distribution of surface kinematics” to the new “Section 4.4. Average Spatial velocity and relative velocity change”.Within this new section, we added the specific study, related to the NMAD quantification. Now you can read:

[...]

4.4 Average spatial velocity and relative velocity changes

Average pixel-based spatial velocity fields were estimated using a linear fit using the cumulative surface displacements maps, allowing to produce coherent velocity maps for a selected period for the entire study area. Then, the representative surface velocity was extracted for each corroborated PMA. The most common approach to obtain average representative surface velocity values is to use the most active portion, often situated in proximity to the central profile (RGIK, 2023). This avoids the potential for lateral variability within the landform (Fig. 3). For instance, Kääb et al., (2021) employed a small area on the most active sectors to express the representative velocity for the entire rock glacier. Nevertheless, the selection of this ‘active’ area remains somewhat subjective and may vary between users. In other respects, Blöthe et al., (2020) proposed the selection of pixels at the 95th percentile above the limit of detection (LoD) to remove the lateral effects. As shown in Fig. 3a to d, the pixels located in the borders often have values close to 0 $m\ a^{-1}$, due mainly to natural geomorphological causes (i.e. increased friction and low/no ice content in lateral margins) as well as to window sizes of feature-tracking algorithms. So, the boundary effect for each PMA can bias the average velocity. To mitigate this *bias, we propose a similar metric than Blöthe et al., (2020) to keep only the Top 50% pixels within each PMA (hereafter referred to as Top 50% average velocity) to represent the average spatial velocity for each PMA. Refer to Section 6.3 for a more detailed discussion.*

Uncertainties of surface displacement and velocity fields were computed using the ***Normalised Mean Absolute Deviation (NMAD; Höhle and Höhle, 2009)*** *over stable areas. Stable areas were defined using TanDEM-X DEM and slopes lower than 35°, without taking into account neither glacier outlines with a buffer of 500 m for each glacier (RGI Consortium, 2017) nor all PMAs, also not corroborated ones produced in this study (Fig. S4). In this sense, stable areas correspond to 53% of the entire study area i.e. 45x45 km² (Fig. S4).*

In this study, relative velocity changes are considered and can be calculated using Equation 1, by using the first period as the reference. The related uncertainties of the relative velocity change can be calculated using Equation 2, assuming that the NMAD for both periods are not so different (σV). Finally, from Eq. 1 and Eq. 2 we estimate a pixel-based relative velocity change and their related uncertainty, for each PMA.

$$V_{change} = \frac{V_2 - V_1}{V_1} \tag{1}$$

$$\sigma V_{change} = \left(\frac{V_2 + V_1}{V_1^2}\right) * \sigma V \,, \tag{2}$$

[...]

Finally, as suggested by the Reviewer #1, we added the error bars on the Figure 5 to show the influence of the uncertainties on the time series. Now you can see:

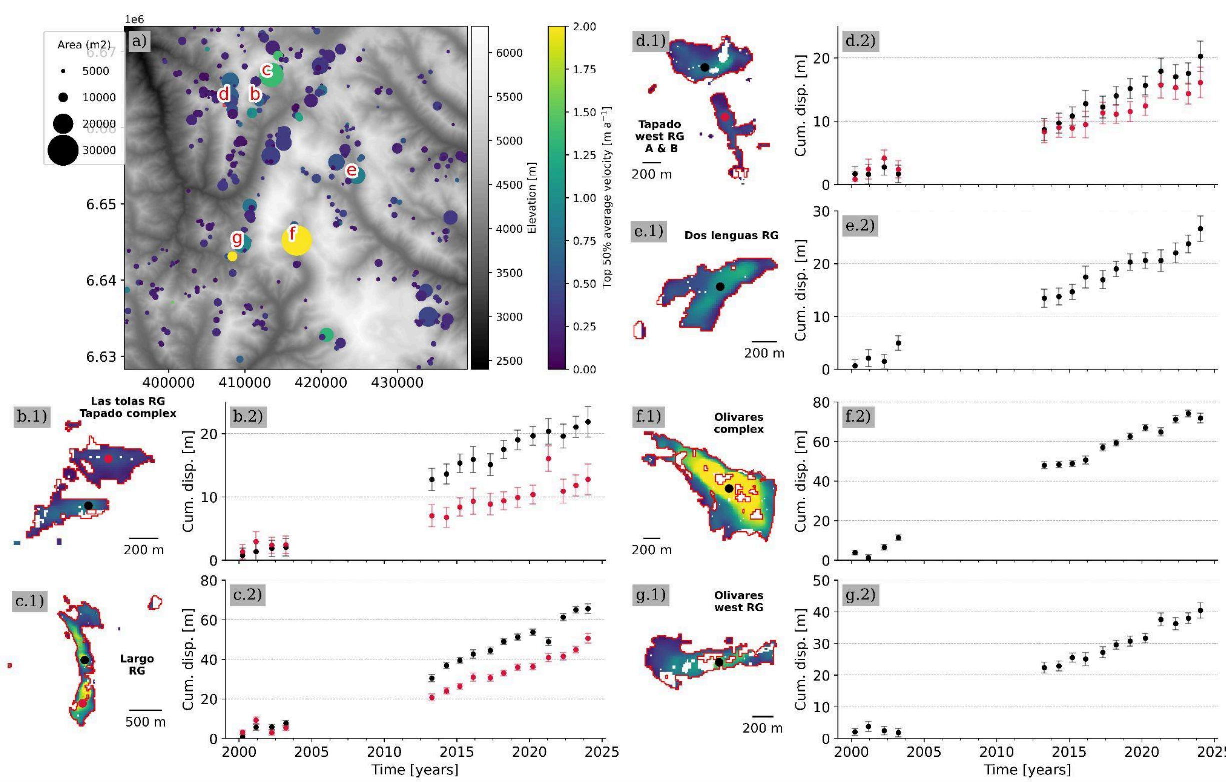

Figure 5: Surface kinematic characterisation for all PMAs in the central Andes region. a) Illustrates the spatial distribution of all valid PMAs (rock glaciers = 146; landslides = 115; others = 103) coloured by the 'Top 50% average velocity' (viridis colorbar) within the PMA surface. The size of the circle is proportional to the rescaled PMA surface in m²/1,000 for better visualisation. The red letters correspond to the study cases presented in the following subplots. The remaining subplots b) to g) ***(with a suffix of *.1)*** *illustrate mean annual velocity field over the 24 years (2000-2024) for a specific landform* ***(name is displayed in bold) where the magnitude of velocity is coloured using viridis colorbar from panel a). Subplots with a suffix of *.2 represents the*** *cumulative surface displacement time series in metres, extracted on the black (and red) point within the landform.* ***Cumulative error bars were computed NMAD on stable areas for each date respectively (Section 5.4).***

[...]

GC-3. Currently, I am concerned that there is insufficient evidence for the following conclusion: "Despite underestimations due to pixel size and temporal gaps of images, decadal velocity changes were observable under certain conditions, notably when average velocities are greater than 1 m yr-1. Below this velocity threshold, changes in velocity using L7/8 data are not statistically significant and could not be safely assessed." Similar statements can also be found in the abstract.

Response > As explained in Section 4.4. (Equation 2) and Section 5.5. (see the text below), the uncertainty of velocity change depends on the magnitude of the velocity of both encountered periods, for instance 2000-2013 and 2013-2014. An increase in velocity of 100% on small velocity magnitudes encompasses higher uncertainties. This is not the case with higher velocity magnitudes. The main reason behind it is the high uncertainties intrinsic to the L7/8 dataset and the error propagation through time. Please refer to Section 5.5 (text copied below) for more details about the given examples.

[...]

5.5 Velocity changes

Using 24-year surface displacement data, decadal velocity changes were analyzed by calculating surface velocities over two periods: 2000–2014 (V_1) and 2013–2024 (V_2), for all PMAs. The uncertainty in velocity change depends on the magnitude of velocity in both periods (Eq. 2). Smaller velocity magnitudes result in greater relative uncertainties, whereas higher velocities yield proportionally smaller uncertainties. ***To illustrate, a velocity increase from 0.5 to 1.0 m a^{-1} (100% change) has an uncertainty of 0.78 m a^{-1}, representing 78% of the relative change. In contrast, an increase from 1.0 to 2.0 m a^{-1} has an uncertainty of 0.39 m a^{-1}, or 39% of the relative velocity change. Consequently, only PMAs with velocities exceeding 1 m a^{-1} can be considered reliable for statistically significant velocity change.*** *By considering PMAs with velocities exceeding 1 m a-1, nine 'rock glaciers' and 2 'landslides'. Within those selected features, three rock glaciers showed an increase in velocity 11% (Fig. 5c), whereas six rock glaciers showed a decrease in velocity of 18% over two decades. On the other hand, two Landslides exhibited a larger increase in velocity of 50%. For further discussion, please refer to Section 6.5.*
[...]

GC-4. Was the accuracy of decadal velocity changes evaluated directly based on quantitative data? Section 5.2 contains a back-of-the envelope appraisal of the expected uncertainty in the relative change (which would fit better in the methods), but a dedicated assessment is missing. Furthermore, equation (2) seems suspect, as the numerator can be negative (I suppose it should be replaced by the geometric mean). The assumptions should be made explicit: If two quantities are assumed to be equal, it is not sufficient to say they are assumed to be similar. Section 5.3 contains an evaluation of velocity estimates, not of changes in velocity. The statement (and Section 5.2) mentions statistical significance, but it is not clear to me how and under what assumptions statistical significance was determined.

Response > Regarding the uncertainties assessment and the suggestion of moving the Section 5.2 in the methods section, please refer to the response of **GC-2** in this document to look more in detail about uncertainties assessment and the **new "Section 4.4. Average spatial velocity and relative velocity changes"** in the method section.

Regarding the Equation 2, the numerator can not be negative because there is no negative velocity. If the reviewer refers to the Equation 1, the value of velocity change could be negative because we are computing relative velocity changes in respect to the first period. Negative velocity changes reflect decelerations, compared to the first selected period.

$$V_{change} = \frac{V_2 - V_1}{V_1} \quad (1)$$

$$\sigma V_{change} = \left(\frac{V_2 + V_1}{V_1^2}\right) * \ \sigma V \ , \quad (2)$$

GC-5. In the statement from the conclusion, the observed underestimation is attributed to discrepant spatial scales and temporal gaps. It is not apparent to me what dedicated quantitative analyses were conducted to establish this conclusion, or whether it is a subjective interpretation.

Response > The observed underestimation of surface velocities was evidenced when the comparison between GNSS and pseudo-GCPs and satellite image correlation was carried out. In fact, this underestimation is highlighted in "Section 5.3 Velocity validation using GNSS and VHR datasets", from where, a quantitative analysis has been dedicated to this (more specifically in Figure 6 and 7). We added a numerical comparison between GNSS and pseudo-GCPs in the new Table 2 (please refer to the response of **GC-2** in this document) in the **new section 5.4. Reported uncertainties**.

GC-6. Presentation: I have found the manuscript difficult to follow, primarily for two reasons:
1) Cohesion: Discrepancies in content and terminology between the different sections presented challenges to my understanding of the manuscript. I was repeatedly taken by surprise by sudden changes in direction: The results introduce new methods and analyses that were not covered in the methods, while the discussion introduces new results and analyses not mentioned previously. The introduction contains a long literature review, but I have found it difficult to relate it to the remainder of the manuscript. In particular, the discussion section comprises six subsections, of which at least three have no easily discernible (for me) connection to the introduction or conclusion:

Response > We have corrected and homogenized all existing discrepancies in the terminology.

Regarding the "new methods" found in the discussion sections, we agree on the fact that some paragraphs were better placed in the methods and results sections. In this sense, we move those paragraphs to the corresponding sections in methods (**new Section 4.4 Average spatial velocity and relative velocity changes**) as well as one subsection from the discussions.

Regarding the introduction, we think that the introduction summarizes all key points related to how surface velocity was estimated classically, as well as all the limitations regarding each methodology (i.e; InSAR and optical imagery). We believe that all these statements are necessary to better know the reader, what is the main contribution of our robust methodological approach.

GC-7.- Section 6.2: I am not sure how this discussion relates to the objectives of the manuscript, as its connection to the presented results is tenuous. A new figure is introduced, but it is not described in detail. Is the primary purpose of the InSAR to classify the PMAs (together with Google Earth imagery) or does it also contribute to the quantitative appraisal of the Landsat results?

Response > The primary goal of InSAR wrapped interferograms was to corroborate PMAs. As PMAs come from an automatic extraction, it is not except for errors. This is evidenced in Table 1 showing that 60% of PMA were not corroborated by InSAR (mostly the smallest ones). The goal of the Figure 8 is to show the good agreement of largest PMAs as well as the ambiguity of some other features, notably of landslides, who may have been active during several years and were not detected within L7/8 dataset (Fig 8d and e) and thus, they don't appear on wrapped interferogram. Clear fringe patterns are visible on 60 days of interferograms, and not identified at all with L7/8 (Fig. 8b), very likely related to the type of landform (very slow moving landslides and slow rock glaciers). In addition, PMAs are often located in the middle and frontal sector of the landform, with some discrepancies in the upper region (Fig. 8d).

GC-8.- Section 6.3: Consider moving the new results shown here to the results section. In addition, the more tightly this analysis is integrated with the remainder of the manuscript, the easier it will be for the reader to appreciate it.

Response > We moved the entire paragraph as suggested. Please refer to the response of **GC-2** for a look at the changes..

GC-9.- Section 6.6: Introduces new results (Fig 10) that are not referred to elsewhere in the main body of the manuscript. Consider cutting it or motivating it.

Response > We decided to motivate this section rather than cutting it. We agree that this is not the main focus of the current manuscript. However, not so much studies have focused on regional patterns. We find it a bit unfortunate not to be able to value this regional aspect. So we decided to motivate the section. Now you can read:

6.5 ***Wider geomorphic implications*** *of PMA*

Understanding the broader geomorphic implications of PMAs is critical for interpreting their role within high mountain environments and their response to climatic and

geomorphological processes. While much of the manuscript focuses on kinematic and spatial characteristics of PMAs, this section aims to contextualize the observed patterns within a regional framework, bridging findings with topographic and geomorphological contexts, shedding light on the factors influencing their spatial distribution and surface dynamics. T*he PMAs* ***in the study area show*** *heterogeneous spatial distribution* ***across topographic conditions (Fig. 5a). Analysis of the*** *Top 50% average velocity* ***and its relationship to*** *slope, aspect, elevation and surface area,* ***derived from*** *the TanDEM-X 12.5 m DEM,* ***reveals several key patterns*** *(Fig. 11, Fig. S7)*

GC-10. Inconsistency in terminology also presents challenges to the reader. For instance, the comparison to GNSS is referred to as "ground truth" in the results only, while the word GNSS is only used in the subsection header in the results. Furthermore, the expression "false PMA" is only used in the discussion.

Response > We agree on the inconsistencies regarding “ground truth” terminology. We checked all the inconsistencies throughout the manuscript and we were replaced by GNSS.

GC-11. 2) Style: I find that the verbose style detracts from the content of the manuscript. I believe that reducing the word count by 25% is a realistic target. Removal of filler phrases such as "we can also state that", "we proceeded to compare surface velocity fields in more detail", "as mentioned in", "on the other hand", or "briefly", would help the reader focus on the content. So would strong topic sentences that succinctly summarize the content of the paragraph, thus guiding the reader through the manuscript. I provide more specifics in the minor comments.

Response > We effectively reduced 15% of the text guaranteeing the coherence and substance of the message we want to convey. In addition, we removed those “filler phrases“ to avoid redundancy within the manuscript.

GC-12. In addition, extensive language edits are advised, as illustrated by the following phrases from the abstract: "The results of this study shows [...]" -> show; "over a 24-years" -> over a 24-year period or over 24 years; "of which 153 corresponds" -> correspond; "providing an alternative to InSAR, for monitoring": remove comma.

Response > Modified as suggested.

Specific comments

SC-1. Title: Kinematics?

Response > Modified as suggested.

SC-2. Abstract: Mention the study area?

Response > We added some text in the abstract. Now you can read:

[...] (153 corresponding to rock glaciers) ***over a 24-years, over an area of 2250 km².*** *[...]*

SC-3. l23: Isn't it the method that underestimates the velocity?

Response > Not necessarily. As mentioned in Section 4.1, we applied the same methodology to two different types of data i.e. L7/8 and VHR. The results shown in Fig. 6 and Fig. 7, do not show underestimation on the VHR dataset. Only L7/8 underestimate the velocity. Mainly due to the coarse pixel size (i.e. 15 m) of L7/8 plays an important role on well discriminating detailed surface velocity patterns, resulting in an underestimation of velocities. All these aspects have been discussed in Section 6.1.

SC-4. l115: rapid: rugged or steep?

Response > We change “rapid” by “rugged” as suggested. Now you can read:

*[...] The **rugged** topography from coastal position [...]*

SC-5. l129-130: Incomplete sentence? Consider cutting the entire paragraph.

Response > We precise the sentence. No you can read:

[...] More recent studies highlight the complex interaction between remnants of glaciers, debris covered glaciers and rock glaciers (Navarro et al., 2023a; Robson et al., 2021) as well as the importance of rock glaciers as water storage resources (MacDonell et al., 2022; Schaffer et al., 2019; Schaffer and MacDonell, 2022). [...]

SC-6. l160: "data gap existed": Spatial data gaps due to SLC failure or data gap because you did not include these images?

Response > We did not include images within the 2004 - 2013 period for both reasons. We clarify this statement in the text. Now you can read:

*[...] **However, due to the** Scan Line Corrector **failure** on the **Landsat-7** satellite **between 2004 and 2013** (Markham et al., 2004), **we excluded Landsat-7 scenes within this period to avoid data gaps.** [...]*

SC-7. l196: I do not know what you mean by "interferograms averaged in 2-looks", as subsequently a 2 by 8 boxcar filter is mentioned.

Response > Multilooking processing is an optional step on SAR processing and is used to produce a product with nominal image pixel size. Multiple looks may be generated by averaging over range and/or azimuth resolution cells improving radiometric resolution but degrading spatial resolution. As a result, the image will have less noise and approximate square pixel spacing after being converted from slant range to ground range. The text refers briefly to the methodology used for multilook FLATSIM interferograms to advertise to the reader how the interferograms were built.

SC-8. l236: "The MGM algorithm reduces the amount of high-frequency artefacts": Compared to what method? High-frequency: what (presumably spatial) frequencies are being referred to?

Response > We added a clarification to the sentence. Now you can read:

*[...] The MGM algorithm reduces the amount of high-frequency **spatial** artefacts **(compared to classic Block-Matching algorithms)** in textureless regions and produces smooth surface displacement fields. [...]*

SC-9. l249: "redundant system" of equations? It would help to be explicit on the assumptions and methods here. Any regularization?

Response > No, is the redundant system of pixels. No regularization is used in the redundant system. Please refer to Bontemps et al., 2018 for all the details.

SC-10. l260: Is the slope direction oriented down or up the slope?

Response > is the downslope direction. We added this clarification on the text.

SC-11. l285: Fix subsection header

Response > changed as suggested.

SC-12. Table 1: Consider changing the class names "non valid" (or invalid?) and "valid" to something like not corroborated by / corroborated by InSAR to better convey the substantial epistemic uncertainty

Response > As suggested, we change the class names "non valid" and "valid" by "confirmed by InSAR" and "not confirmed by InSAR". We check the consistency throughout the manuscript.

SC-13. l305: of all PMAs?

Response > changed as suggested.

SC-14. l330: How was the NMAD computed (equation)? What normalization was applied?

Response > As Normalized Mean Absolute deviation (NMAD) is a common statistical metric to assess uncertainties in geospatial sciences, we belive that introduce the equation in the manuscript wont add any useful information. Nevertheless, we refer the reader to the scientific publication about NMAD meaning. Now you an read:

*[...] The Normalised Mean Absolute Deviation (NMAD; **Höhle and Höhle, 2009**) computed over stable [...]*

SC-15. l334: "due to window sizes of feature-tracking algorithms": Is this the only conceivable reason? If the attribution is speculative, consider removing it from the Results.

Response > We added some clarification why borders has velocities close to 0. Now you can read:

*[...] Fig. 5 b to g, the pixels located in the borders often have values close to 0 m a^{-1}, due **mainly to natural geomorphological causes (i.e. increased friction and low/no ice content in lateral margins) as well as** to window sizes of feature-tracking algorithms. [...]*

SC-16. l377: "It can be observed that": where?

Response > We removed the sentence as suggested.

SC-17. l392: A concise topic sentence that summarizes the entire paragraph would make this paragraph easier to read.

Response > Changed as suggested. Now you can read:

[...] We compare surface velocity fields in more detail for the two selected sub-regions around the Tapado and Largo rock glaciers (Fig. 2a). [...]

SC-18. l395: What is the time period of the various estimates? Where changes in displacement rates evaluated?

Response > Those details are already explained in Section 3.4 as well as Figure 6. Please Please refer to Figure 6 for more details.

SC-19. l401: important != big

Response > We prefer to keep important instead of big because even if both surface velocity fields are different, the underestimation of velocity goes up 30% only.

SC-20. l409: Is this the root mean square deviation?

Response > The value corresponds to the standard deviation of the comparison.

SC-21. l449: I am not sure what the hypothesis is, why it is rough, and its precise relation to the rest of the sentence.

Response > The sentence talks about the suitability of L7/8 for monitoring rock glaciers across the world. We modified the sentence to remove the ambiguity. BNow you can read:

*[...] This **suitability largely depends** on **rock glacier size**, **and** pixel covera**ge within** the landform **(Section 5.1)**. [...]*

SC-22. l464: what do you mean by "enhance/difficult"

Response > When using remote sensing imagery, the presence of ridges and furrows can sometimes enhance the contrast and texture of the surface. On the other hand, when images are taken with different solar angles, the shadow effect on ridges and furrows structure could also make feature tracking processing difficult. This is something well known on feature tracking algorithms and well explained in Kääb & Heid, (2012).

SC-23. l568: sub-estimation -> underestimation?

Response > Changed as suggested

SC-24. l578: "NMAD over stable areas corresponds to 60% of the [area]" What do you mean?

Response > The entire surface considered as a stable area used to quantify NMAD values is equal to 60% of the entire area. We removed this sentence from here as it is to far for the reader to discover the percentage of the areas used to compute NMAD. We placed the correctes sentence in Section 5.2. Now you can read:

[...] Our calculations indicate that the NMAD over stable areas (Fig. S4) equates to 0.07 m a^{-1} ± 0.16 as standard deviation. *[...]*

SC-25. l671: -> Conclusion

Response > Changed as suggested