Response to Reviewer #2,

We thank reviewer for the detailed reviews, and we made all suggested corrections. In this response, the reviewer's comments are in black standard font. Our response is in standard blue font and the modifications to the manuscript are in blue bold font.

Major comments:

1) I strongly suggest switching velocities to meters per year rather than meters per day, for three reasons. Firstly, the velocities are generated from monthly pairs, yet their values and uncertainties are reported in meters per day. Although the conversion from one unit to the other is a simple operation, it is confusing to use meters per day because the temporal resolution (monthly) does not allow for the retrieval of results in a unit so small compared to the real measurement (meters per month). Reporting meters per day assumes that the velocity observed can be discretized in days, while it is not. The second reason is that in the manuscript, the authors also refer to meters per year in some cases. This introduces more confusion, and they should adopt a unique, coherent unit throughout their study. Finally, it is my personal opinion that meters per year are a more relatable unit as they allow for spotting outliers more instinctively in the dataset. For example, an uncertainty on stable terrain of 0.05 m per day appears unrealistically small, but in meters per year (0.05 m/d = 18.25 m/yr) this metric appears more reasonable.

Thank you for this important suggestion. We agree that expressing velocities derived from monthly image pairs in m d^{-1} can be confusing, because the retrieved quantity represents the mean velocity over the full image-pair interval rather than a discretely observed daily motion. We also agree that a single and coherent unit should be used throughout the manuscript.

In response, we have revised the manuscript to report glacier surface velocity consistently in m yr^{-1} throughout the text, figures, tables, and uncertainty descriptions. We have also revised Sect. 3.3 to clarify that displacement is first divided by the temporal separation of each image pair to obtain an interval-mean velocity, which is then expressed in m yr^{-1} for reporting consistency and easier interpretation. In addition, all trend units have been converted accordingly (e.g., from m d^{-1} $decade^{-1}$ to m yr^{-1} $decade^{-1}$).

"For Landsat and Sentinel-2 imagery, velocities were derived with COSI-Corr using a frequency-domain cross-correlation algorithm. The search window was 32×32 pixels; the step size was 2 pixels for Landsat OLI and 3 pixels for Sentinel-2 MSI; the correlation threshold was 0.95. East-west (E-W) and north-south (N-S) displacements were combined to form total displacement, **which was then divided by the pairwise time baseline to derive the interval-mean glacier surface velocity for each image pair. Because the retrieved quantity represents the mean velocity over the full image-pair interval rather**

than discretely observed daily motion, all velocities reported in this study are expressed in m yr⁻¹. The resulting monthly velocity products were generated at 30 m spatial resolution."

These revisions remove the previous unit inconsistency and improve the interpretability of the reported velocities and uncertainties.

2) I don't know if I missed it in the text, but it seems that the authors do not convert the different remote sensing datasets to the same reference grid. This can yield important impacts on the results as the workflow only accounts for individual pixels. If velocities are generated on different grids, the data fusion will include overlaps and inconsistencies between the grids, potentially skewing the results especially for slow (stable) areas, which are the backbone of the uncertainty analysis and of the coregistration. I suggest that the authors resample each dataset to a common grid in order to homogenize the spatial location of each value.

Thank you for this helpful suggestion. We agree that explicit description of the pre-fusion grid harmonization is necessary for ensuring the validity and reproducibility of the pixel-wise WLS fitting and subsequent fusion. We have therefore added a brief statement in Sect. 3.3 clarifying (i) the reference grid used for harmonization, (ii) the resampling method adopted for co-registration, and (iii) that these operations are performed prior to the WLS fitting.

"To support pixel-wise WLS fitting and the subsequent fusion, all velocity products were harmonized to a common 30 m reference grid prior to WLS. We used the Sentinel-2 COSI-Corr velocity image as the reference geometry, and co-registered the Landsat-, Sentinel-1-, and UAV-derived velocity image to this grid using nearest-neighbour resampling, so that the original velocity values are preserved without interpolation smoothing."

3) I am not entirely convinced by the selection of Sentinel-1 to fill the data gaps. I do not understand why Sentinel-1 is not included more in the analysis (L.221 provides some information), but it is used to fill the gaps. I understood that it is considered "unreliable" on steep terrain, but we can make the argument that glaciers usually have a much flatter topography than stable terrain. I would like to see more explanations concerning the choice of Sentinel-1 for the gap-filling part.

Thank you for this valuable comment. We agree that the rationale for using Sentinel-1 in the gap-filling step needed to be explained more clearly. In the revised manuscript, we have strengthened this explanation by explicitly noting that, although Sentinel-1 offset tracking is generally less reliable in steep and deeply incised mountain terrain, glacier surfaces are usually smoother and less topographically extreme than the surrounding stable slopes. Therefore, Sentinel-1 remains suitable as an auxiliary source for filling gaps within glacierized areas. We have incorporated this reasoning into Sect. 3.4 and clarified that Sentinel-1 is used as an auxiliary constraint for gap repair rather than as the primary source for direct interpretation of the final fused velocities.

"**Although weighted fusion can effectively integrate multi-source information, some areas may still contain NoData. To further fill these gaps and enhance data continuity, this study introduces a sliding-window enhancement-coefficient infilling method. Because Landsat- and Sentinel-2-derived velocity maps often exhibit large gaps in this cloud- and snow-prone region, and simple interpolation can be unreliable in areas with spatially variable glacier motion, we use Sentinel-1 SAR velocities as a more spatially complete auxiliary field to guide gap repair. This choice is also supported by the topographic characteristics of glacierized terrain: although Sentinel-1 offset tracking is generally less reliable in steep and deeply incised mountain areas, glacier surfaces are usually smoother and less topographically extreme than the surrounding stable slopes. Therefore, within glacierized areas, Sentinel-1 can still provide useful motion-pattern information for gap filling, while avoiding an over-reliance on simple interpolation. In this framework, Sentinel-1 is used as an auxiliary constraint for reconstructing missing values rather than as the primary source for direct interpretation of the final fused velocities.** Specifically, Sentinel-1 is used to infer the local spatial variation of the fused field and fill NoData accordingly: for each pixel…"

4) I suggest that the authors develop further the discussion about the limitations of the "small" UAV footprint compared to the study area. While their analysis is comprehensive and interesting, its foundations can seem weak based on the fact that the data-fusion workflow relies on the weights evaluated over the UAV's area of interest. This can have important repercussions as it assumes that the relationships (and weights) established for a very narrow area are applicable to the rest of the study area, knowing that this UAV's area of interest does not encompass the same variety of surface parameters (aspect, slope, surface type, glacier type) as the whole study. I am aware that this is a strong limitation, but it needs to be discussed further and stressed more in the discussion.

Thank you for this important comment. We agree that the limited UAV footprint relative to the full study area is a significant constraint of our fusion framework and should be discussed more explicitly. In the previous revision, we clarified in Sect. 3.2 that the UAV surveys, although spatially limited, cover heterogeneous conditions including fast-flowing trunk ice, slower marginal zones, debris-covered and clean-ice surfaces, crevassed areas, and a seasonal window from the ablation season to the early accumulation period. However, we agree that this does not fully resolve the broader issue of representativeness.

In the revised manuscript, we have therefore strengthened the Discussion to explicitly acknowledge that the UAV-calibrated weights are derived from a relatively small reference area near the Yanong Glacier terminus and are then extrapolated to glaciers with potentially different aspect, slope, surface characteristics, glacier types, and seasonal states across the Kangri Karpo region. We now state clearly that this transferability assumption is a methodological limitation and should be kept in mind when interpreting the regional applicability of the fused product. We have also expanded the future-work discussion to note that this limitation could be reduced by using spatiotemporally adaptive weighting strategies and by incorporating additional high-accuracy reference data from multiple glaciers and seasons.

In Sect 3.2："In addition, we acquired UAV photogrammetry for reference and evaluation using a DJI Matrice 300 RTK equipped with an M6 Pro (M6P) metric mapping camera. Six orthomosaics were collected on 8 June, 4 July, 10 August, 1 September, 3 October, and 20 November 2023, forming five image pairs. Although coverage is limited to ~30 km² near the Yanong Glacier terminus, the UAV surveys span heterogeneous conditions: (i) the period covers the ablation season through the transition to the early accumulation period, (ii) the mapped area includes both fast-flowing trunk regions and slower glacier margins, and (iii) surface types include debris-covered ice, clean ice, and crevassed areas. **Therefore, this UAV subset provides a heterogeneous and useful reference for benchmarking the satellite-derived velocity products and for calibrating fusion weights in this study, although its limited spatial footprint means that full regional and year-round representativeness cannot be guaranteed.**"

In Sect 4.6："**A key limitation of this study is that the UAV reference data used to calibrate the fusion weights cover only a relatively small area near the Yanong Glacier terminus, rather than the full range of glacier and terrain conditions across the Kangri Karpo region. Although this UAV subset includes heterogeneous conditions, such as fast-flowing trunk ice, slower marginal zones, debris-covered and clean-ice surfaces, and observations spanning the ablation season to the early accumulation period, it does not fully encompass the broader diversity of aspect, slope, surface characteristics, glacier type, and seasonal states represented in the entire study area. Therefore, the present weighting scheme implicitly assumes that the relative performance of Landsat, Sentinel-1, and Sentinel-2 derived from this limited reference area is transferable to other glaciers and time periods, which may not always hold. This limitation should be kept in mind when interpreting the regional applicability of the fused product. Future work should therefore aim to develop spatiotemporally adaptive weighting strategies, for example by calibrating weights separately for different topographic settings, surface conditions, glacier types, and seasons, and by**

incorporating additional high-accuracy reference data from multiple glaciers and periods."

5) I would like to address the uncertainty analysis, which is a delicate topic when applied to glacier velocities from remote-sensing. In the ITS_LIVE documentation, the authors acknowledge the strong limitations of uncertainty estimation based on stable ground pixels. They do mention that their propagation to on-ice pixels is not trivial, and vastly underestimates the uncertainties over the ice. This is an issue inherent to any remote-sensing-derived ice surface velocity dataset, and I do not expect this manuscript to come up with a solution that the entire community still fails to provide. However, I would suggest that a more precise uncertainty analysis should be provided, especially considering that the authors have in their possession highly precise UAV data. My suggestion is to focus part of the uncertainty discussion on the specific area covered by the UAV, and compare the uncertainties derived from UAV-only velocities to fused velocities from Sentinel 2 and Landsat. I would also like the authors to acknowledge that the uncertainties from the data-fusion method are likely to be underestimated.

Thank you for this important and constructive comment. We agree that uncertainty estimates derived from stable-ground pixels cannot fully represent the true uncertainty over glacier surfaces, and that their propagation to on-ice pixels is inherently limited. We also agree that, within a data-fusion framework such as ours, the resulting uncertainty may be underestimated if it is evaluated only from stable terrain.

Following your suggestion, we have added an additional uncertainty assessment within the UAV survey area, where high-precision UAV-derived velocities are available as a local on-ice reference. In the revised manuscript, we present a new Figure 7 showing the uncertainty results of the UAV-, Landsat-, Sentinel-2-, and fused velocity products within the UAV-covered area from June to November 2023. The mean $Eoff$ values are 2.9 for UAV, 25.9 for Landsat, 21.7 for Sentinel-2, and 18.6 for the fused product. These results show that the fusion method effectively reduces uncertainty relative to the individual optical products, but that a substantial gap still remains between the fused result and the UAV-derived reference. This indicates that data fusion improves the velocity estimates, but does not reach the accuracy level of the high-precision UAV observations.

Based on this comparison, we have revised the manuscript to explicitly acknowledge that uncertainty estimates based on stable-ground pixels, although useful as a regional and long-term reference metric, cannot fully capture the additional error sources present over glacier surfaces, such as surface deformation, crevassing, seasonal snow cover, illumination changes, and texture evolution. We now clearly state that the uncertainty estimated from the present data-fusion framework is likely to be underestimated. We also emphasize that the UAV-footprint analysis serves as an important local on-ice complement to the stable-area uncertainty assessment, rather than a replacement for it.

"Figure 7 presents the uncertainty analysis results for the Landsat-derived velocities, Sentinel-2-derived velocities, UAV-derived velocities, and fused velocities within the UAV survey area. The results show that the fusion method effectively reduces uncertainty relative to the individual optical products. However, a substantial gap still remains between the fused results and the UAV-derived reference. This indicates that, although data fusion can reduce errors to some extent, it still does not achieve the accuracy level of high-precision UAV observations. Moreover, uncertainty estimates based on stable-ground pixels and subsequently propagated to glacier surfaces have inherent limitations. Therefore, the uncertainty obtained from the present data-fusion framework is likely to be underestimated.

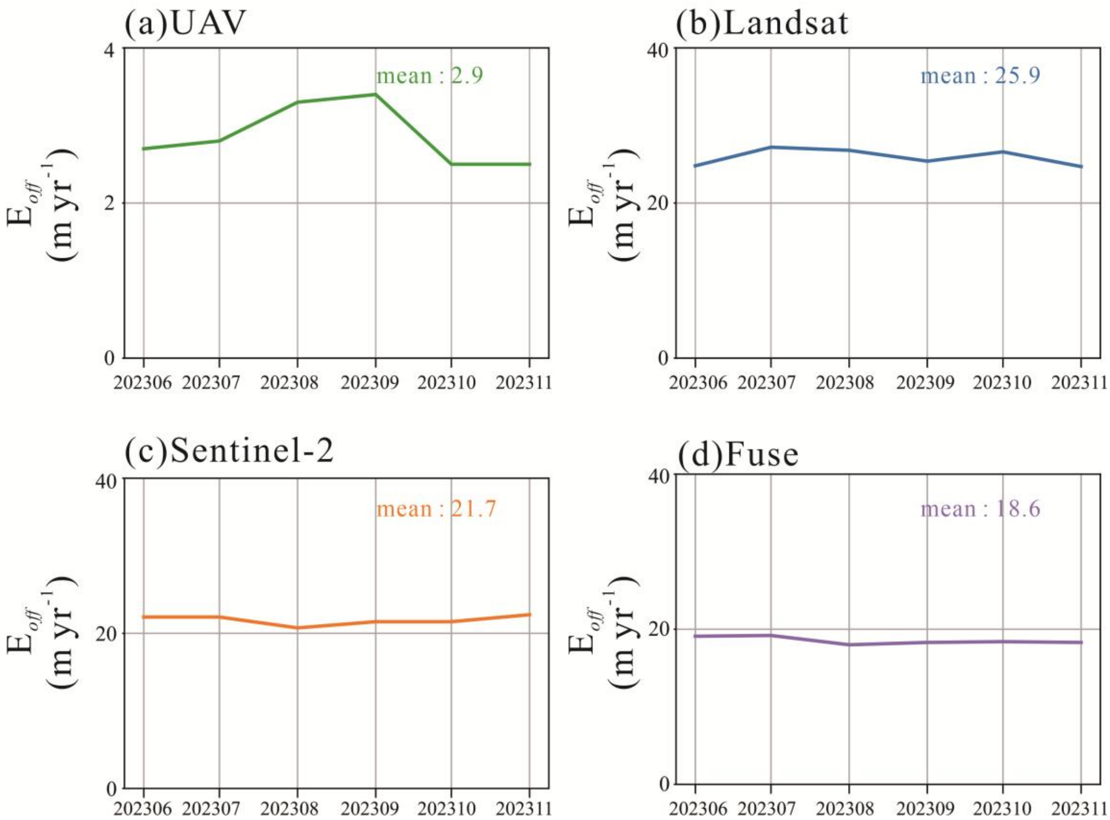

Figure 7: Uncertainty analysis of the four velocity products within the UAV survey area from June to November 2023: (a) UAV-derived velocity uncertainty; (b) Landsat-derived velocity uncertainty; (c) Sentinel-2-derived velocity uncertainty; and (d) fused velocity uncertainty.

"

6) My last major comment focuses on the reconciliation of the different temporal baselines of the image pairs used for generating velocities. There is an obvious limitation to generating monthly velocities (or assumed monthly) when the temporal baselines of the image pairs fluctuate by several weeks and rarely correspond to a month exactly. This has the potential for skewing the analysis, knowing that an important

part of it focuses on the monthly velocities. Recent work from Charrier et al. (2021 & 2025, complete reference in the pdf) provides tools to correct the overlap between different image-pair baselines and produce regularized velocity time-series. I am aware that the data analysis of the manuscript might have occurred before Charrier et al. published their Python package and manuscript, as I am aware that applying their method to the output of this study might represent a tremendous amount of work. I suggest trying to apply their workflow to the final output of this manuscript, or at least mentioning their method in the discussion of the manuscript (specifically the discussion part about "limitations and future directions"). I do not have a conflict of interest concerning this suggestion. I do however want to mention that I have worked with L.Charrier in the past which is why I am aware of her publication.

Thank you for this very important comment. We agree that reconciling the different temporal baselines of the image pairs is a key issue for nominal monthly glacier-velocity products. In our current workflow, glacier displacement is always divided by the exact temporal separation of each image pair, so the retrieved quantity is the interval-mean velocity for that pair rather than a velocity obtained by assuming an exact one-month baseline. However, we fully agree that differences in revisit cycles among Landsat, Sentinel-1, and Sentinel-2 lead to baseline-length mismatches, temporal overlap, and temporal smoothing, which may affect the strict comparability of the monthly time series and introduce uncertainty into the interpretation of intra-annual variability.

We appreciate your suggestion to consider the regularized time-series framework proposed by Charrier et al. We agree that this is a very valuable direction, as it can better reconcile irregular and overlapping image-pair baselines and generate temporally regularized velocity time series. However, applying that workflow to the full multi-sensor dataset used in this study would require substantial reprocessing and restructuring of the present pipeline, and is beyond the scope of the current revision. Following your suggestion, we have therefore strengthened the discussion in the “Limitations and future directions” section by explicitly acknowledging this limitation and by citing the approach of Charrier et al. (2021, 2025) as an important avenue for future improvement. We now also clarify that the present products should be interpreted as nominal monthly interval-mean velocities rather than strictly calendar-month velocities.

In sect 4.6:” **In addition, differences in sensor revisit cycles (Landsat ≈ 16 d; Sentinel-1 ≈ 12 d; Sentinel-2 ≈ 10 d) preclude strict month-to-month alignment, producing baseline-length mismatches, temporal overlap, and temporal smoothing that inevitably introduce additional uncertainty into the nominal monthly velocity series. Although glacier displacement was always divided by the exact temporal separation of each image pair, the resulting products should therefore be interpreted as nominal monthly interval-mean velocities rather than strictly calendar-**

month velocities. This limitation is particularly relevant for the analysis of intra-annual variability, because irregular and overlapping baselines may blur short-term temporal signals. Future work should explore regularized velocity time-series approaches that explicitly reconcile overlapping image-pair baselines, such as the framework proposed by Charrier et al. (2021, 2025), in order to generate temporally more rigorous and directly comparable glacier-velocity series."

Detailed comments:

L.65: Since the uncertainty of the reduction is stated and different from the uncertainty of 2015, state the uncertainty of the 1980s result.

Thank you for this comment. We agree that, since the uncertainty of the area reduction is reported, the uncertainty of the 1980s glacier-area estimate should also be stated explicitly. In the revised manuscript, we have corrected this sentence according to the original values reported in Wu et al. (2018). The glacierized area in 1980 is now given as 2728.00 ± 34.24 km², rather than the previously cited inventory value without uncertainty, and the corresponding reduction is consistently reported as 679.51 ± 59.49 km² relative to the 2015 area of 2048.50 ± 48.65 km². The text has been revised accordingly.

L.82: Are topographic shadows also an issue for optical datasets in this region? Especially if we consider small glaciers in steep valleys.

Thank you for this helpful comment. We agree that topographic shadowing is also an important limitation for optical remote sensing in this region, especially for small glaciers located in steep and deeply incised valleys. In the revised manuscript, we have therefore expanded the description of the optical-data limitations to explicitly include terrain-induced illumination effects (e.g., topographic shadowing), in addition to cloud cover and seasonal snow.

"**Optical sensors offer high spatial resolution, but their applicability is strongly constrained by persistent cloud cover, seasonal snow, and illumination effects such as topographic shadowing, especially for small glaciers in steep valleys, thereby limiting temporal coverage and data usability (Scherler et al., 2008; Berthier et al., 2005)**"

L.124: What is the reference grid? Since the sensors all have their own grid, their derived velocities need to be resampled on a reference grid. Can you detail how you do this step?

Thank you for this comment. We agree that the reference grid and the resampling procedure should be described explicitly, because pixel-wise fusion requires all input velocity products to be aligned to a common grid. In the revised manuscript, we have clarified that, prior to WLS fitting and fusion, all velocity products were harmonized to a common 30 m reference grid. Specifically, we used the Sentinel-

2 COSI-Corr velocity field as the reference geometry, and the Landsat-, Sentinel-1-, and UAV-derived velocity fields were co-registered and resampled to this grid using nearest-neighbour resampling, so that the original velocity values were preserved without interpolation smoothing. We have added this explanation explicitly in Sect. 3.3.

“To support pixel-wise WLS fitting and the subsequent fusion, all velocity products were harmonized to a common 30 m reference grid prior to WLS. We used the Sentinel-2 COSI-Corr velocity image as the reference geometry, and co-registered the Landsat-, Sentinel-1-, and UAV-derived velocity image to this grid using nearest-neighbour resampling, so that the original velocity values are preserved without interpolation smoothing.”

L.125: Can you give a quick example in parentheses?

Thank you for this helpful suggestion. We agree that a brief example improves clarity. In the revised manuscript, we have added a short parenthetical example after “basic corrections” to clarify the preprocessing step, namely applying orbit files to Sentinel-1 images prior to offset tracking.

“(1) Image pairing and preprocessing: Assemble monthly image pairs from Landsat-8/9 OLI and Sentinel-2 MSI (optical) and Sentinel-1 IW GRD (radar); perform geometric co-registration and basic corrections**(e.g., applying orbit files to Sentinel-1 images)**.(2)…”

L.130: Can you specify which robust filtering you are using? Or say directly if it's detailed later in the methods.

Thank you for this helpful comment. We agree that the wording here was too brief. In the revised manuscript, we have clarified that the robust filtering method is described in detail later in Sect. 3.3.

“.(4) Temporal robustification and quality control: Remove mismatches using the signal-to-noise ratio (SNR) threshold and a robust time-series filtering method **(described in detail later in Sect. 3.3) to improve cross-sensor consistency and reliability**.(5)…”

L.131: Can you explain in a few words what it means? I do not understand what an "enhancement-factor field" is in this case. Especially considering "enhancement-factor field" does not appear later in the text.

Thank you for this helpful comment. We agree that the term “enhancement-factor field” was unclear in this summary description. In the revised manuscript, we have replaced it with a more explicit and consistent explanation. We now clarify that this step estimates a local enhancement coefficient from overlapping Sentinel-1 and preliminary fused velocities, smooths this coefficient spatially, and then uses it to reconstruct missing pixels. This wording is now aligned with the terminology used later in the Methods section.

“(6) **Sentinel-1-guided gap filling: For residual voids, construct a spatially smoothed enhancement-coefficient field from the relationship between Sentinel-1 and the preliminary fused velocities, and use it to reconstruct missing pixels and improve spatiotemporal continuity.**(7)…”

L.147: I would suggest separating this supplementary table into 2: one for Landsat 8 and one for Landsat 9, or at least specify whether you used a mix of the two satellites for the pairs or if you generate pairs only with the same satellite.

Thank you for this helpful suggestion. We agree that the Landsat pairing strategy should be clarified more explicitly. In this study, Landsat velocity pairs were allowed to be generated from both same-satellite and cross-satellite acquisitions, i.e., Landsat-8/Landsat-8, Landsat-9/Landsat-9, and Landsat-8/Landsat-9 pairs, depending on image availability and cloud conditions. In the revised manuscript, we have added an explicit clarification in the main text to state this pairing rule. We retained the existing supplementary table format, but the pairing strategy is now clearly described in the manuscript.

“**Detailed information on the Landsat imagery used in this study is provided in Table S1 of the Supplementary Material. Landsat-8 OLI and Landsat-9 OLI-2 were treated jointly rather than as independent sensor streams because of their high inter-sensor consistency (Xu et al., 2024), and Landsat velocity pairs were therefore allowed to be formed from both same-satellite and cross-satellite acquisitions (Landsat-8/Landsat-8, Landsat-9/Landsat-9, and Landsat-8/Landsat-9).**”

L.159: Could you add a supplementary table of the exact dates at which the UAV mosaics were collected? It would be consistent with the fact that you provide tables for Sentinel 1 & 2 and Landsat.

Thank you for this helpful suggestion. We agree that the exact acquisition dates of the UAV mosaics should be stated explicitly. Because the number of UAV mosaics used in this study is limited, instead of creating a separate supplementary table, we have added their exact acquisition dates directly in the main text. The revised manuscript now states that six UAV orthomosaics were collected on 8 June, 4 July, 10 August, 1 September, 3 October, and 20 November 2023, forming five image pairs.

L.190: This echoes my comment from L.147: I suggest you write a short sentence about why Landsat 8 and 9 are not considered as independent sensors. Maybe you can cite: Xu, Hanqiu, Mengjie Ren, and Mengjing Lin. 2024. “Cross-Comparison of Landsat-8 and Landsat-9 Data: A Three-Level Approach Based on Underfly Images.” (they seem to show that the two sensors are similar). I am asking because in comparison, ITS_LIVE separates the two missions when using image pairs (although I am not certain why)

Thank you for this helpful suggestion. We agree that the rationale for not treating Landsat-8 and Landsat-

9 as independent sensors should be stated more explicitly. In the revised manuscript, we now clarify that Landsat-8 OLI and Landsat-9 OLI-2 were treated jointly in this study because of their high inter-sensor consistency, and because combining them helps increase the number of usable image pairs in this cloud-prone mountain region. We have also added the suggested reference (Xu et al., 2024), which shows a high level of consistency between Landsat-8 and Landsat-9 data.

L.199: See specific comment section.

Modified accordingly.

L.205: Is this the "enhancement-factor field"? If yes, they have different names and this can be confusing.

Thank you for this comment. We agree that using different names for the same concept could be confusing. In the revised manuscript, the terminology has been made consistent throughout, and this term is now referred to uniformly in the text.

L.206: I suggest specifying the standard deviation used for the Gaussian filter, as this variable can have huge impacts on the results of the filter (for reproducibility purposes).

Thank you for this helpful comment. We agree that the Gaussian filter parameter should be reported explicitly for reproducibility. In the revised manuscript, we now specify that the enhancement-coefficient field was smoothed using a Gaussian filter with a standard deviation of σ=2 pixels. We also clarify that nearest boundary handling was used during filtering.

"**Next, $\hat{\boldsymbol{a}}(\boldsymbol{i},\boldsymbol{j})$ is smoothed with a Gaussian filter(σ =2 pixels) to form a spatially varying enhancement-coefficient field $\boldsymbol{a}(\boldsymbol{i},\boldsymbol{j})$ over the entire image. Finally, for missing pixels $(\boldsymbol{i},\boldsymbol{j})$ in the fused map, infilling is performed as…**"

L.226: I suggest using a column : for coherence with the rest of the paragraph.

Modified accordingly.

L.227, 229, 230: Needs a space after the dot.

Modified accordingly.

L.232: This comment might apply to the introduction. It would be worth mentioning that the glaciers are not surge-type, for two reasons: 1) Other glaciers in the area are surge-type (Guillet et al., 2022). 2) A surge-type glacier could cause a great change in the standard deviation, thus invalidating these claims. You could clear any doubt concerning these claims by specifying that the glaciers are not surge-type.

This would also help justify that you consider velocities above 1.5* or below 0.5* their reference as outliers.

Thank you for this important comment. We agree that this assumption should be stated explicitly. In the revised manuscript, we now cite the published surge-type glacier inventory for High Mountain Asia (Lv et al., 2022). After checking this dataset against our study area, we found that no glaciers within the study area are identified as surge-type glaciers. This clarification has been added in Sect. 2 and also supports the interpretation of the smoothness analysis and the outlier-screening strategy in Sect. 3.3.

In sect 2:" Glaciers are widely distributed across the massif. According to RGI 7.0, the region contains 218 glaciers (Figure 1): 162 smaller than 1 km², 30 between 1 and 5 km², and 26 larger than 5 km². Among the largest, the Yanong and Xirinongpu glaciers have surface areas of approximately 165 km² and 94 km², respectively (RGI Consortium, 2023). **According to the published surge-type glacier inventory for High Mountain Asia (Lv et al., 2022), no glaciers within our study area are identified as surge-type glaciers. Accordingly, surge-type behaviour is not considered in the subsequent analysis.**"

In sect. 3.3"To improve reliability, we first removed mismatches with speeds > 1095 m yr^{-1} and low-confidence pixels with SNR < 0.9. For the optical-image-derived velocities (Landsat and Sentinel-2), additional stabilization is necessary because optical matching is more susceptible to cloud/cloud-shadow contamination, seasonal snow cover, illumination changes, and surface-texture variations, which can produce anomalously large or small mismatches in some months. **Because no glaciers within our study area are identified as surge-type glaciers in the published High Mountain Asia inventory of Lv et al. (2022), such abrupt excursions are unlikely to reflect true surge-related dynamics.** We therefore applied…."

L.239: How do you select these areas? Using RGI outlines or by manually filtering snow and ice-free pixels?

Thank you for this comment. In this study, the stable areas were selected manually rather than automatically extracted from the RGI outlines. Specifically, we manually identified topographically stable, snow-/ice-free bare-rock areas surrounding the glaciers and excluded zones that showed obvious snow cover, shadow, water, or potential surface instability. We have clarified this point in the revised manuscript.

"Considering the scarcity of long, continuous ground observations in high-mountain regions, **we employ a stable-area velocity-fluctuation method, constructing an error model in manually selected topographically stable, snow-/ice-free bare-rock zones surrounding the glaciers** to indirectly evaluate the velocity uncertainty of different products."

L.272: I notice in the supplementary materials that the southern and southwester areas are consistently "flaring up", especially from 2017 to 2022. 2022 also has really high velocities that look like noise and not a coherent pattern. Could you comment on this? It's a non-negligible part of the study area that seems impacted.

Thank you for this careful observation. We agree that the southern and southwestern parts of the study area show persistent local “flaring” patterns in some years, especially between 2017 and 2022, and that the particularly high velocities in 2022 do not always form a fully coherent glaciological pattern.

We interpret this behaviour mainly as a consequence of the challenging observation conditions in that sector. The southern and southwestern parts of the study area are located near mountain passes, where glaciers occupy narrow, steeply incised valleys with very large local elevation differences. In addition, this area is a major corridor for moisture transport, and is therefore frequently affected by persistent cloud and fog cover. Under these combined conditions, all three satellite data sources used in this study are more prone to mismatches, decorrelation, and residual retrieval noise than in other parts of the region.

Although we applied relatively strict outlier-screening and filtering procedures, these measures cannot completely eliminate retrieval artefacts in such particularly unfavorable terrain and atmospheric conditions.

L.310: How would you explain the jumps in variance in the Landsat and Sentinel products that are not replicated by the fusion? Are these outliers or actual ice acceleration/deceleration?

Thank you for this important question. We interpret the abrupt jumps in variance seen in some monthly Landsat and Sentinel-2 products mainly as retrieval artefacts rather than as true glacier acceleration or deceleration signals. In this cloud- and snow-prone mountain region, optical matching is highly susceptible to cloud/fog contamination, shadow, seasonal snow, and local illumination changes, which can introduce anomalous mismatches in certain months and cause abnormally large pixelwise variance.

These variance jumps are not reproduced in the fused product because such anomalous optical mismatches are suppressed during the fusion workflow. Specifically, obvious low-quality matches are first screened using quality-control criteria, and the optical time series is then further stabilized using the robust temporal filtering procedure described in Sect. 3.3. As a result, anomalous values that artificially inflate the variance in individual Landsat or Sentinel-2 products are largely removed before or during fusion, and therefore do not propagate into the final fused result.

L.321: Needs a "t".

Modified accordingly.

L.325: If I understood well, you take the GoLIVE uncertainties directly from the dataset. I see a potential issue for direct comparison, because GoLIVE uncertainties probably do not consider the same off-ice pixels as you do, and might therefore include more outliers and impact the sigma values. It would be worth mentioning this, otherwise the datasets are not directly comparable for their uncertainties.

Thank you for this important comment. We would like to clarify that the uncertainty values compared in this study were calculated within the same type of manually selected off-ice stable areas, rather than directly adopting the uncertainty values provided in the original datasets. In other words, the uncertainty estimates for GoLIVE, ITS_LIVE, and the products generated in this study were all evaluated over manually delineated ice-free stable zones surrounding the glaciers, using a consistent framework.

L.345, 356, 358: I understand that it could be the custom to just write $p<0.05$ rather than its value. I thought maybe it could add more details about the analysis by allowing for the comparison of the significance between each test. Up to you if you want to change it.

Thank you for this helpful suggestion. We agree that exact p values may provide additional detail for comparing significance levels across tests. However, to maintain a concise presentation and a consistent reporting style throughout the manuscript, we prefer to retain the notation $p < 0.05$ in the main text.

L.351: It might be worth showing the correlation just for the centerline points as well. It seems that they correlate much better, which is a fascinating result: the correlation between slope and velocity could be a function of the distance to the centerline.

Thank you for this insightful suggestion. We agree that examining the slope–velocity relationship specifically along the glacier centerline could provide additional information, and that the correlation may indeed vary with distance from the centerline. This is an interesting direction for further analysis. However, the present study focuses on the regional-scale relationship between slope and velocity based on the full glacier area, and a dedicated centerline-based analysis would require an additional methodological framework beyond the current scope of the manuscript. We have therefore not added this extra analysis here, but we agree that it is a valuable topic for future work.

L.402: I suggest switching to parentheses because the "-" sign impacts readability.

Modified accordingly.

L.471: You did mention precipitations earlier in the text. Why not include an ERA-5 plot of the average precipitation on the same plot (secondary Y-axis)?

Thank you for this helpful suggestion. We agree that precipitation is also an important climatic factor potentially related to glacier velocity. However, we chose not to add ERA5 precipitation to this figure for two main reasons. First, compared with air temperature, the influence of precipitation on glacier surface velocity is more indirect and complex. In particular, precipitation falling on a glacier does not necessarily or immediately reach the glacier bed; instead, it may be temporarily stored within the snow/firn/ice system, and the associated hydrological response can vary substantially among glaciers. Therefore, placing precipitation on the same plot could easily suggest a simple direct relationship that is difficult to justify in the present context.

Second, precipitation reanalysis products are generally subject to relatively large uncertainties in this high-mountain region. A rigorous precipitation-based interpretation would therefore require additional processing and evaluation (e.g., bias correction and/or downscaling), which is beyond the scope of the present study. For these reasons, we retained the current figure design and focused this part of the discussion on temperature only.

L.485: I think this section could benefit from two points: 1) You should expand on the possible impacts of calibrating the weights on the glacier tongue only, and what it might imply for areas at higher altitude with different surface cover (snow, less debris, different aspects, slope). It is good that you are acknowledging this limitation, but in my opinion this potentially yields stronger limitations on the applicability of those weights (as shown by the consistently noisy results south of the domain, as shown by the mosaics in the supplements for years 2017 to 2022). 2) The second point concerns the adaptive weighting. This is a note I will put in the general comments: some work has been done that would complement yours greatly, by Charrier, L., Dehecq, A., Guo, L., Brun, F., Millan, R., Lioret, N., Copland, L., Maier, N., Dow, C., and Halas, P.: TICOI: an operational Python package to generate regular glacier velocity time series, The Cryosphere, 19, 4555–4583, https://doi.org/10.5194/tc-19-4555-2025, 2025.

I am aware that most of your analysis might have been done before this paper was released. They provide a straightforward and ready-to-use code on their GitHub. I think it addresses some of your future direction points because your results are inherently impacted by the issue of non-alignment of the dates for the monthly pairs, and thus have some overlap between the velocities when you fuse them.

Modified accordingly.

L.504: Inconsistent use of units, I suggest either using only m/d or m/yr. I personally have a preference for m/yr, as detailed in the specific comments.

Modified accordingly.