

Author's Response Letter

We would like to again thank both the reviewers and the editor for reviewing and providing thoughtful and constructive feedback, and for handling our manuscript. We also appreciate the time and effort for the same. The point-by-point response below is given as per the reviewer's comments, separately in two sections.

Response from Authors to Anonymous Reviewer 1 (RC1)

I sincerely apologize for the delay in completing my review. I thank the authors for their detailed reply and the revisions made. The revised manuscript is now scientifically strong and logically sound, and it fills an important gap in the understudied Zanskar region.

Most of my previous suggestions have been adequately incorporated. However, I would like to highlight one remaining issue related to the use of raw ERA5-Land data. The authors cite Mandal et al. (2024) to support the statistical performance of the raw dataset. While the coefficients of determination are indeed high ($r^2 = 0.93\text{--}0.97$ for temperature), Satyapragyan et al. (2026) demonstrate that strong linear correlations do not preclude large systematic biases. In the Higher Central Himalaya, raw ERA5/ERA5-Land showed seasonal temperature offsets of 5–8 °C and RMSEs of 7–8 °C. Notably, Mandal et al. (2024) also reported a wide RMSE range of 2.3–14.9 °C for temperature in their validation (Sect. 3.6), indicating that substantial absolute offsets exist despite the strong correlations.

To the best of my knowledge, trend analyses of raw versus bias-corrected ERA5 data can differ significantly (paper currently under review). Therefore, I strongly recommend that the authors clearly discuss this important limitation (Satyapragyan et al. 2026) in the Discussion section. This would greatly benefit readers and improve the transparency of the manuscript.

We thank the Anonymous Reviewer again for reviewing and providing thoughtful and constructive feedback on our manuscript.

Based on the general comment above regarding the ERA-5L bias, we added the following section in the Limitation section (5.4) as given below:-

“Finally, due to the lack of reliable in situ meteorological observations, ERA5-Land data were not bias-corrected. While this may influence the magnitude of derived climatic trends (e.g. Satyapragyan et al., 2026), it does not affect the primary objective of this study, which is to examine the temporal evolution of glacier velocity.”

Please find the line-by-line response (in blue) to all the comments (in black) below-

Minor suggestions:

L15: The velocity trend is given as “-2.4 m year⁻¹ decade⁻¹” in the abstract, but the text (L277) and Table 2 report -2.43. Please use consistent rounding (“-2.4” or “-2.43”) throughout.

Changed to maintain consistency throughout the manuscript.

L16: The abstract states approximately $-0.21 \text{ m year}^{-1}$, whereas the text (L352) uses $-0.22 \text{ m year}^{-1}$ for the 2000–2005 period. Please ensure consistency.

Changed to maintain consistency.

L276: The text reports a region-wide p-value of 0.027, whereas Table 2 shows $p = 0.00$ (i.e., $p < 0.01$) for “All Glaciers”. Please update the text to match the table or clarify that $p = 0.027$ refers to a different statistical test (e.g., pre- vs. post-2010).

Made the changes in the text and table to $p < 0.01$.

L405: The reported winter precipitation decline of “ $-0.0003 \text{ mm yr}^{-1}$ ” is very small relative to the values shown in Figure 10b ($\sim 0.01\text{--}0.04 \text{ mm}$). Please consider rephrasing this as “no statistically significant change” for more clarity.

I hope this will help.

Thanks for pointing this out. The sentence now reads:

“In contrast, the winter precipitation shows no statistically significant change (figure 10b).”

Response from Authors to Reviewer 2 (RC2)

I reviewed a previous version of this article, and recommended it be published after suitable revision. Having carefully reviewed the changes based on all reviewer comments, I agree that this manuscript is now in good shape and close to publication. I have listed a few technical corrections below, mostly on the abstract and other key areas, and suggest the manuscript be published after a final round of these changes.

We thank Dr Maximillian Van Wyk de Vries again for reviewing and providing thoughtful and constructive feedback on our manuscript. We also appreciate the time and effort for the same. We have updated some texts for better clarity and readability of the manuscript. Please find the line-by-line response (in blue) to all the comments (in black) below-

Title – The change is clearer although I think you want to make sure you clearly highlight how you infer causation between these and not just correlation in the abstract. It is currently clear from what you write that they both slowed and thinned, but not how the thinning ‘induced’ the slowdown.

Thanks for pointing this out. Please see the modifications made in the abstract.

“This slowdown is accompanied by progressively increasing surface thinning, from $\sim 0.22 \text{ m year}^{-1}$ between 2000 and 2005 to $\sim 0.57 \text{ m year}^{-1}$ by 2015–2020, with the strongest thinning and velocity reductions occurring at lower elevations. The spatial and temporal agreement between thinning and velocity decline, together with the expected reduction in driving stress associated with thinning, suggests that ice mass loss is the dominant control on the observed deceleration”

L10 'but' -> 'and' possibly

Corrected.

L11 'Remote sensing-based image correlation provides a tool for observing surface' -> 'Satellite image correlation enables observation of surface'

Updated as suggested.

L15 'overall experienced deceleration at an average rate of -2.4 m year⁻¹ decade⁻¹ in this region' – deceleration at a rate of -2.4 is somewhat ambiguous and could be interpreted as a speedup. Maybe remove the negative sign? Or 'decelerate with an average change in speed of -2.4'

We agree to this, and we made the following changes in the abstract:

“We show a statistically significant, region-wide deceleration in glacier flow, with a mean rate of 2.43 m year⁻¹ decade⁻¹. This slowdown is accompanied by progressively increasing surface thinning, from ~0.22 m year⁻¹ between 2000 and 2005 to ~0.57 m year⁻¹ by 2015–2020,”

L16 Same as the comment above about elevation decrease of -0.21

Please see the previous response.

L17-21 These last sentences of the abstract can be strengthened and would set the paper up better.

This sentence “While glacier mass loss, particularly through thinning, and associated reduction in driving stress was identified as the primary driver of velocity deceleration, glacier-specific characteristics, such as geometry, topography, debris cover and terminus type, also influenced glacier response.” would be better if connected with your evidence.

This last sentence “Overall, our results confirm loss of ice mass in this region, which generally corresponds with a reduction in surface velocity.” This seems to not quite highlight the key result here, and possibly be a little misleading (as this paper does not really ‘confirm’ ice mass loss). Perhaps it would be better to highlight the insight gained into glacier dynamics, something about the longer-term trends, or other aspects that come directly from your data.

Thanks for pointing this out. We updated the abstract to incorporate these changes, and it now reads:-

Abstract: Trends in surface velocity provide insight into glacier response to climate change as well as local drivers of ice dynamics. The Zaskar Himalayas are heavily glacierised, and retreating glaciers pose a threat to local and regional water security. Satellite image correlation provides a tool for observing surface velocity over multiple glaciers in a remote and challenging area for field work, providing key observations for tracking changes in this important region. This study provides a comprehensive analysis of long-term (1992-2023) interannual glacier surface velocity and elevation change for 12 selected glaciers with varied

morphological characteristics in the Zaskar Basin of the Ladakh Himalayas. We show a statistically significant, region-wide deceleration in glacier flow, with a mean rate of 2.43 m year⁻¹ decade⁻¹. This slowdown is accompanied by progressively increasing surface thinning, from ~0.22 m year⁻¹ between 2000 and 2005 to ~0.57 m year⁻¹ by 2015–2020, with the strongest thinning and velocity reductions occurring at lower elevations. The spatial and temporal patterns between thinning and velocity decline, together with the expected reduction in driving stress associated with thinning, indicate that ice mass loss is the dominant control on the observed deceleration. Glacier-specific characteristics such as geometry, topography, debris cover, and terminus type further influenced glacier response. For example, lake-terminating glaciers exhibited local increases in ice velocity near their termini. Overall, our results demonstrate a sustained multi-decadal slowdown in glacier flow that is closely linked to ice mass loss and providing new insight into the mechanisms governing glacier dynamics in the Zaskar Himalaya under ongoing climatic change.

L185-190 I'd merge objectives 3 and 4 which cover the same thing. In fact, objective 2 is a rather small task that ties in with 3 so I might just break it down into (1) and merged (3&4).

Thanks for this suggestion. We have updated the objectives as:

- 1. Characterise the long-term (1992-2023) glacier surface velocity trend for selected glaciers in the Zaskar basin, Western Ladakh and evaluate the glacier surface elevation change using the existing dataset by Hugonnet et al., (2021).*
- 2. Investigate the factors influencing glacier velocity evolution, including changes in surface elevation, glacier morphology and extent, and assess whether glacier velocity is linked to a single climatic factor or a combination of interacting factors.*

L440 (Figure 2)

This figure is great and addition of the vectors provides much improved confidence in the results (and similarly for the Sup Mat figure that complements this). Overall the results here look convincing. The areas where this breaks down provide you with some information that might be worth highlighting in the limitations/uncertainty section later. See a few places I've broken out in the screenshot below: 1 and 2 are upstream accumulation areas with vectors that are clearly wrong (sideways/upslope) – the data here is therefore not reliable. Same with 3 on smaller tributaries (likely snow covered/slow/few features to track). The fact that the vectors are reliably downstream and internally coherent for the main glacier trunks provides you something you can point at for the reliability of the data. I'll let you decide where best to highlight this, but it tends to be valuable to show for readers who are non-experts in the technique.

Thanks for the suggestion. Even though we highlighted the limitations of the optical image correlation in the Limitations section. We have now updated the section with more details on the spatially varying reliability of the data and artefacts. This is how the updated version reads:

“In addition, erroneous velocity due to the presence of cloud cover, shadows, or snow may occur. For example, spatially, velocity retrieval was found to be less reliable in accumulation zones and small tributaries for some years, where low surface texture and seasonal snow cover lead to noisy or non-physical velocity vectors. In contrast, the main glacier trunks show internally coherent and consistently downstream flow, supporting confidence in the primary velocity patterns while highlighting regions of reduced reliability”