Author's Response Letter

We would like to 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. The final section discusses the changes made to the supplementary document.

Response from Authors to Anonymous Reviewer 1 (RC1)

Summary:

Ghosh et al. (2025) present a comprehensive analysis of long-term glacier surface velocity and elevation changes in the Zanskar Basin, Ladakh Himalaya. Utilizing satellite remote sensing data, primarily from Landsat imagery processed via COSI-Corr for velocity estimation and reanalyzed elevation data from Hugonnet et al. (2021), the study examines 12 selected glaciers to characterize trends in flow deceleration and thinning. The main findings indicate an overall velocity slowdown of -2.4 m yr^{-1} $decade^{-1}$, with thinning accelerating from -0.22 m yr^{-1} (2000–2005) to -0.57 m yr^{-1} (2015–2020). The authors attribute these changes to climate-driven mass loss, which reduces gravitational driving stress and is modulated by glacier geometry, debris cover, and terminus type (e.g., lake-terminating glaciers exhibit localised accelerations). However, weaknesses were observed in inconsistent terminology, insufficient methodological details, and potential biases in ERA5 climatic data. The use of median rather than mean values for velocity and elevation changes may hinder comparisons with prior studies that use mean velocity and elevation changes. The discussion could be enhanced by incorporating regional contrasts, such as the accelerating surrounding Karakoram glaciers (Heid and Kääb, 2012), to better contextualise the slowdown of Zanskar's glaciers. Please see my following suggestions. I hope this will help improve the text.

We thank the Anonymous Reviewer for reviewing and providing thoughtful and constructive feedback on our manuscript. We also appreciate the time and effort for the same. We 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-

Note: The changes made in the manuscript are presented here in *italicised* text

Line-by-Line comments

L9: Several sentences include the term 'Glacier' twice or three times (L9, L16-19, L20, L23, L24, L40-41). For example, in this study, we have selected 12 glaciers as test glaciers to study the evolution of inter-annual glacier surface velocity and glacier surface elevation change (L110-111). This sentence contains the word 'glacier' four times.

Thanks for pointing this out. The sentences were modified, accounted for in the suggested lines as well as throughout the manuscript. For example:

Previously: "Trends in glacier surface velocity provide insight into the response of glaciers to climate change as well as local drivers of ice dynamics" modified to: *"Trends in surface velocity provide insight into glacier response to climate change as well as local drivers of ice dynamics."*

L11: Remote Sensing provides a tool; what type of tool is it? You may mention the image correlation here.
Modified as suggested. The sentence now reads-

"Remote sensing-based 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"

L15: After point 1, a digit is sufficient, for example, -2.4 instead of -2.43 throughout the manuscript. Also, what does an uncertainty ± represent?

Thanks for this suggestion. Changes were made across the manuscript.
The uncertainty reported here is the 1σ standard deviation of the data.

L15-16: Somewhere written year-1 and somewhere written yr-1. Please maintain consistency throughout the entire manuscript.

Thanks for pointing this out. The unit representation has been changed wherever required to maintain the consistency. We now use $year^{-1}$ throughout.

L16: Between 2000 and 2005, instead of 2000-2005. Please maintain consistency throughout the entire manuscript.

Thanks for the suggestion. We have made all the changes.

L28-29: Please use the word 'glacierized' instead of 'glaciated', as the latter word implies a former glacier area that has deglaciated now. Please check throughout the manuscript.

Thanks for pointing this out. We have updated this terminology where required.

L35: You may replace 'impacted' with 'reduced'.

Changes made as suggested.

L38: Weertman (1957) is an outdated reference; you may replace it with more recent references.

Thanks for noting this. We updated and added Minchew and Joughin, (2020) to the citation. However, as Weertman (1957) is the classic and foundational study on glacier sliding, and we keep this as well.

L44: First mention the complete form of DGNSS and then write its acronym.

Thanks for this suggestion. We have made the necessary changes. This section now reads-

"For example, Differential Global Navigation Satellite Systems (DGNSS) can be used to track the position of ground stakes on a glacier over time, from which ice surface velocity can be estimated (Azam et al., 2012; Sugiyama et al., 2013)"

L45: Surface velocity can be estimated; a reference is needed.

Thanks for pointing this out. We have added- Azam et al., (2012) and Sugiyama et al., (2013)

L48: Two times mention 'particular' and 'particularly' in a single sentence.

Thanks for noting this. We have updated it, and now it reads-

"....characterise the evolution of a glacier system, particularly mountain glaciers, which are remote and difficult to access."

L49-50: Satellite-based remote sensing methods. Please describe the specific techniques here.

Thanks for this suggestion. We have reworded the sentence as-

".....satellite-based remote sensing methods, such as feature tracking, provide.."

L58: Please describe what other local conditions are.

Thanks for the suggestion. We have updated the sentence as

"....other local conditions such as ice thickness and basal conditions"

L73: The three-year mass balance of Rulung Glacier and the four-year mass balance of Stok Glacier do not present a declining trend. To understand the trend in mass balance, we require a long series of in situ measurements. You may mention here the negative mass balance only.

Thanks for noting this. We have updated this section based on this comment and the comment from Reviewer 2. It now reads-

"Various in-situ mass balance measurements suggest these glaciers are losing mass (Azam et al., 2025; Mehta et al., 2021; Shrivastava et al., 1999; Soheb et al., 2020). While these values quantify ice loss, their implications for regional water security are governed by the timing of 'peak water'- the tipping point where annual meltwater discharge reaches its maximum before declining due to reduced glacier volume (Huss and Hock, 2018)."

L75-76: Correct the reference of Soheb et al. (2020) and Mehta et al. (2021).

Thanks for pointing this out. Updated as suggested.

L93: Omit Western Himalaya.

Updated as suggested.

L97: Test……………………..reframe this sentence.

We have removed this objective, and included it in the third objective and reworded as-

"Evaluate the factors influencing glacier velocity, including changes in surface elevation, morphology and extent"

L110: Why were selected 12 glaciers? Please give the justification. What is the range of area and elevation of these glaciers? Please add 2-3 sentences on this.

Thanks for this suggestion. We have updated the section as-

"In this study, we have selected 12 glaciers to study the evolution of inter-annual glacier surface velocity and surface elevation change. We selected the glaciers to capture the morphological and glaciological diversity in the region based on their size, elevation, degree of debris cover, terminus type, orientation, and slope. From small mountain glaciers (G12; 3km^2) to large valley glaciers (DDG;68 km^2) and a mean elevation ranging from 4800m to 5400m (Table 1). The glaciers are given unique IDs based on their known name (DDG-Drang Drung Glacier, HG- Hagshu Glacier); the remainder are denoted G3 to G12 (shown in figure 1). Refer to Table 1 for further details"

L139: Green (B2 band), why was the Green band? Please explain here.

Thanks for noting this. We clarify this by rewording the sentence as-

"In this study, we used the panchromatic band (B8) of the Landsat series 7 ***Enhanced Thematic Mapper Plus (****ETM+), Landsat 8 and 9 Operational Land Imager (OLI) and the Green (B2) band from Landsat 5 Thematic Mapper (TM), which is closest to the wavelength range of the panchromatic band"*

L158: There is some confusion. Did you co-register the Landsat images, as these images are already orthorectified? If not performed, please omit this.

Thanks for pointing this out. We did not co-register images. It was written about the capabilities of the tool. However, we have omitted this sentence for clarity.

L162: What is the "salt-and-pepper" effect?

By "salt-and-pepper" effect, we meant to signify the noisy pixels within the velocity raster. We have removed the term as it was redundant with the preceding word 'noise'.

L166: 4*4 pixels, but Landsat 5 does not contain a panchromatic band. Possibly you used 2*2.

Thanks for pointing this out. Yes, Landsat 5 does not contain panchromatic band and we used 2*2pixel . We have added the information and updated it as-

*"A step size (refers to the spatial resolution or the distance between the centres of the image patches) of 4*4 pixels for all except for Landsat 5 (30m spatial resolution), where a 2*2 pixel step size was set, resulting in a resultant output of 60m spatial resolution velocity fields"*

L176-178: I feel five iterations are best, whereas two iterations may give some noise. Please carefully check and rewrite.

Thanks for noting this. In this study, we used iterations of 4. We have updated the text as-

"The Robustness iteration, which is a quality control loop that ensures the derived displacement or velocity vectors are consistent and robust against errors or noise, was set to 4"

L204: It may also arise due to a horizontal shift in two images; you may mention this here.

Thanks for suggesting this. We have updated the sentence by adding this. It now reads-

"For example, it may arise due to a change in surface characteristics between the two images, such as snow cover change, varying sun angles and shadows, and also due to a horizontal shift in the two images"

L213: Please provide more details on pairwise uncertainty in the supplementary material.

Thanks for the suggestion. In the supplementary, we have already reported the pair-wise uncertainty of all the velocity results under section Table S2. However, it was not referred in the main text. We have now added it and modified to-

"This method was applied to all the velocity datasets generated. The uncertainties range from 0.85 m year^{-1} to 5.49 m year $^{-1}$, which are of similar magnitude to previous studies (Bhushan et al., 2018; Das and Sharma, 2021; Shukla and Garg, 2020). All the pair-wise uncertainty is listed under Supplementary Table S2."

L221-223: Please reframe this sentence, and replace glacier mass balance with elevation changes.

We have updated as suggested. It now reads-

"Bin-wise elevation change analysis is an effective approach for capturing the spatial heterogeneity of glacier responses to climate forcing, revealing how processes operating at different elevations shape overall surface elevation change and dynamics."

L229: Equation 5, how are these parameters calculated in this equation? What is the uncertainty in these estimates? Please describe in the text. I also could not find how this equation helps to interpret the result of glacier surface flow velocity in the manuscript.

Thanks for noting this. However, we did not explicitly calculate the parameters in equation 5. This was provided for reference to the readers to show the relationship between driving stress with gravity, ice thickness and slope (Cuffey and Paterson, 2010). As we refer to the concept of driving stress in subsequent sections, we believe this might help the readers to understand how we draw our conclusions.

L242: ERA5 Land reanalysis climate data: Many studies have reported biases in the ERA5 land reanalysis dataset in mountainous areas. Our own study found a 5- to 8-degree temperature difference between the automatic weather station and the ERA5 Land dataset in different seasons, without bias correction, in the Central Himalaya. Such climatic trend analysis without bias corrections is not appropriate and provides unrealistic trends. I would suggest conducting bias correction in ERA5 data using nearby IMD sites or the IMD grid dataset.

Thanks for pointing this out and the suggestion. We agree with the reviewer's comment that ERA5-Land reanalyses can contain significant biases in mountainous regions. Since we do not perform any quantitative meteorological analysis or use it as a forcing data and restrict to only trend analysis, we believe using raw ERA5-Land is suitable for the present study. Moreover, the study by Mandal et al., (2024) has shown that ERA5-Land is fairly reliable for the same region by comparing the raw data using statistical assessment with nearby station datasets (see figure 2). Given that we do not use the data to drive models, we do not think it would add any value to bias correct using the IMD (or other) datasets.

L247: Section 4.1, Glacier velocity trend and surface elevation change: Both sections must be presented separately. Also, section 4.3 "elevation-wise glacier velocity trend is actually section 4.2 (possibly typo mistake), which can be merged with section 4.1 (glacier velocity trends) following the removal of surface elevation change.

Thanks for the suggestion. We have modified the sections and updated the indexing. They are now in the series of-

Section 4.1- Glacier Velocity Trends
Section 4.2- Glacier Surface Elevation Change
Section 4.3- Non-climatic factors
Section 4.4- Climatic Trends

L257-258: In the result and discussion section for surface flow velocity and elevation changes, the median flow velocity term and median elevation change term are frequently used. However, most of the previous studies use mean or average surface flow velocity or mean elevation change values. It is not straightforward to compare the median surface flow velocity with the previously published mean surface flow velocity.

Thanks for noting this. Upon receiving this feedback, we conducted a comprehensive audit of our data processing scripts and analysis. We discovered a terminological inconsistency in the original draft: while our calculations were correctly outputting the mean, the text and figure labels erroneously identified these values as medians. We have corrected all text, figure captions, and label inconsistencies related to this.

L260: Use 24.5 ±5.7 instead of 24.50±5.73 throughout the manuscript.

Thanks for the suggestion. We have updated this throughout the manuscript.

L263: T-statistic test, please describe this statistical test in detail in the methodology section for the interest of readers.

Thanks for the suggestion. We have added and described the t-test under section 3.4 Data analysis and supporting datasets. It reads-

"A T-statistic test that compares the means of two groups while considering how much the data "spreads" (variance) was used to calculate the p-value in our data analysis (Kim, 2015). The p-value, derived from this statistic, represents the probability of observing results as extreme as the sample, assuming the null hypothesis. A p-value below 0.05 generally indicates statistically significant results (rejecting the null hypothesis)."

L281: Between 2001 and 2010, instead of between 2001-2010. Please carefully review the entire manuscript for any similar text.

Thanks for the suggestion. We have made all the changes as required throughout the manuscript.

L283: From 1992 to 2000 instead of from 1992-2000.

Thanks for the suggestion. We have update throughout the manuscript.

L296-300: This is related to methodology, not related to the result section. Please shift to the relevant section and write only the results here.

Thanks for pointing this out and the suggestion. We have shifted this section to Methodology under section 3.4 Data analysis and supporting datasets.

L326: omit theory.

We have updated as suggested.

L326: median glacier surface elevation change. Primarily, studies use mean glacier elevation changes. Please see my previous comment.

Please see the above point.

L327: What are bottom-heavy glaciers? First, define these terms.

Bottom-heavy glaciers feature a disproportionately large area at lower elevations (Jiskoot et al., 2009). We have defined these terms in Methodology under section 3.4 Data analysis and supporting datasets for reference to readers. The modified text reads-

"Additionally, we used the Hypsometric Index to calculate and classify the glaciers as very bottom-heavy ($HI > 1.5$), bottom-heavy geometries ($1.2 < HI \leq 1.5$), very top-heavy ($HI < -1.5$), top-heavy ($-1.5 < HI < -1.2$), and equidimensional glaciers ($-1.2 < HI < 1.2$) following Jiskoot et al., (2009). While top-heavy glaciers concentrate their mass at higher elevations within expansive accumulation zones, bottom-heavy glaciers feature a disproportionately large area at lower elevations."

L355; Figure 8: In sub-panel A, also show Drun Drung glacier in the subset, as shown in sub-figure b.

Thanks for the suggestion. We have updated the figure with inset in panel a.

L380; Figure 9: In this figure, you mentioned median velocity and mean elevation, whereas in text L326, you mentioned median glacier surface elevation change. Please correct it throughout the manuscript.

Thanks for noting this. However, in the figure, the 'mean elevation' is the elevation from the DEM and not the surface elevation change. Whereas, in L326, we discuss the results from surface elevation change analysis from Huggonet's data.

L386; 4.4 Climatic trends: Please see my previous comment on the ERA5 Land dataset without Bias correction, such trends are not appropriate. Additionally, please describe how the calculated uncertainty of the ERA5 land dataset is ±0.01 °C in the methodology.

Thanks for pointing this out. While the absolute values of meteorological variables such as temperature and precipitation require bias correction to be reliable, the trends (which are more a function of the large-scale circulation and its changes through time) are appropriate. For the second part of the comment, we have updated it in the methodology. It now reads-

"Data from the nearest grid to the study location were extracted for further analysis. The uncertainty was estimated based on the 1σ (standard deviation) of the data."

L396: The discussion section can also be improved. Please see the following papers, which can help enhance the discussion section, particularly regarding the increase in lake-terminating glacier flow velocity and its comparison with the Karakoram region.

Thanks for the suggestion. We have incorporated these studies and modified the discussion sections as follows-

For overall discussion on glacier velocity and elevation change-

"…….Although previous studies have reported a slowdown of glaciers in the Ladakh region, a contrasting increase in glacier velocity was found in the Drang Drung Glacier. For example, the results from the study by (Singh Jasrotia et al., 2024) show velocity increased from 71 ± 6.1 m year^{-1} in 1999 and 2000 to 140 ± 7.4 m year^{-1} in 2019 and 2020 (by ~ 50 %), which contrasts with our findings, and is likely an artefact of their consideration of two timeframes only. Overall, our declining glacier velocity trends align with patterns observed for nearby glaciers in other studies. However, our results contrast with recorded flow acceleration of Karakoram glaciers, reported by Heid and Kääb, (2012), which was due to positive mass balance, and also glacier surging in this region."

For discussion on lake-terminating glaciers-

"… Such lake expansion could alter the local force balance near the termini, promoting frontal acceleration (Pronk et al., 2021). A recent study on Bhutan lake-terminating glaciers found, despite high thinning rates, some glaciers showed acceleration near the terminus, which is potentially due to increased meltwater flux (Hyde et al., 2025). A similar conclusion was drawn by Wu et al., (2020) for a lake-terminating glacier in Kangri Karpo Mountains in South-Eastern Tibet."

Response from Authors to Reviewer 2 (RC2)

Summary:

Review of – Slowdown of glacier velocity emerging in the Zanskar Himalaya
By Tirthankar Ghosh, RAAJ Ramsankaran, Felicity S McCormack, Andrew N Mackintosh
In this manuscript, the authors quantify multi-decadal change in glacier dynamics in the Zanskar Basin (Ladakh Himalaya) by estimating interannual surface velocities from Landsat feature tracking (1992–2023) and relating these to surface elevation change from the Hugonnet et al. dataset. The authors analyse 12 selected glaciers spanning a range of sizes, slopes, aspects, debris cover fractions, and terminus types. The manuscript is well written overall and complements other available papers on the region. However, some revisions would be useful to improve the final version of this. Firstly, given that most of the results in this paper are based on the optical feature tracking velocity maps, it would be ideal to either make these rasters available directly or at a minimum add some vector plots to the supplementary material. The absence of this makes evaluating underlying data quality more challenging. The discussion of the processes driving glacier acceleration and slowdown could use some edits, particularly capturing the more complex links to glacial hydrology that are currently absent. Also, the discussion of non-climatic drivers (including lake impacts) has some weaknesses in the causality inferences, with differences in glacier size/thickness not being discussed. None of these revisions will fundamentally alter the manuscript and I expect it to be suitable for publication in TC with appropriate edits.

We thank Dr Maximillian Van Wyk de Vries 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-

Note: The changes made in the manuscript are presented here in *italicised* text.

Line by line comments:

The title could be clearer – there's some ambiguity around what 'emerging' means

Thanks for the suggestion. We have changed the title to *"Thinning-induced glacier deceleration in the Zanskar Himalayas"*

L13 – a brief word about why they were selected – 'representative'? 'distrubuted'? 'large'?might help

Thanks for the suggestion. We have updated the sentence as:
"…12 selected glaciers with varied morphological characteristics in the Zanskar Basin of the Ladakh Himalayas"

L20 'glacier health' avoid this term. Spell out what you mean. Also you just noted acceleration of some lake termini – would this not be an acceleration in the face of climate change? Maybe caveat to capture the complexity.

Thanks for this suggestion. Yes, we agree to the argument and have updated the text. The sentence now reads:

"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. For example, some lake-terminating glaciers exhibited local increases in ice velocity near their termini. Overall, our results confirm loss of ice mass in this region, which generally corresponds with a reduction in surface velocity."

L25 Perhaps note here these figures exclude the ice sheets. If we include Antarctica/Greenland they are much lower.

Thanks for the suggestion. We have updated as suggested. It now reads-

"Globally, glacier mass outside of Greenland and Antarctic ice sheets is projected to decline by 26 ± 6% to 41 ± 11% by the end of the 21st century (relative to 2015), under emission scenarios that correspond to +1.5 °C and 4 °C warming, respectively (Rounce et al., 2023)."

L28 glaciated ice -> ice
Updated as suggested.

L32 'caters to a population of over a billion people' while >1 billion live in the catchments, the glacial water fraction is negligible for many of these. As written this somewhat oversells the importance of glacial water (which is nevertheless locally crucial). Please reword to better capture this.

Thanks for noting this. We have updated and reworded this section. It now reads:

"For example, the meltwater generated from Himalayan glaciers and snow modulates the discharge of major river systems such as the Indus, Ganges, and Brahmaputra. While the glacial contribution to total runoff varies across the catchments, it underpins the water security of upstream populations and the broader hydrological systems upon which over a billion people downstream depend, recharges river-fed aquifers, and contributes to global sea level rise"

L35-40 Glacier basal hydrology is of course a critical driver of velocity which can substantially complicate this relationship. Many glaciers undergo a large seasonal cycle in velocities which of course does not reflect equivalent seasonal mass changes. Can you please say a little more about this here, and how it can be mitigated when interpreting changes in velocity (e.g. long timeseries, multi-glacier analyses, context-aware). You do this all already so this can strengthen your case.

Thanks for this suggestion. Yes, we agree that glacier basal hydrology has an important role in velocity and needs to be mentioned here. We have modified this section to include the subglacial hydrology. This is how it reads:

"As glaciers flow due to their weight and gravity, by the processes of internal deformation and sliding at the bed, basal hydrology can substantially complicate this relationship (Bindschadler, 1983; Minchew and Joughin, 2020). Variations in meltwater supply to the bed modify the basal water pressure, which in turn regulates basal sliding (Bindschadler, 1983). As a result, many mountain glaciers exhibit pronounced seasonal velocity cycles, with speed-

ups during periods of enhanced surface meltwater input to the bed. These short-term hydrologically driven fluctuations can obscure longer-term dynamic trends if not carefully accounted for. Such effects can be mitigated using long time series spanning multiple decades, which average out seasonal variability and highlight persistent interannual to decadal trends. Furthermore, glacier-specific, process-based understanding can also help better interpret these results. These flow processes are also influenced by other internal factors, such as ice temperature, glacier geometry, bed characteristics, as well as external factors such as air temperature and precipitation (Cuffey and Paterson, 2010; Iken and Bindschadler, 1986)"

L43-45 Worth noting here that people have been doing satellite-based feature displacement tracking for ice velocity basically as long as GNSS has been around.

Thanks for noting this. We have updated the section. This is how it reads-

"Historically, glacier velocity has been monitored through the simultaneous development of on-field and satellite remote sensing-based measurements, both of which are established standards in glaciology (Hooke et al., 1989; Stevens et al., 2023; Vincent et al., 2022)"

L50 Satellites do not provide higher temporal resolution than a ground-based GNSS station. You can get sub-minute timings from the latter. This paper may be of interest to review – we compare satellite and ground based data (https://doi.org/10.5194/tc-17-4063-2023).

Thanks for noting this. We have updated it as suggested. It now reads-

"By contrast, satellite-based remote sensing methods, such as feature tracking, provide a wide spatial coverage and an easy-to-access dataset."

L55 Agreed that they are interesting but perhaps can be worded differently here. Agarwal et al might be relevant for this section too – a detailed regional analysis with some similar objectives to your study https://doi.org/10.1016/j.scitotenv.2023.165598.

Thanks for this suggestion. We have reworded this section and now include Agarwal et al., (2023). This is how it reads-

With the availability of more satellite datasets, many ice velocity studies have been carried out in the Himalayas, providing critical insights into glacier spatio-temporal patterns and evolution. Several studies have revealed heterogeneous patterns in the velocity of the Himalayan glaciers (Bhambri et al., 2011; Dehecq et al., 2019; Garg et al., 2025; Tripathi et al., 2023). Findings from these studies show that glacier velocity varies spatially and temporally, region-wise and within the same glacier, depending on factors such as elevation, slope, size, debris cover fraction, land vs lake terminating, mass budget and other local conditions such as ice thickness and basal conditions (Agarwal et al., 2023; Bhushan et al., 2018; Dehecq et al., 2019).

L70-75 Could you speak on the concept of peak water here? This matters more for water security than the 'glacier mass loss' values – if peak water is still in the future then greater mass loss might actually lead to a short term increase in water availability. The timescale of interest matters here. Huss and Hock have a global compilation if there are no specific local studies.

Thanks for this suggestion. We agree that discussing the concept of peak water gives another perspective in this section. We have modified this section as:

"The implications for regional water security from this ice loss are governed by the timing of 'peak water'- the tipping point where annual meltwater discharge reaches its maximum before declining due to reduced glacier volume (Huss and Hock, 2018). With peak water for the region projected to occur around ~2028, as reported by Huss and Hock, (2018), the current acceleration in mass loss represents a critical transition from a short-term increase in water availability to a long-term reduction of streamflow, making this region extremely vulnerable to water shortages."

L86 These are some good studies overall, and you might rewrite this sentence in a more positive way e.g.'despite the insight these provide into xx, gaps in our understanding of yy remain'.

Thanks for the suggestion. We have reworded this to:

"Despite the insights these studies provide into discrete periods of glacier surface velocity, gaps remain in our understanding of continuous, long-term velocity evolution and thinning. Thus, a comprehensive analysis of the glacier dynamics in the Ladakh region is still missing."

L87 Does Dehecq et al not cover the whole HMA including this area?
Yes, Dehecq et al do cover this region. But it lacks a detailed glacier-wise analysis.

L92 'glacier flow trend' -> 'glacier surface velocity trend'
Updated as suggested.

L94 What 'reanalysis' was done here?
In this objective, by "reanalysis" we meant using the dataset to perform data extraction and do statistical analysis for our region of interest. However, we have reworded this objective to-

"Extract and evaluate the glacier surface elevation change using the existing dataset by Hugonnet et al., (2021) for the selected glaciers."

L96 This seems a risky aim as worded, as exact determination of these processes is not always possible, particularly with only this remote sensing data.

Thanks for noting this, and we agree to that. We have reworded the objective to-

"Investigate the factors influencing glacier velocity, including changes in surface elevation, morphology and extent"

L97 This is not clear. Do you mean whether temperature vs precipitation are the key drivers? Can you expand this question to make it clearer.

Thanks for the suggestion. We have included this objective in our 3rd objective, as we feel it was redundant.

L139 Can you say exactly which Landsat mission you got images from. Were you forced to use post-SLC failure L7?

Thanks for this suggestion. The Landsat missions used to collect images were already reported in this section. We used the Landsat 5, 7, 8 and 9. Though we have updated this section with the detailed sensor names (e.g, TM, ETM+, etc).
We did not use any post-SLC failure images from Landsat 7. (see supplementary for details of the images used)

L153 Did you just do sequential image matching across years (e.g. 2015-2016)? Or was multiyear matching also done (e.g. 2015-2017, 2018, 2019). For these v. slow glaciers with limited decorrelation the latter can be useful and reduces dependence on single images but unsure what was done here.

In this study, we performed a sequential matching. We agree, multi-year matching can increase displacement magnitude and improve signal-to-noise ratios, particularly for very slow glaciers, but it inherently averages velocity over longer time intervals. Furthermore, the glaciers analysed here exhibit measurable displacements over annual timescales that remain above the detection threshold of the correlation method, and the algorithm performed well in capturing them.

L171 Could you briefly note how the SNR is calculated or reference a paper that does. This is calculated in a few different ways.

Thanks for this suggestion. We used the method as proposed in Leprince et al., (2007) incorporated in COSI-Corr algorithm. We have updated this section by referring to (Leprince et al., 2007).

L191 – 194 If using a median smoothing kernel then "smooth the velocity outputs without losing their details" is probably not true, rather "smooth with acceptable loss of detail". Worth noting that this procedure commonly erodes the glacier boundary by 1-2 pixels with the 'stationary' pixels winning out on the median. Probably still acceptable but worth noting especially for small glaciers.

Thanks for this suggestion. We agree to what was discussed above. We have modified this section to-
*"Finally, we used a 3-pixel * 3-pixels median filter on the velocity maps to remove any outliers (noise) within the maps and to smooth the velocity outputs with an acceptable loss of details"*

L220-225 This is described as a more information rich-approach than 'whole glacier averages' – but the baseline Hugonnet dataset is already a spatially distributed dataset so this is losing information. Might be worth instead framing it in terms of noise suppression and interpretability (i.e. dimensionality reduction).

Thanks for noting this and the suggestion. We have updated this sentence to-

"Bin-wise elevation change analysis is an effective approach for capturing the spatial heterogeneity of glacier responses to climate forcing, revealing how processes operating at different elevations shape overall surface elevation change and dynamics. Aggregating the data into elevation bins acts as a form of suppressing spatial noise while preserving the dominant elevation-dependent signals for better interpretations"

L225-226 this is only true for 'all else equal' – as your subsequent lake analyses show is not always true.

Thanks for noting this. Previously, we have already noted some exceptions here. However, we have reworded this section for better clarity. It now reads-

"However, there could be some exceptions, for example, differential surface thinning rate, leading to increased surface slope, may increase the driving stress or some complex feedback mechanisms from proglacial lake-ice interaction"

L247 onwards – I am not sure that the velocities reported to the nearest 0.01m are warranted here.
Modified as suggested throughout the manuscript.

L284 Looking at the graphs for G5 and G11 that doesn't seem to tell the whole story.
Thanks for noting this. We have removed this part. This section was based on the analysis done for the time ranges as written in the manuscript (1992-2000, 2001-2010, 2011-2023). The relative higher velocity in some years corresponded to overall positive trend in specific decadal ranges for few glaciers, leading to misinterpretation of velocity evolution. The modified section now reads-

"We considered three decades from our study period (1992-2000, 2001-2010, 2011-2023) to understand decadal flow pattern changes. Despite the data gaps between 2001 and 2010, we can discern a broad understanding of changes in the flow trend over this 31-year period. From 1992 to 2000, all glaciers exhibited an increase in velocity. In contrast, from 2001 to 2010 and 2011 to 2023, the glaciers generally show reduced flow speeds"

L289 Is 5000m exactly the ELA on all these glaciers? If not, can you either repeat this with the actual ELA or reword?
Thanks for noting this. Here, 5000m threshold was selected not to signify the ELA of the glaciers, but rather based on the mean elevation distribution of them. For more clarity, we have reworded this sentence to-

"To analyze the relationship between ice velocity trend and elevation, we categorized the glaciers into two elevational zones based on the regional mean elevation: a lower-elevation zone (< 5000m) and an upper-elevation zone (> 5000m). It is important to note that this 5000m threshold was chosen to represent the average elevational distribution of the glaciers in our study area and is not intended to represent the Equilibrium Line Altitude (ELA)."

L315 Could you plot the annual velocities as lines rather than scatterplot? It is very hard to keep track of. If the lines are colored with a continuous gradient from start-end it becomes easier.

While plotting the velocity profile as lines (as suggested), we found that since there are so many data points from different years, the figure becomes quite haphazard and less clear to read. We therefore still use a scatterplot, but each year is represented in a continuous gradient colourmap for better readability and clarity.

L375 To me this reads as all covariates of ice thickness – area and length are themselves strongly correlated (0.97) and both will relate to how large and thick the ice is. Slope anticorrelating with velocity is contrary to usual expectations, but should also be because slope and thickness anticorrelated.

Thanks for noting this. We agree with the reviewer's interpretation of the physical drivers. We have revised the text to explicitly mention the high correlation (0.97) between area and length as redundant thickness proxies. We also added a clarification explaining that the negative correlation between slope and velocity likely stems from the anti-correlation between slope and ice thickness, where thickness remains the dominant control on velocity in this dataset. This is how it reads-

"The matrix indicates that glacier length and area have the strongest positive correlations with mean velocity, with a correlation coefficient of 0.75 and 0.67, respectively, *which could be related to how large and thick the ice is. In contrast, slope (r = -0.37) and aspect (r = -0.36) shows a moderate negative correlation with velocity. While steeper slopes generally increase gravitational driving stress, the negative correlation observed here likely reflects the inverse relationship between slope and ice thickness; in this study area, steeper glaciers could be thinner, and the reduction in thickness exerts a stronger limiting influence on velocity than the slope gradient provides."*

L426-432 This would be a good place to say more about the subglacial hydrology links to velocity which is currently missing.

Thanks for the suggestion. We have now incorporated discussions on subglacial hydrology and its potential links with glacier accelerations. This is how it reads-

"For G4 and G5 (top-heavy glaciers) and DDG (equidimensional hypsometry), sustained ice flux from upstream likely contributes to maintaining flow toward lower elevations. However, the dynamic behaviour of these lower ablation zones cannot be explained by geometry alone. Enhanced surface melt in recent years likely increases meltwater inputs to the glacier bed through moulins or crevasses, promoting periods of elevated basal water pressure and reducing effective pressure at the ice-bed interface, which can lead to higher basal sliding. HG, a heavily debris-covered glacier with a bottom-heavy hypsometry, which is likely more sensitive to such hydrological controls near ablation zone. While debris can modulate surface melt spatially, meltwater routed through crevasses, ice cliffs, and supraglacial ponds can still reach the bed and locally enhance sliding. Bhushan et al. (2018) also reported a dynamically active trunk of DDG (>20 m year^{-1} in 2013–2014), consistent with our mean flowline velocities. This highlights the complex interplay between glacier geometry, debris, subglacial hydrology evolution, and dynamics."

L439-441 In isolation I am not sure how much this tells us – they could simply be larger/thicker glaciers as this has not been controlled for.

Thanks for noting this here. We agree to the fact that it could simply be related to the geometry of the glacier. However, we have modified this section by discussing these potential biases in result interpretation based on the current datasets.

"Glacier velocities across the study region show clear differences between lake-terminating and land-terminating glaciers, with the former generally maintaining higher surface flow speeds – often up to twice that of their land-terminating counterpart. For instance, mean velocity along the central flowline for DDG and G10 corresponds to 57.98 m year^{-1} and 38.52 m year^{-1}, respectively, which is significantly higher than other glaciers. While glacier size and thickness have controls on driving stress and could partly explain these higher velocities, the dynamic influence of proglacial lakes likely provides an additional control on them."

Also, in second paragraph, we modified the text to-

"The contrasting behaviour of lake-terminating glaciers may therefore be explained in two ways: (a) less buttressing and altered local force balance at the ice-lake margin, and (b) enhanced surface lowering near the terminus. The latter, in particular, influences the flow characteristics by regulating the glacier's dynamic thinning phenomenon, as also discussed and found in the majority of lake-terminating Himalayan glacier studies (King et al., 2019; Pronk et al., 2021). However, glacier geometry is equally important in this regard, as longer and thicker glaciers inherently sustain greater driving stresses, enabling higher ice flow velocities irrespective of terminus conditions. However, disentangling the relative influence of these geometric controls from lake-driven dynamic processes remains beyond the scope of the present dataset, as the observations do not directly constrain either factor. This limitation warrants further investigation."

L447 Not sure you have discussed/shown evidence for dynamic thinning here.

Thanks for noting this. We have reworded the sentence, as we explicitly do not show evidence of dynamic thinning. It now reads-

"This thinning and retreat could be related to calving, which was evidenced in 2023"

L460 The high velocity is not particularly apparent in these figures. Here and throughout it would be good to add the velocity vectors on top of the speed field, this is helpful for evaluating data quality. Also, could you use colour base images – the lakes would be much clearer.

Thanks for the suggestions. We have added the velocity vectors in this map as well as wherever required, and also changed the basemap to a true colour satellite image, with better representation of the lake.

L480 perhaps 'detailed' rather than 'comprehensive'

Modified as suggested.

L485-487 I am not sure what this sentence is getting at. As I mentioned above, I think most of this can be summed up as 'larger glaciers tend to be thicker and flow faster' – not particularly insightful in itself.

Thanks for noting this. However, here we not only intend to talk about 'large glaciers tend to be thicker and flow faster', but also the heterogeneity in spatial and temporal evolution of velocity in similarly sized glaciers within similar climate forcings, which could be linked to the mentioned characteristics of these glaciers (debris cover, hypsometry, subglacial conditions, to some extent ice-lake feedback).

L487-488 Again, the absolute velocity being different does not tell us much unless you have controlled for some other parameters here. If we compare a large lake terminating glacier with a small, thin land terminating glacier we cannot attributed the absolute velocity difference only to the lake.

Thanks for noting this. As we agree to the previous comments on lake-ice mechanisms and causality inference. We have modified this section and reworded to-

"Though lake-terminating glaciers exhibited persistently higher velocities compared to land-terminating ones, consistent with previous findings across the Himalayas, the specific mechanisms driving this difference cannot be conclusively resolved here, given current data constraints"

Changes to Supplementary Document

1. We have added a figure (Supp. Figure S1) with velocity maps from different time periods with overlayed velocity vectors as requested by the reviewer.
2. We have also added a figure (Supp. Figure S2) comparing the velocity from ITS_LIVE and the data generated in this study.
3. We have removed the Supplementary Table S3 (bin-wise elevation data). We will provide this file with other data once the manuscript is accepted.
4. Updated the figure numbers for the other older figures as it was required.

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