Dear Editor and Reviewer,

On behalf of all co-authors, we sincerely thank the editor and the reviewer for the time and effort devoted to reviewing our manuscript entitled “The 2020 abrupt drainage of Jinwuco triggered lake- to land-terminus transition and lagged slowdown of Jinwu Glacier, southeastern Tibet” (MS No.: egusphere-2026-1492).

We have carefully considered all comments and provide our point-by-point responses below. The comments have been very helpful in improving the clarity of the motivation, methodology, interpretation, and presentation of the study.

Major comments

Comment (1) *Equation 3 is incorrect. It should be ice speed (m/yr) = displacement (m) * A (days/yr) / N (days). All ice speed measurements in this study need to be revised. Also, please specify the value of A. Is it fixed at 365.25?*

Response: Thank you for the careful check. This was a typographical error in the equation rather than an error in the actual velocity calculation. We rechecked the ice-flow velocity calculations and confirmed that the conversion was performed using the correct formula, i.e., ice speed = displacement × A / N, where A is fixed at 365 days yr^{-1} and N is the time interval between the two images in days. Therefore, the reported ice-flow velocity values, figures, and related conclusions are not affected. We have corrected Eq. (3) in the revised manuscript and specified the value of A.

Is there really a slowdown?

Comment (2) *Figure 4(a) shows a very noisy velocity signal at 0-200m from the terminus. The ice speed can vary by >40 m/yr over a 50 m distance along the flowline. I have checked the velocity maps provided in the Zenodo submission (Thank you for doing this! I appreciate it, as I think this enables efficient communication between us), and they also show a spatially inconsistent pattern in the 0-200m zone. This speed variation is too large to be physically plausible. Since it exceeds the NAMD of bedrock velocity by more than 10-fold, this variation is very likely due to incorrect matches by the feature-tracking algorithm (Zheng et al., 2023). With the presence of incorrect matches, any trends at the 0-200m zone are unconvincing to me. The validation described in Section 5.2 does not address this issue either, because, according to Figure S6, only 12 points are arbitrarily chosen in the 0-200m zone for comparison, which likely excludes any mismatched pixels, as suggested by the low RMSE relative to the speed variation described above.*

To investigate and reduce the effects of incorrect matches, I suggest using a small step size (e.g., 1x1 or 2x2 pixels) so that incorrect matches can be identified as pixel clusters, which can then be masked during post-processing. For velocity maps that show serious spatial incoherence *(e.g., 2020, 2021, 2022, 2023, and 2024), a possible source of incorrect matches is the drastic change in the terminus area (i.e., decorrelation of the glacier-free surface) due to the continuous decrease in the proglacial lake level. It might be helpful to experiment with different tracking parameters, such as a smaller correlation window (e.g., 30x30 pixels) and a narrower search range (e.g., 20x20 pixels), to reduce the chance of incorrect matches.*

Response: Thank you for the careful inspection and constructive suggestions. Following this suggestion, we first tested several alternative feature-tracking parameter combinations, including smaller step sizes of 1 × 1 and 2 × 2 pixels, a smaller correlation window of 30 × 30 pixels, a narrower search range of 20 × 20 pixels, among others. However, these parameter settings did not effectively remove the abnormal matches. This may be related to the small size of the terminus area, strong local topographic effects, rapid terminus changes, surface decorrelation, and image quality, rather than to the tracking parameters alone. Therefore, we focused on post-processing and independent validation to reduce the influence of incorrect matches.

We reprocessed all velocity maps using a two-step quality-control procedure. First, local extreme outliers were identified and removed using a 3 × 3 moving window. Second, ice-flow velocities were sampled along the glacier centerline at 50 m intervals, and profile points showing differences greater than 15 m a^{-1} from adjacent points were flagged as potential mismatches. These flagged points were then inspected together with the original images and velocity maps, and only points that were clearly inconsistent with the local flow continuity were removed. After this filtering, the abrupt and physically implausible jumps in the original profiles were substantially reduced (Figure 1). Because valid data coverage in the 0–100 m zone remained limited, we revised the near-terminus analysis to focus on the 100–200 m section (**错误!未找到引用源。**), where the data coverage is more complete and the velocity signal is more reliable.

To further assess the reliability of the revised velocity results, we added two independent manual feature-tracking tests. The first test was conducted along the glacier centerline of the glacier tongue, using clearly identifiable and repeatable surface features from the original image pairs. A total of 20 feature displacements covering all annual velocity fields were obtained and compared with the automatic tracking results (Figure 3a). The two datasets show good agreement, with an R^2 of 0.92 and an RMSE of 3.84 m a^{-1} (Figure 3b). The second test focused on a near-terminus 100 m section, where nine independent feature displacements were measured to examine whether the terminus slowdown signal could be reproduced (Figure 3c). The manually measured velocities also agree well with the automatic results, with an R^2 of 0.96 and an RMSE of 2.14 m a^{-1} (Figure 3d).

The revised velocity profiles and the independent manual validation both indicate that the reduced near-terminus velocity is not caused solely by a few mismatched pixels (Figure 3). We acknowledge that surface decorrelation and rapid terminus changes still introduce considerable uncertainty in this region. Therefore, in the revised manuscript, we interpret the near-terminus velocity decrease more cautiously and restrict the quantitative analysis to the better-constrained 100–200 m section.

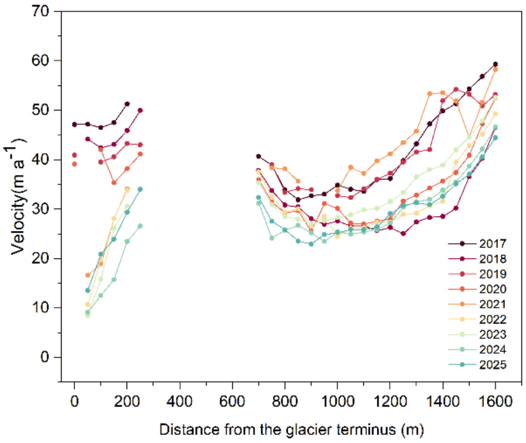

Figure 1 Longitudinal ice-velocity profiles along the centerline of Jinwu Glacier from 2017 to 2025. The x-axis indicates the distance from the glacier terminus. Different colored lines represent annual velocity profiles derived from PlanetScope feature tracking. Velocities were sampled along the glacier centerline at 50 m intervals.

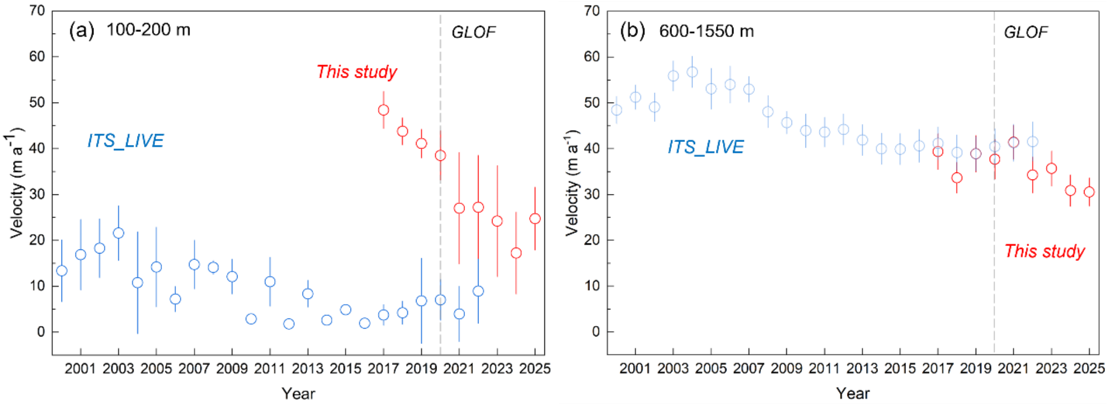

Figure 2 Interannual variability in the mean surface velocity within 100–200 m (c) and 600–1550 m (d) from the glacier terminus.

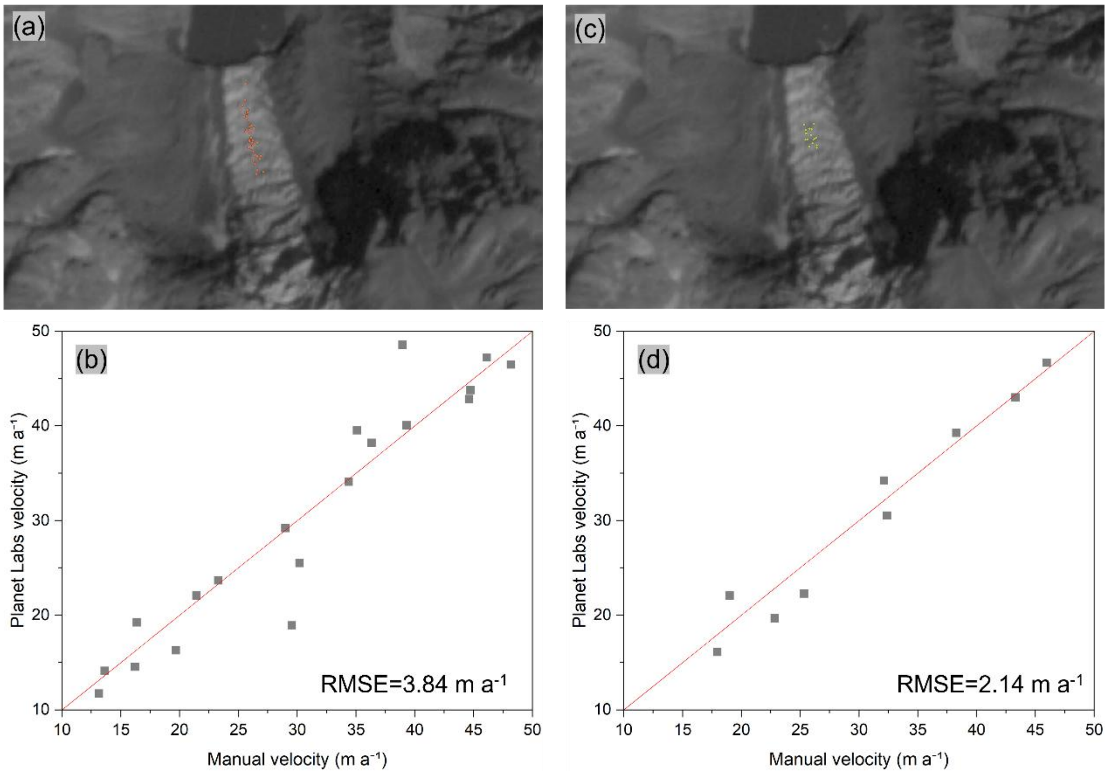

Figure 2 Validation of PlanetScope-derived ice velocities using manual feature tracking. (a, c) Locations of manually tracked surface features on the glacier tongue. (b, d) Comparison between manually measured velocities and PlanetScope-derived velocities.

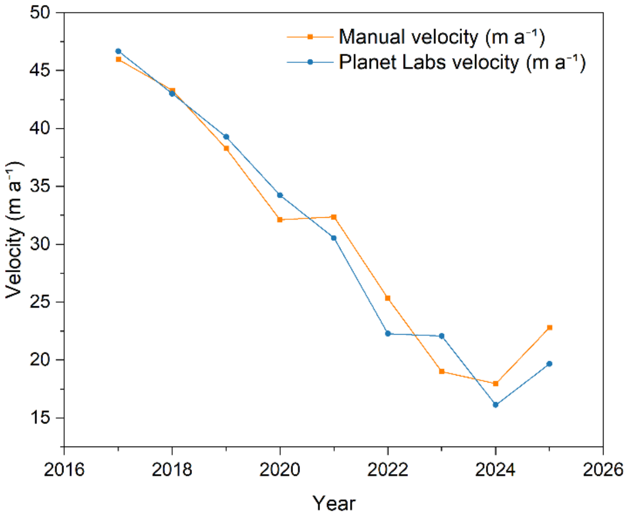

Figure 3 Comparison between manually measured feature velocities and PlanetScope-derived velocities in the near-terminus section of Jinwu Glacier. The velocities were measured within the 100 m section near the glacier terminus from 2017 to 2025. The manual and PlanetScope-derived velocities show consistent interannual variations, supporting the reliability of the detected near-terminus velocity decrease.

Comment (3) *For the 600-1550 m section, the velocity pattern is more spatially coherent than the 0-200 section, but there seems to be no clear deceleration after the outburst event. Based on Figure 4(a), the temporal pattern at this section appears to be more of a biannual fluctuation for unknown reasons, unrelated to the lake outburst. Besides, the authors state, "the mean velocity also decreased from 38.25 ± 9.12 m* a^{-1} *during 2017–2021 to 32.87 ± 8.37 m* a^{-1} *during 2022–2025, representing a 14% reduction," but the pair at the nominal year 2021 has the acquisition period between September 2020 and September 2021 (Table S2), which is entirely after the flood event. What if we compare the average for 2017–2020 (before the flood) with that for 2021–2025 (after the flood)? Will the speed change be significant?*

Response: Thank you for pointing this out. We do not consider the velocity change in this section to represent a significant slowdown signal. In the manuscript, the velocity change in this section was reported mainly to provide a relatively upstream background section for comparison with the section closer to the glacier terminus, thereby showing that the velocity change in the near-terminus section was much larger. However, the use of terms such as "decreased" and "14% reduction" may have caused misunderstanding by implying that we interpreted the 600–1550 m section as showing a clear slowdown. Therefore, we have revised the relevant text in the revised manuscript and no longer describe the velocity change in the 600–1550 m section as a slowdown.

If there is a slowdown, is it really related to the outburst flood?

Comment (4) *Based on the ITS_LIVE data and the PlanetScope velocity maps shown in Figure 4(d), the glacier speed at the 600-1550m section has been constantly decreasing since 2005. This trend seems independent of the 2020 outburst flood event and is consistent with previous findings that glaciers in High Mountain Asia are slowing due to ice thinning (Dehecq et al., 2019). The results presented in the manuscript do not provide sufficient evidence to support proglacial lake drainage as a factor (sole factor or one of many factors) influencing glacier speed, since there is no clear temporal relationship.3*

Response: Thank you for pointing this out. The description of velocity changes in the 600–1550 m section in the manuscript was indeed not sufficiently robust. We do not consider the velocity changes in this section to represent a significant slowdown signal. The purpose of reporting the velocity changes in this section was mainly to use it as a background section relatively far from the terminus disturbance, and to compare it with the section closer to the glacier terminus. However, the use of terms such as "decreased" and "14% reduction" in the original manuscript may have caused misunderstanding, leading readers to think that we interpreted the 600–1550 m section as showing a clear slowdown signal. We no longer describe the velocity changes in the 600–1550 m section as a slowdown, but instead describe them as small-amplitude interannual fluctuations, and clarify that their relationship with the 2020 GLOF is unclear.

Comment (5) *Figure 4(c) also shows a decreasing trend of glacier speed at the 0-200m section based on the ITS_LIVE data, although there is a huge discrepancy between ITS_LIVE and PlanetScope-derived velocity maps. This discrepancy should be further discussed. If ITS_LIVE data can be trusted, then the slowdown can be interpreted the same way above, which is independent of lake drainage. If ITS_LIVE cannot be trusted, what makes the PlanetScope-derived data more reliable? After all, some of the measurements are biased by incorrect matches.*

Response: Thank you for pointing this out. We consider that the ITS_LIVE results are not well suited for constraining local velocity changes in this section. The terminus section is relatively short, less than 500 m, whereas the spatial resolution of ITS_LIVE is approximately 120 m. Therefore, this region is usually represented by only one or two pixels, making it difficult to resolve the velocity gradient near the glacier terminus. In addition, this area is affected by ice cliffs, crevasses, shadows, and newly exposed terrain after the GLOF. As a result, ITS_LIVE pixels may easily mix signals from glacier ice, the lake margin, and non-glacier surfaces. Therefore, the ITS_LIVE results at this local scale are subject to large uncertainty. Moreover, the manually measured feature displacements in the terminus section differ substantially from the ITS_LIVE results, further indicating that ITS_LIVE is not robust in this area.

In addition, although previous studies have reported glacier slowdown in High Mountain Asia (Dehecq et al., 2018) in response to mass loss, the near-terminus velocity gradient changed markedly in our study area (Figure 1). To further test the robustness of the velocity changes in this section, we added an analysis of the near-terminus velocity gradient. We selected manually identifiable feature points along the glacier centerline for the 2017–2018 and 2023–2024 image pairs. The results show that, before the GLOF, velocities in the 2017–2018 image pair were relatively high and varied only slightly along the centerline. After the GLOF, however, velocities in the 2023–2024 image pair clearly decreased from upstream toward the glacier terminus, showing a distinct deceleration gradient toward the terminus (Figure 5). This change in the velocity gradient suggests stronger resistive effects near the glacier terminus after the GLOF, which is consistent with the expected dynamic response to the loss of lake–ice contact, reduced buoyant support, and increased terminus boundary resistance after lake drainage.

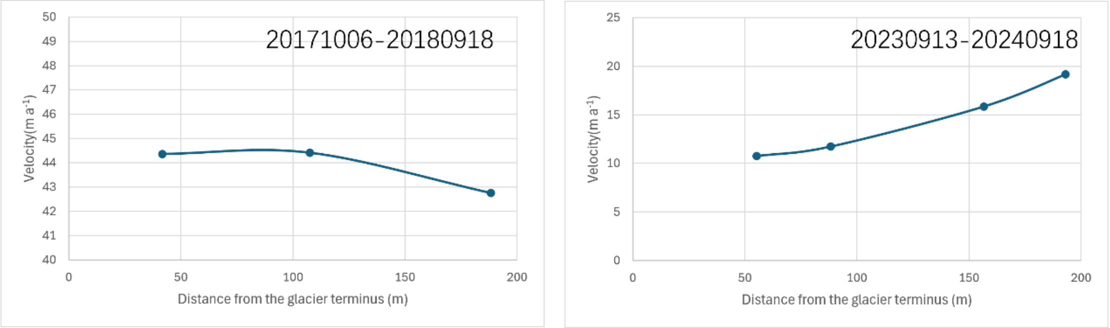

Figure 4 Near-terminus velocity gradients derived from manually tracked feature points along the glacier

centerline.

What caused the lag?

Comment (6) *The supporting evidence for a slowdown after 2021/2022 is weak. In fact, Figure 4(a) and Figure S6 (tie point #6) indicate that a slowdown could take place in 2020 if we only consider the 0-200 m section. A recent paper by Baldacchino et al. (2025) suggests that the frontal dynamic signal can be blocked by an icefall, so there is a reason for not seeing any velocity changes in 2020 at the 600-1550m section. A slowdown with no time lag is physically more convincing than a lagged slowdown since the effective pressure changes immediately when water drains.*

If the authors still prefer a lagged slowdown model, then they should really explain why a lagged slowdown is necessary. After all, a slowdown can be due to many factors, such as glacier thinning, ice temperatures, and changes to subglacial hydrological conditions. It can be irrelevant to lake drainage, and if so, it can take place gradually or abruptly at any time, depending on the driving mechanism.

Response: Thank you for pointing this out. We agree that the term “lagged slowdown” in the manuscript may have caused misunderstanding by implying that the slowdown signal only started sometime after the GLOF. Our intention was not to suggest that the glacier terminus did not respond immediately after the GLOF. Rather, we intended to express that the terminus velocity had already begun to decrease after the GLOF, but that a more stable low-velocity state became clearer after 2021.

Therefore, we have removed or revised the wording related to “lagged slowdown” in the revised manuscript. The revised interpretation is that the near-terminus section may have undergone a rapid dynamic response and velocity decrease after the GLOF, while a more stable low-velocity state gradually became evident after 2021.

Comment (7) *Elevation change is not necessarily related to the flood. The authors use three DEMs for the differencing analysis (Section 4.3), but none of them represent the elevation near the time of the outburst event. The 2014-2025 DEM differencing results can include surface mass balance and dynamic signals from both before and after the flood. This has made it difficult to explain anything. With a NAMD of 6.46 m (0.59 m/yr; conventionally in the sense of 1-sigma uncertainty) for the 2014-2025 ASTER DEM differencing (Section 3.3), it is hard to justify the elevation change signals for the zone with neutral changes at 600m, which is interpreted by the authors as an ice bulge due to glacier slowdown.*

Are there more elevation data available? If data scarcity is an issue, I would suggest a more conservative argument that the elevation change analysis cannot determine whether the outburst flood affected the ice dynamics.

Response: Thank you for pointing this out. Based on the available DEM differencing results, it remains difficult to robustly distinguish glacier surface elevation changes before and after the GLOF. This is particularly the case near the glacier terminus, where elevation-change results from different data sources show differences. In the manuscript, we used 2014 as the temporal division mainly to reduce uncertainty by extending the DEM differencing interval. However, we also recognize that this treatment mixes pre- and post-GLOF signals within the same period, making it difficult to clearly separate surface elevation changes before and after the event. To further evaluate possible post-GLOF changes, we compared our results with the 2014–2019 glacier surface elevation changes (Hugonnet et al., 2021) and restricted the analysis to the relatively stable area above the icefall, where data consistency is better. In the manuscript, we did not directly link the GLOF to surface elevation changes near the glacier terminus, but mainly discussed the relatively more robust reduction in thinning rate in the upstream section.

To further assess the reliability of the elevation-change results, especially the reduced thinning signal, we estimated the uncertainty of glacier surface elevation changes along the glacier centerline using the method proposed by Hugonnet et al. (2022). The results show that, in the main section where reduced thinning was observed, the uncertainty ranges of the two elevation-change periods mostly do not overlap, suggesting that the reduced thinning signal at this regional scale is relatively reliable.

Nevertheless, the influence of the GLOF on surface elevation change should not be overinterpreted. Therefore, in the revised manuscript, we have further weakened the relevant statements and only present them as conservative descriptions and interpretations in the Discussion.

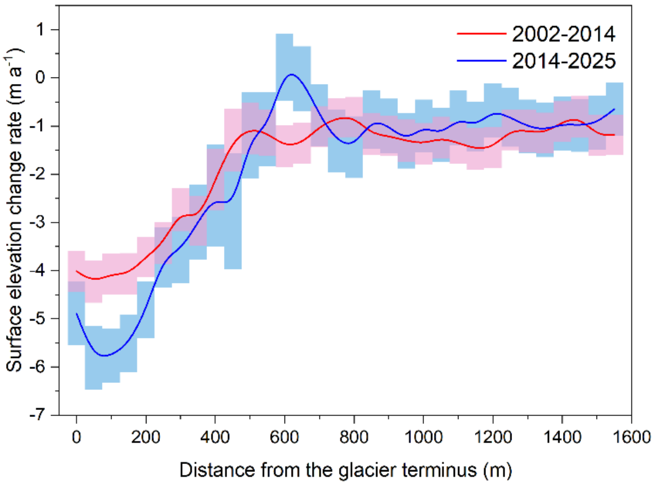

Figure 5 Surface elevation-change rates along the centerline of Jinwu Glacier during 2002–2014 and 2014–2025. The x-axis indicates the distance from the glacier terminus. Red and blue solid lines represent the mean surface elevation-change rates for 2002–2014 and 2014–2025, respectively. Shaded areas indicate the corresponding uncertainty ranges.

Comment (8) *L214-217 and Figure S4: Questionable arguments. (1) Minowa et al. (2023) do not address anything about Jinwu Glacier. (2) I have no idea what this talks about regarding proglacial lake depth. According to my understanding, Figure S4(a) plots the bathymetry of Jinwuco with terminus positions Jinwu Glacier between 2003 and 2012. As a result, the data plotted in Figure S4(b) must be the velocity data from 2003 to 2012, which cannot be derived from the PlanetScope velocity maps used in this study. Hence, I think this argument, "ice-flow velocity (600-1550m) was significantly positively correlated with lake depth," is falsified. (3) For Figure S4, I am not sure how a paper published in 2019 (Zhang et al., 2019) contains the data measured after the 2020 GLOF.*

Response: Thank you for the careful review. The relevant statements in the manuscript were indeed not sufficiently clear and may have caused misunderstanding. First, Minowa et al. (2023) did not study Jinwu Glacier. Our purpose in citing this study was only to indicate that previous studies have shown that changes in proglacial lake depth may affect the dynamics of lake-terminating glaciers, rather than to use it as direct evidence for Jinwu Glacier.

In addition, our purpose in discussing proglacial lake depth was to provide a more complete discussion of how the past influence of the glacial lake on glacier motion may have been related to lake-depth changes. The velocity data used in Fig. S4 were not derived from the PlanetScope velocity results in this study, but from the ITS_LIVE dataset.

Finally, the reviewer is correct regarding the reference for Fig. S4. In the original manuscript, the data source was incorrectly cited as Zhang et al. (2019), which was an oversight on our part. The correct reference should be Zhang, G., Bolch, T., Yao, T. et al. Underestimated mass loss from lake-terminating glaciers in the greater Himalaya. Nature Geoscience, 16, 333–338 (2023).

After further consideration, we have decided to remove the discussion related to lake depth from the manuscript. This is because lake depth is not the focus of this study. In addition, linking upstream velocity to lake depth is not sufficiently reliable, given the presence of the icefall.

Other specific comments

Comment (9) *L31: Can be more specific: "expansion of proglacial lakes."*

Response: We have revised the relevant sentence accordingly by replacing the wording with "expansion of proglacial lakes" to make the expression more specific and clear.

Comment (10) *L46-47: This sentence is a bit hard to read. A "retreating glacier" already indicates the retreat is continuous. Consider rewrite.*

Response: We have revised the sentence to avoid redundancy and improve readability.

Comment (11) *L54: How does lake expansion (rather than lake shrinkage) lead to the detachment from the glacier?*

Response: Thank you for pointing this out. We did not mean that lake expansion itself directly leads to detachment from the glacier. Rather, as glacier retreat and the potential lake basin becomes fully exposed, the lake may no longer remain in contact with the glacier terminus. We have revised the sentence in the manuscript to clarify this process.

Comment (12) *L57: A short-lived perturbation? But the case in Jinwuco shows a persistent detachment, isn't it?*

Response: What we intended to express was that, unlike the gradual lake–glacier detachment caused by the complete exposure of a potential lake basin during glacier retreat, the detachment between Jinwuco and the glacier terminus occurred through a much more rapid process. We recognize that the wording may have caused confusion. Therefore, in the revised manuscript, we have revised this expression and no longer use "short-lived perturbation". Instead, we emphasize the "rapid transition" or "abrupt detachment process" to more accurately describe the persistent lake–glacier detachment state that formed after the GLOF at Jinwuco.

Comment (13) *L58-60 and L228-230: More details about this previous study are needed in order to make a better context. Is this the only study about the glacier response to the proglacial lake changes? How similar is it to Jinwuco? Was Longbasaba also detached from water during the drainage? Do we expect to see the same behaviors at Jinwuco?*

Response: Thank you for this comment. Longbasaba Glacier is not the only case study concerning the influence of proglacial lake changes on glacier dynamics, but it is the only case we are aware of that involves lake-level change and its possible influence on glacier behavior. Our purpose in citing this study in the Introduction was to show that changes in proglacial lake level may modify the boundary conditions at the glacier terminus and further affect the dynamics of the glacier tongue. The study mainly examined the velocity changes and retreat of Longbasaba Glacier since 1990. It noted that the water level of Longbasaba Lake was artificially lowered by approximately 5.6 m in 2005, after which the glacier experienced accelerated retreat during 2005–2008. The velocity results also showed that Longbasaba Glacier accelerated in 2006, although the velocity began to decrease again after 2007. This study suggested that, in general, a decrease in lake level tends to stabilize a glacier, because reduced subglacial water pressure can increase the effective pressure. However, if a substantial part of the glacier is close to flotation, lake-level lowering may increase the ice-surface slope near the grounding line, thereby increasing grounding-line flux and enhancing calving into the lake. The lake-level lowering at Longbasaba Lake was relatively small compared with that at Jinwuco, and the glacier remained in contact with the lake after the drainage. In contrast, Jinwuco experienced a much larger lake-level drop during the 2020 GLOF, which directly caused the detachment between the lake and the glacier terminus. Therefore, we do not expect Jinwu Glacier to show the same dynamic response as Longbasaba Glacier. Instead, we use the Longbasaba case as a reference to further discuss the changes observed at Jinwu Glacier.

Following this suggestion, we have added more background and details about the Longbasaba study in the Introduction and Discussion of the revised manuscript.

Comment (14) *L80-83: SRTM and RGI data need to be cited. Also, (1) "an elevation range of ~3666 m" does not make sense. (2) "Basin topography is high in the west and low in the east." (3) Why does a high topographical relief favor the existence of glacial lakes? Or do you just mean "high elevation?"*

Response: Thank you for this comment. We have added the appropriate citations for the SRTM DEM and RGI 7.0 datasets. In addition, we have removed some unnecessary descriptions, including the ambiguous statements about elevation range, basin topography, and the relationship between topographical relief and the existence of glacial lakes.

Comment (15) *L125-127: "In autumn and winter, terrain shadows often obscure large parts of the lower glacier…" and "We therefore mainly selected scenes from September and October to minimize shadow effects" are conflicting ideas and do not make sense.*

Response: Our intention was to indicate that terrain shadows become much stronger after October, which can affect the feature-tracking results over the lower glacier. Therefore, we have revised the relevant wording to "after October" and clarified that we mainly selected images from September and early October to minimize the effects of snow cover and terrain shadows on velocity retrieval.

Comment (16) *L138: NMAD of stable-terrain velocities? I can see how the authors define the stable terrains, but it would be better if they could provide the actual locations where the velocities are used for NMAD. Is Figure S2 something we are looking for?*

Response: Yes. Figure S2 shows the areas used to calculate the NMAD of stable-terrain velocities. We have further clarified in the revised manuscript that the NMAD of stable-terrain velocities was calculated based on the velocity results over the off-glacier stable terrain shown in Fig. S2. To avoid ambiguity, we have also added a reference to Fig. S2 in the main text to indicate the specific spatial locations used for the uncertainty assessment.

Comment (17) *L141 and Equation 2: Please avoid using Vx and Vy for displacement components. They should be reserved for velocity components.*

Response: Thank you for this suggestion. We have changed Vx and Vy in Equation 2 and the related text to Dx and Dy to represent the east–west and north–south displacement components.

Comment (18) *Section 4.1: I do not see a clear relationship between this section and the other parts of the paper. How does the change of lake area over time relate to glacier speed? Besides, some data appear to be identical from Zheng et al. (2021), but there is no corresponding citation in this section. What's new? Finally, this study focuses on glacier dynamics rather than lake* *evolution, so why not map changes in terminus position instead of lake area?*

Response: Following the comment, we have rewritten Section 4.1 by shifting the focus from changes in lake area to changes in glacier terminus position and lake–glacier contact state. In the revised manuscript, we added interpretations of glacier terminus boundaries for different periods and quantified terminus retreat/advance distances along the glacier centerline.

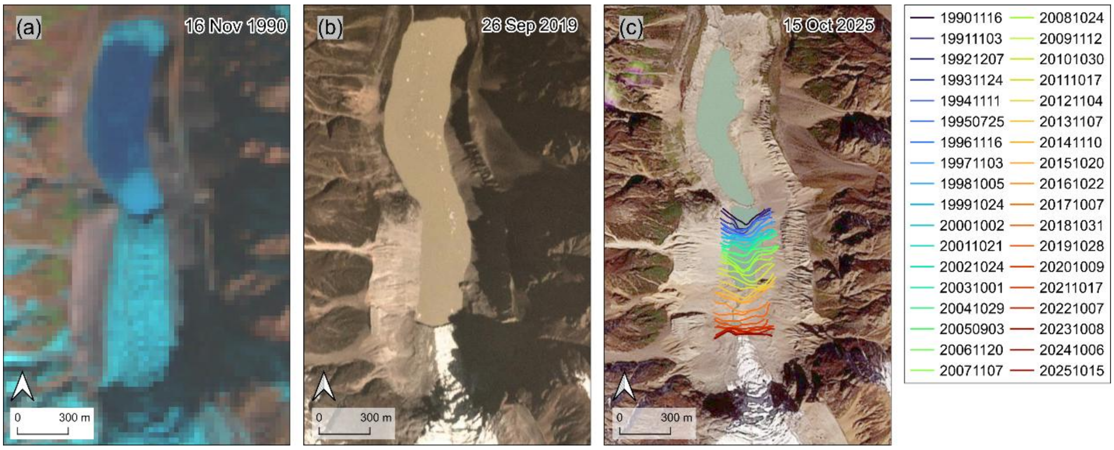

Figure 7 Multi-temporal evolution of the glacier terminus. (a) Landsat 5 TM imagery as the basemap acquired on 16 November 1990; (b–c) PlanetScope imagery as the basemap acquired on 26 September 2019 and 15 October 2025, respectively. Colored lines indicate glacier terminus positions mapped from satellite images between 1990 and 2025.

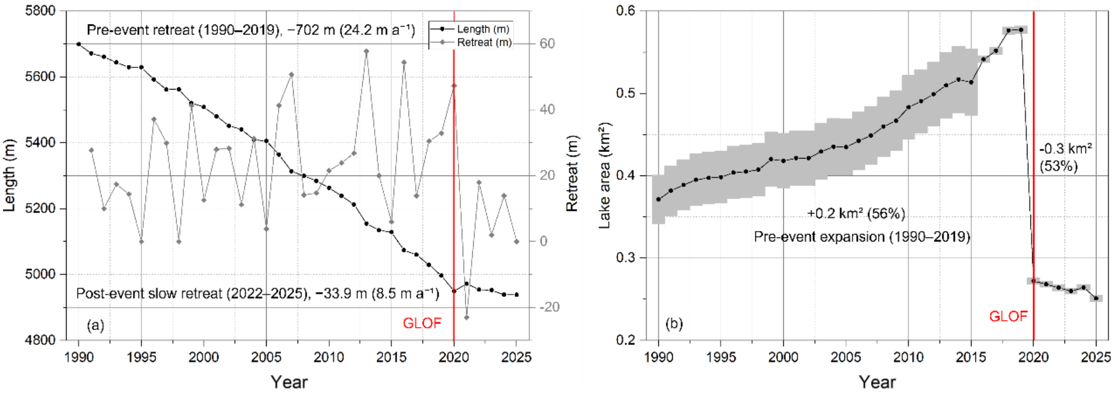

Figure 6 Changes in glacier terminus position and lake area during 1990–2025. (a) Glacier length and annual terminus retreat of Jinwu Glacier measured along the glacier centerline. (b) Lake area changes of Jinwuco during 1990–2025. Black dots denote the mapped lake area for each year. The grey shading indicates the uncertainty of lake area.

Comment (19) *Figure 2: Specify the Landsat mission whenever possible.*

Response: We have specified the Landsat mission used in the caption of Fig. 2 by changing “Landsat TM imagery” to “Landsat 5 TM imagery”.

Comment (20) *Figure 3: The outburst flood took place in June 2020, but the lake area changed* *drastically between 2019 and 2020, according to this figure. Please check.*

Response: The apparent lake-area change from 2019 to 2020 in Fig. 3 was caused by the GLOF in June 2020. The corresponding image acquisition dates are listed in Table S1. To avoid misunderstanding, we have further clarified this point in the figure caption and the main text.

Comment (21) *L188: What do you mean by "ice-cliff morphology progressively degraded?" How much is the terminus advance? (See my comment for Section 4.1)*

Response: Our interpretation was mainly based on observations of the glacier terminus morphology from multi-temporal images. After October, a steep terminus ice cliff usually produces a clear shadow zone at the glacier terminus. However, this shadow zone largely disappeared after 2022, suggesting that the formerly steep terminus ice cliff may have collapsed, lowered, or become morphologically fragmented. To avoid ambiguity, we have removed the vague expression "progressively degraded" and replaced it with a more specific description, stating that the terminus ice cliff became discontinuous after 2022 and showed local collapse and morphological fragmentation. In addition, following the reviewer's suggestion, we have added a quantitative description of terminus-position changes in the Results section. Measurements along the glacier centerline show that the glacier terminus advanced by approximately 23 m in 2021 relative to its position in 2020.

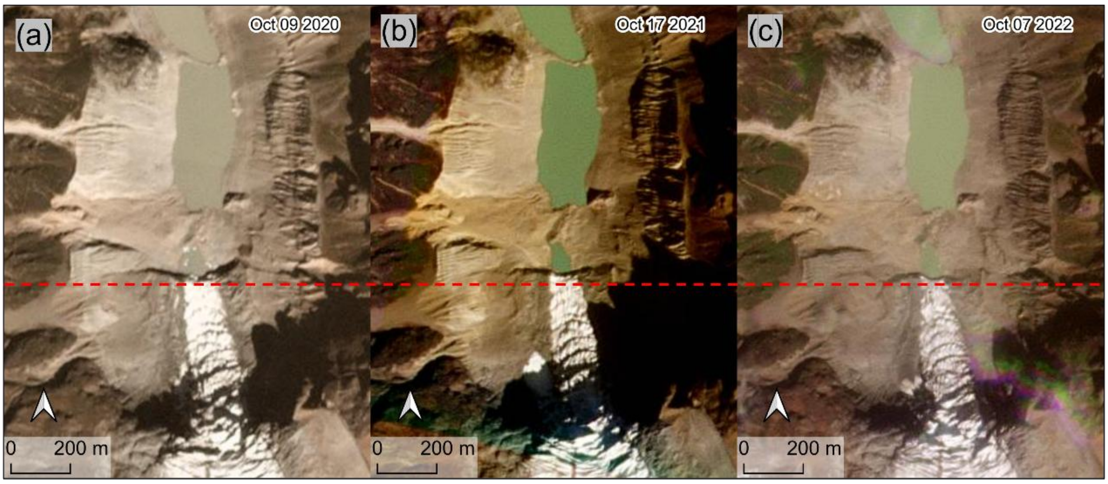

Figure 7 Map of terminus changes of Jinwu Glacier during 2020–2021 based on PlanetScope imagery. The red dashed line is used as a visual reference to facilitate comparison of glacier terminus-position changes among different years.

Comment (22) *L192: "magnitude of deceleration" does not make sense here. Maybe just "ice speed"?*

Response: We have revised the wording accordingly by replacing "magnitude of deceleration" with "ice speed".

Comment (23) *Figure 4: How do you derive the data (including ice speed and its uncertainty) to be plotted in panels (c) and (d) from panel (a)? This needs to be explained in the methods section. For ITS_LIVE data, it is necessary to indicate what data set you use (annual mosaics or image-pair velocities).*

Response: We have added an explanation in the revised manuscript. The longitudinal velocity profiles shown in Fig. 4(a) were derived from each PlanetScope velocity map by sampling velocities along the glacier centerline at 50 m intervals. Figures 4(c) and 4(d) were then calculated based on these profiles by extracting the mean velocities within the 0–200 m and 600–1550 m sections, respectively, to represent the mean ice-flow speed of each section for the corresponding year. The error bars indicate ±1 standard deviation of the 50 m binned velocity values within each section, reflecting the spatial variability of velocity within that section. The uncertainty of the velocity maps themselves was assessed using the NMAD of velocities over off-glacier stable terrain. For the ITS_LIVE data, we have clarified in the Methods section that we used the ITS_LIVE annual velocity mosaics rather than the image-pair velocity products. The ITS_LIVE velocities for the 0–200 m and 600–1550 m sections were also extracted along the glacier centerline, and section-mean velocities were calculated using the same distance ranges.

Comment (24) *Figure 5: What is the red dashed line? (Also, see my comment for Section 4.1)*

Response: The red dashed line was added as a visual reference to help readers identify changes in the glacier terminus position. To avoid confusion, we have clarified its meaning in the figure caption.

Comment (25) *L201: Should be moved to the Methods section.*

Response: We have moved the relevant sentence to the Methods section.

Comment (26) *L221-223: The reduction of hydrostatic pressure is immediate when the lake level drops, so it makes no sense for a temporarily maintained extensional flow afterward.*

Response: Thank you for pointing this out. We agree that the hydrostatic pressure and buoyant support would decrease immediately after the lake level dropped. Therefore, the statement about “temporarily maintained extensional flow” in the original manuscript was not rigorous and may have caused misunderstanding. We have removed this expression and revised the related mechanism explanation. The focus of our revision is that the rapid lake-level drop after the GLOF immediately changed the boundary conditions at the glacier terminus, including the loss of lake–ice contact, reduced buoyant support, and increased basal resistance. However, the adjustment of the glacier terminus from a lake-terminating to a land-terminating state may not have been completed instantaneously. During this transition, the terminus ice that was previously supported by lake water may have undergone changes in grounding conditions, ice-cliff instability, and enhanced collapse, which could have led to a short-term increase in terminus ice flux and ice-cliff collapse. As the terminus boundary conditions gradually stabilized, enhanced basal resistance and land-terminating conditions became dominant, and the glacier entered a relatively stable low-velocity state after 2022**.**

Therefore, in the revised manuscript, we no longer explain the response as the maintenance of extensional flow after lake-level lowering. Instead, we describe it more cautiously as a rapid reorganization of the terminus force balance after the GLOF, possibly accompanied by a short-term increase in terminus ice flux and ice-cliff collapse, followed by a gradual transition to a relatively stable land-terminating state.

Comment (27) *L227: Since you used "ice calving," it is necessary to clarify whether the terminus is still connected to any water bodies in 2021. Did this calving affect the terminus position? Is it related to the slight terminus advance in 2021 (Figure 5)?*

Response: The term "ice calving" could lead readers to misunderstand that the terminus of Jinwu Glacier was still directly connected to Jinwuco in 2021. In fact, after the 2020 GLOF, the terminus of Jinwu Glacier had already become detached from the main lake body. Therefore, the phenomenon observed in 2021 does not represent typical calving of a lake-terminating glacier. More accurately, it reflects local instability and collapse of the terminus ice cliff after the loss of lake support following the GLOF (Peng et al., 2023).

Following this comment, we have revised the relevant wording in the manuscript by replacing "ice calving" with the more specific terms "ice-cliff collapse" or "local collapse of the terminus ice cliff". This phenomenon mainly reflects the unstable state of the terminus ice after the GLOF, rather than continued lake-terminating calving.

Regarding the slight terminus advance in 2021, we do not interpret it as a direct result of ice-cliff collapse. The slight advance more likely reflects a short-term adjustment of the terminus stress balance after the abrupt loss of lake support and the transition from a lake-terminating to a land-terminating state.

Comment (28) *L230-235: The topic jumps abruptly from velocity analysis to elevation data within the paragraph without transition. Consider rearranging.*

Response: We have reorganized this part in the revised manuscript to improve the transition between the velocity analysis and the elevation-change analysis.

Comment (29) *L247-248: The changes at Jinwu Glacier may always be controlled by the climate forcing, isn't it? (cf. Fig 4(d) and L230-235). Before making this argument, the authors need to clearly show the velocity/elevation changes caused by the outburst flood with convincing evidence. See my major comments for details.*

Response: Thank you for this comment. We agree that long-term changes at Jinwu Glacier may also be influenced by climatic forcing. In the revised manuscript, the elevation-change results are discussed more conservatively, while the GLOF-related dynamic adjustment is mainly supported by the observed change in the near-terminus velocity gradient after the lake drainage.

Comment (30) *L254: GLOFs from moraine-dammed glacial lakes, not moraine-dammed GLOFs.*

Response: We have revised the wording from "moraine-dammed GLOFs" to "GLOFs from moraine-dammed glacial lakes".

Comment (31) *L278-281: How is the pairwise RMSE defined? The mean standard deviation of what? What agreement is substantially improved? I can get a rough sense of these quantities, but I think these should be explicitly defined, along with what they are used to assess.*

Response: Thank you for pointing this out. The definitions of pairwise RMSE, mean standard deviation, and "agreement" were not sufficiently clear in the original manuscript. We have added explanations of how these metrics were calculated and what they were used to assess.

Specifically, pairwise RMSE refers to the root-mean-square difference between any two elevation-change profiles within the same distance section and is used to evaluate the consistency of elevation-change-rate estimates from different data sources. For two data sources A and B, it was calculated as the root mean square of the differences between the two elevation-change-rate profiles at the same centerline sampling points. The mean standard deviation refers to the standard deviation of elevation-change rates from the three data sources at each centerline sampling point, averaged over all sampling points within the target distance section. This metric is used to represent the overall dispersion among the three data sources.

Therefore, "agreement is substantially improved" means that, compared with the 0–550 m terminus section, both the pairwise RMSE and the mean standard deviation among different data sources are markedly lower in the 600–1550 m upstream section, indicating that the elevation-change estimates from different data sources are more consistent in this section. We have clarified these definitions in the revised manuscript and explained that these metrics are used to assess the consistency and reliability of different elevation-change datasets in different glacier sections.

Comment (32) *L293: The thinning near the terminus is robust, but how is it (not) related to the lake drainage? The latter is the main focus of the study, but is skipped here. (See my major comment on the elevation analysis as well.)*

Response: Although our results show that the thinning signal near the glacier terminus is relatively robust, the available DEM data make it difficult to distinguish glacier surface elevation changes before and after the GLOF. In the manuscript, we used 2014 as the temporal division mainly to reduce uncertainty by extending the DEM differencing interval. However, we also recognize that this treatment mixes pre- and post-GLOF signals within the same period, making it difficult to clearly separate surface elevation changes before and after the event. In the manuscript, to further evaluate possible post-GLOF changes, we additionally compared the glacier surface elevation changes during 2014–2019 (Hugonnet et al., 2021) and only provided a limited discussion for the relatively stable area above the icefall, where data quality is better. We did not interpret the terminus thinning as a direct result of the GLOF. Following the comment, we have emphasized the uncertainty of surface elevation changes near the glacier terminus in the revised text to avoid misunderstanding.

Comment (33) *The term "ice-flux demand" repeatedly appears in the manuscript, but what actually is it? It should be defined or explicitly explained.*

Response: By "ice-flux demand", we intended to describe a situation in which the ability of the downstream glacier tongue to transport ice forward is reduced after the terminus slows down, while ice from the upstream section continues to be supplied downstream without a clear change in velocity. This may lead to longitudinal compression in the 500–750 m section. To avoid ambiguity, we have removed the term "ice-flux demand" from the revised manuscript and replaced it with a more direct dynamic explanation.

Comment (34) *L325-327: What is the availability for the PlanetScope images used in this study? Are they open to the public?*

Response: The PlanetScope images used in this study were accessed through an educational account. The PlanetScope imagery is not publicly redistributable under the data-use agreement.

References

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