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) *The authors describe a pronounced ice fall (L88–90): "Its lower tongue is partly debris covered and includes a pronounced icefall. The icefall has an elevation drop of ~300 m, and the maximum slope exceeds 35°, based on the SRTM DEM (Fig. 1e)." This ice fall is also clearly visible in Figure S6. I believe that this icefall disconnects the terminus region from the upper part of the glacier and that a signal from the terminus cannot propagate upstream across this icefall. Consequently, I think that this physical setting makes the analysis of dynamics in the upper part of the glacier unrelated to the GLOF event at the terminus or at least strongly complicates it.*

Response: Thank you for raising this important point. Our intention was not to attribute the velocity changes above the icefall to the GLOF. The velocity variations in the 600–1550 m section are relatively small and do not show a clear post-GLOF response. However, the original manuscript was not sufficiently clear and may have led to an overinterpretation of this section. Following this comment, we have revised the relevant Results and Discussion. We no longer describe the small changes in the 600–1550 m section as a slowdown but instead refer to them as velocity fluctuations. We also explicitly state that the icefall may limit stress transmission from the terminus to the upper glacier, and therefore the velocity changes above the icefall should not be interpreted as direct evidence of a GLOF-induced dynamic response.

Comment (2) *The authors assess the surface elevation change rate before and after the GLOF event. However, the chosen time periods make this comparison difficult. The pre-GLOF period is 2002–2014, and the post-GLOF period is 2014–2025. However, the GLOF occurred in 2020, meaning that the "post-GLOF" period includes signals from both before and after the event. In addition, the dataset from Hugonnet et al. (2021) for the period 2015–2019 shows the same surface thinning rate as the period 2014–2025. The datasets support the conclusion that the glacier experiences long-term thinning in both the terminus and the upstream regions. However, no robust interpretation of the GLOF's effect can be drawn from the current analysis, which appears to be the central goal of the paper. Ideally, the signal should be disentangled to clearly represent pre- and post-event periods, or different DEMs should be used (though I assume these* *may not be available). Considering these two points, the current dataset supports only the findings of a flow velocity decrease in the terminus region and geometry changes at the front. Interpretations regarding elevation change and flow velocities in the upstream region are, in my view, not well supported by the data as presented.*

Response: Thank you for this important comment. In fact, as stated in the manuscript, enhanced thinning near the glacier terminus had already occurred before the GLOF, and the agreement among different elevation-change datasets is relatively poor in the terminus region. Based on the available DEM differencing results, it remains difficult to robustly distinguish surface elevation changes before and after the GLOF, especially near the glacier terminus.

We used 2014 as the temporal division mainly to reduce uncertainty by extending the time interval of DEM differencing. Due to the limited availability and accuracy of DEM data in this region, it is still difficult to accurately capture differences in terminus surface elevation change before and after the GLOF. We also recognize that this treatment mixes pre- and post-GLOF signals within the same period, making it impossible to clearly separate surface elevation changes before and after the event. To further evaluate possible post-GLOF changes, we incorporated the 2014–2019 glacier surface elevation change results from Hugonnet et al. (2021) and restricted the analysis to the relatively stable area above the icefall, where data quality is comparatively better.

Following this comment, we will explicitly acknowledge this limitation in the revised manuscript. The revised manuscript will no longer treat the local elevation changes near the terminus as direct evidence for a GLOF-induced dynamic response. Meanwhile, surface elevation changes in the upper glacier will only be cautiously interpreted as part of the discussion.

Minor comments

Comment (3) *Long term evolution of Jinwuco: Zheng et al. (2021) (https://doi.org/10.5194/tc-15-3159-2021) present almost the exact same analysis of Jinwuco lake as presented here. Therefore, the difference to this analysis should clearly be established and it should be justified why the analysis should be repeated.*

Response: Thank you for this comment. The main reason for re-mapping the Jinwuco lake boundaries in this study is that we needed to extend the lake-change record to 2025. Because lake boundaries in this study were mapped by visual interpretation, we reconstructed the lake extent for the entire 1990–2025 period using a consistent interpretation standard, rather than directly combining the results of Zheng et al. (2021) with our newly mapped 2021–2025 results. This avoids inconsistencies caused by different datasets or mapping criteria and reduces additional uncertainty in estimating lake-area changes. We have also added the appropriate citation to Zheng et al. (2021). In addition, we have revised this section so that it no longer focuses only on lake-area evolution. We have added the 1990–2025 terminus-position changes of Jinwu Glacier, which are more directly related to the glacier-dynamic analysis in the manuscript (Figure 1).

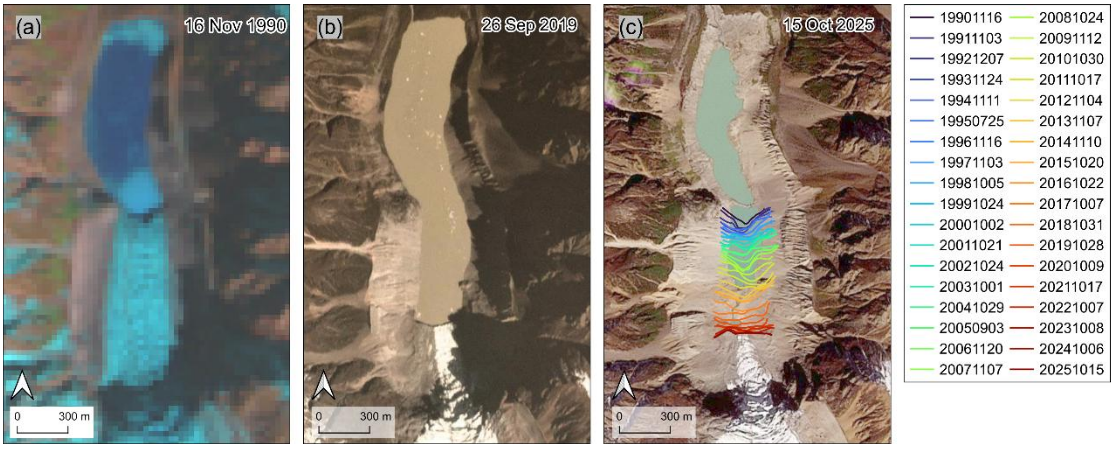

Figure 1 Multi-temporal evolution of the glacier terminus. (a) Landsat 5 TM imagery as the basemap acquired on 16 November 1990; (b–c) PlanetScope imagery as the basemap acquired on 26 September 2019 and 15 October 2025, respectively. Colored lines indicate glacier terminus positions mapped from satellite images between 1990 and 2025.

Comment (4) *Flow velocities: The authors clearly demonstrate that flow velocities in the terminus region decrease distinctly following the GLOF and after a short adjustment period. In the upstream section, a decrease in flow velocities is also reported. However, this decrease lies within the stated uncertainty range (from 38.25 ± 9.12 m a^{-1} to 32.87 ± 8.37 m a^{-1}). Combined with the likely dynamic decoupling discussed in Major Comment 1, I do not find the upstream signal convincing.*

Response: *Thank you for this comment. We have revised the relevant descriptions in the Results and Discussion sections. We no longer interpret the small velocity change in the 600–1550 m section as a clear post-GLOF slowdown, but instead describe it as velocity fluctuations.*

Comment (5) *Study area section: The study area section lacks information on lake depth and on water depth at the glacier front. This information would be valuable for understanding the terminus regime.*

Response: Thank you for this suggestion. We have added information on lake depth in the Study area section. Post-GLOF bathymetric measurements show that the Jinwuco basin consists of two main depressions, with the depression near the glacier terminus having a maximum post-GLOF water depth of approximately 17 m (Zhang et al., 2023). Considering the approximately 20 m lake-level lowering during the GLOF (Zheng et al., 2021), the pre-GLOF maximum water depth in this depression was inferred to be approximately 37 m.

Comment (6) *Discussion opening: The discussion opens with an interpretation of flow* *velocities and lake depth (L215): "Our analysis shows that when lake depth exceeded 10 m (measured after the GLOF) (Zhang et al., 2023a), ice-flow velocity (600-1550m) was significantly positively correlated with lake depth ($p< 0.001$, $R^2 = 0.78$, Fig. S4)". However, neither in the methods nor the results I find this analysis and it is only displayed in Fig. S4.*

Response: Thank you for pointing this out. We have removed the discussion of the relationship between lake depth and ice-flow velocity. This is mainly because the correlation analysis used velocities from the 600–1550 m section, which is located relatively upstream and may be affected by weakened stress transmission across the icefall. Therefore, directly linking velocity changes in this section to lake-depth changes is not sufficiently robust.

Specific comments:

Comment (7) *Title: I find the current title somewhat hard to read and I think does not capture the central contribution of the study. A possible alternative, which puts the focus on the lake to land transition following a GLOF: "Glacier response to the transition from lake to land-terminating following a glacial lake outburst flood" (optionally: Jinwu Glacier, southeast Tibet).*

Response: Thank you for this helpful suggestion. We plan to revise the title to "Glacier response to the lake- to land-terminating transition following a glacial lake outburst flood at Jinwu Glacier, southeastern Tibet".

Comment (8) *L18: a decrease from 40 m a^{-1} to 20 m a^{-1} is a 50 % not a 49 % decrease. I assume this stems from rounding in the underlying values.*

Response: The decrease is now described as approximately 50%.

Comment (9) *L111: Do you mean manual lake delineation when you say "manual visual interpretation"?*

Response: Yes. The phrase "manual visual interpretation" in the manuscript refers to manual delineation of the lake boundaries. To avoid ambiguity, we have revised the sentence to: "We reconstructed changes in the extent of Jinwuco from 1990 to 2025 by manually delineating lake boundaries from historical remote sensing imagery (Table S1)."

Comment (10) *L170: This would be more clearly expressed as "m per year."*

Response: We have removed this sentence in the revised manuscript, because we have added new results on glacier terminus-position changes and simplified the description of lake-area expansion.

Comment (11) *L186: Which year does the value 41.82 m per year refer to?*

Response: The value of 41.82 m per year refers to the mean velocity during 2017–2020. We have clarified this in the revised manuscript.

Comment (12) *L202-204: Are the reported changes within the uncertainty range?*

Response: Thank you for raising this point. We have estimated the uncertainty of the elevation change rates following the method of Hugonnet et al. (2022). The results (Figure 2) show that, in the main section where reduced thinning was observed, the uncertainty ranges of the two periods mostly do not overlap. This suggests that the reduced thinning signal in this section is relatively robust. We have added this uncertainty assessment to the revised manuscript.

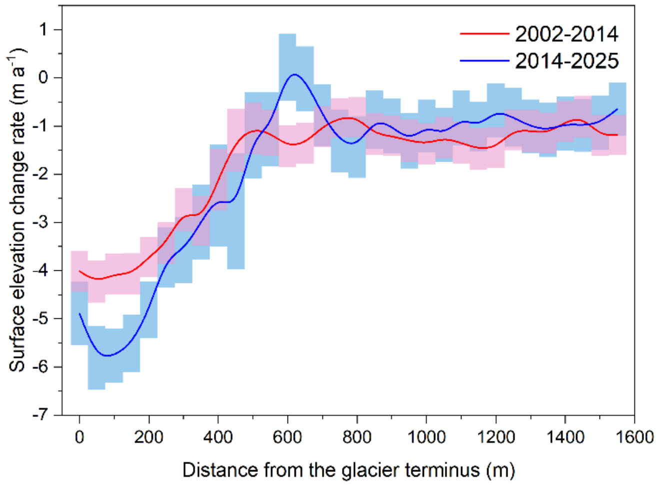

Figure 1 Surface elevation-change rates along the centerline of Jinwu Glacier during 2002–2014 and 2014–2025. The x-axis indicates the distance from the glacier terminus. Red and blue solid lines represent the mean surface elevation-change rates for 2002–2014 and 2014–2025, respectively. Shaded areas indicate the corresponding uncertainty ranges.

References

Zhang, G., Bolch, T., Yao, T., Rounce, D. R., Chen, W., Veh, G., King, O., Allen, S. K., Wang, M., and Wang, W.: Underestimated mass loss from lake-terminating glaciers in the greater Himalaya, Nat Geosci, 16, 333-338, 10.1038/s41561-023-01150-1, 2023.

Zheng, G., Mergili, M., Emmer, A., Allen, S., Bao, A., Guo, H., and Stoffel, M.: The 2020 glacial lake outburst flood at Jinwuco, Tibet: causes, impacts, and implications for hazard and risk assessment, The Cryosphere, 15, 3159-3180, 10.5194/tc-15-3159-2021, 2021.