
Reviewer #2

Thank you very much for your positive comments and suggestions. We have added statements for some problems, and made some revisions according to the review opinions in our revision. We thank the reviewer's keen insight on the revision of the manuscript very much.

Below is our response to the key points raised in the review:

* This study aims to model the stability of a temperate glacier by means of multi-coupling analyses, selecting Midui Glacier in southeastern Tibet as a test case. The region is one of the most concentrated areas of monsoon-type temperate glaciers in High-Mountain Asia, and investigating the stability of steep ice and ice-rock complexes here is timely: several catastrophic collapses have occurred during the past two decades (e.g., the 2000 Yigong and 2018 Sedongpu ice-rock avalanches).

Nevertheless, the manuscript does not address the key physical processes that govern the stability of Midui Glacier. Because the glacier terminates in a pro-glacial lake, the mechanical (and thermal) state of the ice tongue is strongly influenced by calving and sub-aqueous melt. Similarly, the stability of the icefall and the steep ice above it is controlled by the glacier's thermal structure and morphology, neither of which is constrained by in-situ data in the present work.

A useful template for the level of observation required is provided by Nanni et al. (2025, Nat. Commun.), who combined detailed field measurements with numerical modelling to demonstrate a positive feedback between surface ablation, crevasse opening and glacier acceleration. Ice-temperature measurements (October 2023–April 2024) are reported for a single sensor installed 1.2 m below the surface in the ablation area. Because substantial surface lowering is expected at this site, the actual sensor depth—and thus the interpretation of the temperature record—is highly uncertain. The locations of the supraglacial RTK (real-time kinematic) GPS stations are not shown, preventing readers from assessing the representativeness of the velocity field. No field evidence is presented to validate the modelled stress or temperature fields.

I strongly felt that the authors have not any ideas about the mechanism of glacier dynamics as they have a lack of background of glaciology. Taken together, these shortcomings indicate that the authors currently lack the observational basis needed to support the conclusions drawn from their numerical simulations. This study does not demonstrate a mechanistic understanding of glacier dynamics at Midui Glacier.

A: Thank you very much for your constructive comments. We agree that our original manuscript did not communicate the scope, assumptions, and observational constraints clearly enough. In the revised manuscript, we have substantially revised the Methods, Results, and Discussion to clearly define the model spatial and temporal domains and the physical processes. We corrected potentially misleading statements and strengthened the linkage between the discussions and modeling results. Finally, we have added a detailed description of the field observations used for model validation to explain the type, location and purpose of the instrument.

The specific contents of the modification are as follows.

(1) We agree that calving and sub-aqueous melt can strongly influence the thermo-hydromechanical state of glacier tongue. Our study is a 2D cross-sectional thermo-hydromechanical modeling framework for glacial dynamics and stability, rather than dedicated calving or submarine melt models. Therefore, we explicitly state that calving and sub-aqueous melt processes are not simulated, and our conclusions are limited to glacier cross-section stability evaluated by the THM model. We also revised Fig. 2 to distinguish between catastrophic IAs and lake calving.

(2) We agree that the stability of steep ice is controlled by thermal structure and geometry. In the revised manuscript, We explain that the model is a 2D cross-section extracted along the glacier centerline, and its geometry is derived from DEM and ice thickness data. The meaning of the governing equation is clarified, and the description of coupling model and boundary conditions is strengthened.

(3) The ablation of glacier surface does cause a change in the depth (1.2m) of the temperature sensor. Therefore, in the revised manuscript, we clarified the field settings and interpretation limits of temperature records. We specify the sensor to measure the near-surface ice temperature in the ablation area, which is mainly used to verify the change trend of ice temperature. We explain that model validation uses the observed time series trend and error metrics (RMSE and MAPE), but we do not claim that a single sensor constrains the full englacial thermal structure.

(4) We agree that the description of GNSS location in the manuscript may be confusing. Therefore, in the revised manuscript, we have clarified the configuration of GNSS. The GNSS measurement station is installed on the glacier, while the GNSS reference station is located on stable bedrock nearby. We have clarified this in the revised manuscript and in Figure 7.

To ensure the reliability of the field observation data, a comprehensive monitoring station was set up on the glacier (Fig. 7a). The monitoring station includes a meteorological monitoring system (temperature, precipitation, etc.), a temperature sensor at 1.2 m below the glacier surface, and a GNSS measurement station for glacier displacement. The GNSS monitoring station includes a measurement station and a reference station. Therefore, a GNSS reference station was established at the stable bedrock near the glacier (Fig. 7b), and the displacement monitoring accuracy was improved by differential positioning.

(5) We acknowledge that direct validation of stress fields is challenging in mountain glaciers, and we did not have in-situ stress measurements to verify the numerical model. Therefore, in the revised manuscript, we emphasize the use of multiple indicators to evaluate glacier stability, including displacement mutation, plastic strain penetration, generalized shear strain, and FOS, and discuss the uncertainty accordingly.

(6) We respectfully disagree that we lack any ideas about glacier dynamics. Our original Discussion did not sufficiently demonstrate mechanistic interpretation based on results and existing literature. In the revised manuscript, we substantially modified the discussion, add relevant recent literature, and link our findings to the mechanisms that have been established. Finally, we have compared the research results with the relevant research summarized in the introduction, and clarified how this study provides a new perspective and contribution to the existing glacier stability research.

We hope these revisions address the reviewer's concerns by clearly defining the study scope, correcting misleading statements, improving the documentation of observations used for validation, and strengthening the mechanistic interpretation.