

Specific comments

1. Dataset

The earliest event in the dataset occurred in 1940, not 1941 as stated in the abstract.

The dataset appears to be derived entirely from Zhang et al. without original data collection.

Huascarán 1970 is classified here as an ice avalanche, but this event is a rock-ice avalanche initiated by bedrock failure. The authors state they excluded rock-ice avalanches (Line 37), which is internally inconsistent.

The Mount St. Helens event is included in the dataset. This was a volcanic eruption followed by a lahar, not an ice avalanche triggered by seismic activity. Its inclusion requires justification or the event should be removed.

Several events contain multiple dates (IDs 4, 5, 12, 13, 18, 22, 28, 32, 35, 45, 48, 50, 56, 57, 63, 64, 70, 81, 82, 83, 87, 88, 89, 90, 92, 94, 95, 97, 98, 99, 100, 101). Given that the analysis is based on earthquake-IA temporal association, using multiple dates per event is methodologically incorrect.

Event ID 18 (Kolka) is dated 2006. The Kolka glacier disaster occurred in 2002. The 1902 Kolka event is a separate historical event. The date requires correction and clarification.

The dataset contains entries marked "/" in the seismic association field. This notation is not defined in the dataset documentation on Zenodo or in the manuscript. 13 events carry this marking. Its meaning must be clarified.

68 events are marked "None" and 81 events have no seismic association. The rationale for including events with no earthquake association in a study of seismic triggering is not explained.

The dataset contains magnitude values reported as Ms, Mw, and potentially mb (e.g., M3.92 for a 2008 event). Without conversion to a unified scale (preferably Mw), comparison across events is not valid. This affects the derivation of Eq. 1 and all magnitude-based analyses.

Glacier and event names are inconsistent across the dataset. Standardization is needed.

2. Text

Line 20: The definition of ice avalanche is poor and lacks references to foundational literature on glacier failure typology.

Line 25-27: The statement about seismic activity on the southeastern Tibetan Plateau is supported by a reference (Wood et al., 2024) that focuses on earthquake-triggered GLOFs in the Tropical Andes. This reference does not support claims about ice avalanche risk management on the Tibetan Plateau. A more appropriate citation is needed.

Line 34: Shugar et al. (2021) does not examine the role of earthquakes as triggers of ice avalanches at the global scale and does not compare seismic and non-seismic triggers. The conclusion drawn from this reference does not follow from its content.

Line 37: The authors state that rock-ice avalanches were excluded, but Huascarán is included in the dataset. This is a direct contradiction.

Line 56: M_s is used without definition or discussion of its relationship to M_w .

The epicentral distance threshold (Eq. 1) is derived from a subset of 20 events filtered by $M_s < 4.5$ and delay time < 2 months and is then applied to the full dataset to identify IA - earthquake pairs. This introduces a circularity problem because the same criteria used to define the calibration dataset are also used to derive the relationship applied for event selection.

The calibration subset of 20 events is insufficient for deriving a robust empirical relationship, particularly given the subsequent extrapolation to magnitudes well beyond the calibration range ($M_s > 4.5$). The wide confidence intervals in Fig. 1a inset reflect this uncertainty.

The choice of $M_s < 4.5$, 20 events, 2-month temporal window, and 30-day interval are all adopted without justification.

The Keefer (1984) relationship was developed for landslides in unconsolidated material. Its application to ice avalanches is not justified.

Earthquake magnitude and epicentral distance alone are insufficient proxies for seismic influence on glacier stability. Peak ground acceleration (PGA) should be considered, as it directly characterizes seismic loading intensity at a given location by integrating magnitude, distance, depth, and site geology.

The authors describe $p > 0.05$ as indicating a "statistically significant normal pattern." This is incorrect. A p -value above 0.05 in a normality test indicates failure to reject the null hypothesis, not confirmation of normality. Furthermore, the fitted normal curve in Fig. 1b poorly describes the observed distribution.

No baseline IA frequency is established for the study region. Plus, without climate data, the authors cannot distinguish whether the observed concentration of delay times reflects seismic triggering or seasonal climatic forcing coinciding with the post-seismic window.

The statement "the faster a glacier moves, the lower its stability" is an oversimplification. Anomalous acceleration relative to background velocity is the relevant indicator, not absolute speed.

The pre- and post-earthquake velocity intervals correspond to the same seasonal window but different years (2021 vs. 2022). Fig. 2d shows that October 2022 was warmer than October 2021 (+0.6°C), coinciding with the period of maximum acceleration. This interannual climate difference is not discussed. The observed velocity changes may at least partly reflect climate variability rather than seismic forcing.

Excluding temperature and precipitation as dominant drivers does not demonstrate that seismic forcing is the cause. Other factors, such as subglacial hydrology, interannual mass balance variability, internal glacier dynamics are not considered.

The velocity data are derived from satellite remote sensing, not field measurements. The term "field evidence" in Line 145 is a mischaracterization.

The conclusion that the Luding earthquake was the "main factor" responsible for velocity differences requires quantifying the relative contributions of all relevant drivers. This is not done.

The mechanical model is calibrated at -10 to -15°C. The applicability of the model to temperate or polythermal glaciers common in HMA is not discussed.

The model does not explain the observed delay timescale. Seismic loading dissipates within seconds, yet the proposed metastable state persists for weeks to months. This timescale gap is not addressed anywhere in the manuscript.

The zonation in Fig. 1c (delayed-triggered, critical-triggered, coseismic IA zones) is defined visually rather than through quantitative classification. Zone boundaries are not defined in the text, and no statistical test demonstrates that these groups are significantly distinct.