Response to the comments of Referee #1 and Referee #2 on manuscript egusphere-2025-1151

S. Weber et al.

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Dear Editor and Referees,

Thank you very much for giving us the opportunity to submit a revised version of our paper titled ”Progressive destabilization of a freestanding rock pillar in permafrost on the Matterhorn (Swiss Alps): Field observations, laboratory experiments and mechanical modeling”. We also thank the two referees for their insightful, constructive and positive reviews. We are confident that we can address all comments and incorporate them into the revised version and add a point-by-point reply to all major comments highlighted in blue on the following pages.

With kind regards,
Samuel Weber
On behalf of all authors

Reply to the major comments of Anonymous Referee #1

The article addresses a very current topic, i.e. the analysis of processes and mechanisms responsible for the instability of rock masses in permafrost, with particular emphasis on climatic drivers, and therefore on the potential impacts of climate change, and on monitoring/modelling procedures potentially useful for warning purposes. While many studies have shown a significant increase in rock slope instability under ongoing climate change, the specific mechanisms that cause individual instabilities are still poorly understood and little investigated, especially with regard to small-scale (but frequent) events. As a result, warning procedures still remain a challenge. The authors propose an approach based on the integration of field data, laboratory tests and numerical modelling, applied to the case study of the collapse of a rock pillar in June 2023 on the Matterhorn. The paper highlights the role of snow-melt water percolation within frozen rock masses, pointing out the occurrence of precursory signals a few days before collapse. The paper is structured in a clear and rigorous way; objectives, data and methods are well illustrated, and the conclusions are adequately supported by the results. Below are some considerations aimed at clarifying some points and further improving the excellent overall quality of the manuscript.

Issue 1: L140: "A Vaisala WXT520 weather station is available both at the rockfall zone at 3500m asl as well as at the Solvay Hut at 4003m asl": What kind of data are recorded by these stations? Is precipitation included? Precipitation data are not mentioned in the text, although rainfall has been indicated as one of the potential drivers of thawing (see L383): the paper doesn't specify whether rainfall occurred in the days preceding the collapse, and only snowmelt is considered responsible for the percolation of water into the frozen rock mass.

Reply: In the revised manuscript, we specify the recorded data types as follows: *"A Vaisala WXT520 weather station is available both at the rockfall zone at 3500 m asl and at the Solvay hut at 4003 m asl, measuring air temperature, humidity, wind speed and direction, liquid precipitation, and hail."* While precipitation data are included in the analysis, they are not considered in the interpretation, as there was virtually no rainfall during the six weeks preceding the collapse (for details, see Fig. 3h).

Issue 2: L255: "While both the rock and air temperatures in spring 2023 and the month of failure (June 2023) are within the range of the longterm average (see Fig. 3 and supplementary Fig. S3). . . ": actually, according to figure S3, the rock temperature shows an interesting trend in 2023, always remaining around -0.5 °C in the months 1-6 at 2m depth, which corresponds to the ALT in 2022, while in previous years it reached a minimum between -1.5 °C to -2.5 °C in April. Furthermore, Fig. S3e shows that at 0.6 m rock temperature increased significantly in the 2 weeks preceding the event. I think these data should be shown in Figure 3 and mentioned and discussed in the paper.

Reply: We moved Fig. S3 to the manuscript, but without merging with Fig. 3, as we want to separate environmental from ground conditions. We further rewrote and clarified the beginning of the paragraph: *"Both the rock and air temperatures measured on the Hörnligrat ridge in spring 2023 and the month of failure (June 2023) are within the range of the long-term average (see Figs. 3a-c and Fig. 4a+b). In contrast, the rock temperature at 2 m depth measured near the Hörnli hut was remarkably high in the winter preceding the failure (Fig. 4c+d). No peculiarity could be detected in ..."*

Issue 3: L256: "the humidity in spring and June 2023 is remarkably high": this observation has not been discussed nor taken into account in the rest of the paper, you may consider to remove this information or you should include in the discussion.

Reply: We agree and have removed this statement from the text.

Issue 4: L366: "rapid decrease in snow depth at the nearby IMIS weather station Stafelalp/ZER4, see supplementary Fig. S7": to make this observation more evident, it would be useful to add a bar in Fig. S7 corresponding to the day of the rock pillar collapse, as it has been done in most of the figures. Furthermore, the dynamics of the snowpack does not appear different compared to previous years: it would be appropriate to mention this point and take it into account in the discussion.

Reply: We have optimized the figure according the suggestion. We further clarify in the manuscript and supporting materials that the nearby IMIS weather station Stafelalp/ZER4 (located 1000 m below the study site) supports the rapid decrease in snow depth in 2023.

Issue 5: L414: "The clear diurnal pattern of short (¡ 9s) seismic pulses implies freeze and thermal contraction as the main drivers of those events". Isn't the observation (Fig. 10b) that the short seismic pulses increase in the pre-collapse period, which in the diurnal cycle are linked to the nocturnal freezing, in contradiction with the thesis supported by the paper that it is the melting triggered by the percolation of snow melt water in the frozen mass that causes the triggering of rock pillar instability?

Reply: Correct, we have removed the fragment *"freeze and"* so that the sentence now correctly states that it is thermal contraction rather than freezing that causes the diurnal pattern of short seismic signals.

Issue 6: For data and method comparison purposes, did you consider the work of the group of Magnin et al. on similar topics? (e.g. Magnin, F., & Josnin, J. Y. (2021). Water flows in rock wall permafrost: A numerical approach coupling hydrological and thermal processes. Journal of Geophysical Research: Earth Surface, 126(11), e2021JF006394; Ben-Asher, M., Magnin, F., Westermann, S., Malet, E., Berthet, J., Bock, J., ... & Deline, P. (2022). Estimating surface water availability in high mountain rock slopes using a numerical energy balance model. Earth Surface Dynamics Discussions, 2022, 1-25).

Reply: We consider the studies by Magnin & Josnin (2021) and Ben-Asher et al. (2023) in the revised manuscript.

Final remark: We are addressing and clarifying all minor/technical corrections in the revised manuscript.

We thank the reviewer for the constructive and helpful feedback, which has contributed to improving the quality of the manuscript.

Reply to the major comments of Anonymous Referee #2

This study provides a comprehensive, integrated multi-method analysis including field observations, laboratory findings, and mechanical modeling investigating the early destabilization to the final failure of a single free-standing rock pillar.

There are very few rockfalls with precursory observations, therefore this study is particularly interesting as it provides multi-disciplinary in-situ observations (GNSS, inclinometers, seismometers, weather data). These observations are essential to develop methods for forecasting such events and to understand the physical mechanisms that promote failure.

In my field, seismology, I found the analysis and the choice of parameters were appropriate and correctly justified. Overall, I found the manuscript very clear. I only have minor suggestions to improve this study.

Issue 1: While the displacement data show a clear acceleration before failure, which can be used to estimate the failure time, the results of the seismic analyses are not so convincing. The data is only analyzed for 13 days before failure. Why not including all data since installation in 2019? I understand that the manual classification of seismic events is time consuming, but only a very small fraction of events is removed. Would the results change significantly if the authors used only the results of STA/LTA detection without manual validation?

Reply: It is true that the time period covered by seismic data is shorter than the displacement data. We agree that extending the products of the seismic analysis (picked events, dv/v, energy rate) can be achieved with low to moderate effort further back into the past (resulting in strong seasonal patterns and the event statistics without manual filtering are strongly influences by mountaineering activity – see Meyer et al. (2019, `https://esurf.copernicus.org/articles/7/171/2019/`) for more details). However, the topical focus of the paper is on the precursor signals during the final stage towards the event, throughout the last ten days with the marked onset of acceleration (Figs. 7–9). We now emphasise in the manuscript that we use the seismic data mainly to better constrain the mechanisms within the rock mass that lead to the surface expression (displacement, inclination), which we can measure independently. We do not necessarily rely on seismic time series only to predict the time of the failure.

Issue 2: The results of the dv/v and energy rate are fully automatic and rather fast to compute. I strongly encourage the authors to look at all available data (at least for dv/v and energy rate) in order to analyze seasonal fluctuations, to quantify the normal variability of each variable and to analyze wether the precursory fluctuations observed in the last days before failure are really unusual.

Reply: In line with the above comment, we agree that extending the seismic analysis further into the past would be a minor effort. But likewise, we would not anticipate major insights to the behaviour of the system from doing so. As correctly indicated by the referee, there will be a superposition on seasonal trends, diurnal trends, weather-related trends and perhaps further (unknown) effects that affect each of the seismic proxy data individually and in combination. Exploring years of such information and studying possible periods when common trajectories of change emerge (sensu Leinauer et al., 2023) would be a viable study on its own and shift the focus of the current study quite far from the current one. We add a sentence at the end of section 5.1 that picks up this information, to be concise to the readers.

In fact we have not argued that each of the seismic variables necessarily show unusual activity. It is rather the combined systematic pattern of several proxy data that adds value to the understanding of how the system propagated towards the failue (cf. first sentence of section 5.1: *"The combination of GNSS, TLS, inclinometer, thermal, ambient environmental and seismic data provides a comprehensive understanding of the processes leading to the failure of the rock pillar."*).

We agree with the referee's opinion that it is likely that each precursor proxy may fluctuate also months or years before the failure. Evidently, inclination, displacement and imagery only start to evolve significantly beyond the range of previous years in the last days.

Issue 3: On l308-309, the authors claim that *"The short events reach a short-term maximum rate around 4 June, followed by a second rise towards the failure of the rock pillar."* I don't agree with this statement. In Fig 10b, the strongest peak occurs about 2 days before failure, then the rate of events decreases until failure. How do you explain this pattern? Possibly seismic events are associated with fracture propagation; and during the latest days before failure, displacement may occur as aseismic shear along these fractures? See also l415-417.

Reply: Correct, we have modified the misleading sentence to: *"The short events reach a short-term maximum rate around 4 June, followed by a second rise until 1–2 days before the collapse of the rock pillar."* In the discussion (section 5.1), we have added the suggested explanation as: *"This trend reversed abruptly 1–2 days before failure, with a 5% decline in velocity, indicating rapid critical destabilization. That destabilisation and increasing displacement rate (see Fig. 7) apparently occurred under only minor emission of short-lived seismic signals (see Fig. 10b)."*

Issue 4a: Evolution of seismic energy rate (Figure 8). Did you filter the data before estimating the energy? Could you test different frequency ranges?

Reply: We calculated the evolution of seismic energy across different frequency bands and present the results for the 2–4 Hz and 5–20 Hz bandpass filters. The manuscript has been revised accordingly: *"Based on ambient seismic noise data exhibiting typical diurnal fluctuations, we could detect a distinct increase in the lower noise level (5% percentile over the last 24 hours) with lead time to failure of 9 days in the frequency bands 2–4 Hz (see Fig. 9a) and 20–80 Hz (see Fig. 9c) although the increase persists for a shorter time in the 5–20 Hz frequency band (see Fig. 9b). Further short-term increased energy levels of this type were already observed on 5–7 June."*

Issue 4b: Could you add a figure showing typical waveforms at all sensors for both short and long events? Is the amplitude generally stronger at the station closest to the rock pillar? Can you identify P and S waves? Is the temporal evolution of short events different if you select only events with a strongest amplitude at the closest station?

Reply: In the revised manuscript, we have added a two-panel element of typical seismic waveforms in Figure 11 and mention the other questions (amplitude differences, lack of P- and S-waves as expected for such signals) in the results section (l. 300): *"... between short and long events at 9.0 s. The two event clusters (see Fig. 10d-e for representative examples) show clear differences in amplitude between the seismic stations, with the highest ground velocities at the station close to the rock pillar, and they lack distinct P- and S-wave arrival times.*

Issue 5: Experiment: How do you explain the large variability between friction angles (50-80°) for air-dried no-cohesion frozen samples?

Reply: In the initially submitted version of the study, friction angle tests were conducted for saturated, moistened, and air-dried discontinuities. For the revised manuscript, we have chosen to omit the air-dried condition due to its limited relevance under the field conditions investigated, where discontinuities are rarely completely dry because of persistent sub-surface humidity. Additionally, in the air-dried state, the influence of surface roughness is disproportionately high, potentially overstating its role in frictional resistance. In contrast, the moistened and saturated conditions more realistically reflect in-situ environmental moisture levels and the mechanical behavior of discontinuities. We therefore focus on these two scenarios, which better represent the natural range of conditions encountered in the field. We explain our assumption in the revised manuscript as follows: *"Dry conditions are not considered in this study because natural alpine bedrock is generally close to saturation. Field measurements and previous studies indicate that only the uppermost 8-10 cm of the rock surface may intermittently get drier due to atmospheric exposure (Sass, 2005). This assumption is particularly valid in permafrost or seasonally frozen rock mass, where freeze-thaw processes further promote moisture retention. Specifically, ice segregation during freezing induces suction forces that draw water toward the freezing front, thereby increasing pore water content and enhancing saturation. Consequently, the bulk of the rock volume in permafrost environments remains at or near full saturation, rendering the influence of dry conditions negligible for the purposes of our experimental setup and modeling framework."*

Issue 6: Numerical model. How to explain the difference in amplitude displacement by a factor of 1000 between the model and observations? Which parameter of the model could you modify to better explain the observations?

Reply: The mechanical toppling behavior can be replicated on a field scale with certain assumptions and simplifications. We discuss the discrepancies in the revised manuscript: *"The magnitudes between measured and modeled displacement of the rock pillar differ due to the following reasons: Upscaling of laboratory-deviated shear parameters from decimeter to decameter size at the field site represents a distinct simplification, neglecting natural irregularities in the geometry of joints such as presence of rock bridges (limited joint trace length), the heterogeneous distribution of discontinuities or varying joint roughness. In addition, other factors, such as hydrostatic pressure or thermal expansion/contraction of rock that may have acted under natural field conditions, were excluded in the UDEC modeling approach."*

Issue 7: Figure 10. Why is there a peak at 0 and 24 hrs for both short and long events? Is it real or a problem of side effects?

Reply: We add a clarifying sentence to the methods description to make sure the existing peak is not misinterpreted as an artifact: *"The remaining events were analyzed by their durations and time of occurrence, both for the total time span of interest and at the diurnal scale. To avoid edge effects in the kernel density estimate of the circular diurnal data, we expanded the diurnal event timings by copies of those timings -24 hours and +24 hours, before calculating the density estimate and then truncating it at 0 and 24 hours again (cf. Dietze et al., 2017)"*. We have also added a sentence to the discussion section: *"The contrasting diurnal activity of long (mainly during day time but also peaking around midnight) and short (mainly during night time) events points at thermal forcing of crack propagation and mass wasting, in agreement with other studies (Collins and Stock, 2016; Dietze et al., 2017; Weber et al, 2018)"*.

Issue 8: Figure 7. "Boxplots give information on all forecasts since the OOA per velocity window." I don't understand precisely what is shown in these plots?

Reply: In the revised manuscript, we explain these panels with boxplot in more detail: *"Boxplots include all forecasts since the OOA per velocity window (colored points in the life expectancy plot in panel (a) indicating estimated time of failure) with median as black vertical line, interquartile as box, 1.5 times the interquartile range of the minimum or maximum data point as whiskers, and red diamonds date to the latest forecast."*

Final remark: *We thank the reviewer for the constructive and helpful comments that significantly improved the clarity and quality of our manuscript.*