General comments

"Rockfall triggering and meteorological variables in the Dolomites (Italian Eastern Alps)" by Bonometti et al. aims to investigate the relationship between meteorological variables and rockfall occurrence in the Italian Eastern Alps. Specifically, a statistical modelling approach based on previous studies and on the Bayesian method has been exploited to assess the frequency of meteorological variables in the ongoing context of climate change and as potential triggers of rockfall events in the studied area in the period 1970-2019. Alongside, an in-depth analysis of the impact of long-term meteorological trends of air temperature, precipitation, temperature variations and freeze-thaw cycles at different aggregation scales on rockfall occurrence has been provided.

The manuscript represents a valuable and innovative contribution to the understanding of meteorological variables-related impact on the rockfall risk. Results of this study are very interesting and properly compared to previous works on the same topic. Despite not being a mother-tongue, I think that the paper is in general well-written.

However, I believe that some major revisions are needed to enhance the overall quality and clarity of the paper before acceptance for publication.

While the methodology is generally well-explained from a technical point of view, there are some assumptions and flows that need clarification. Further, the paper would benefit from a more logical structure, particularly in the Results and Discussion sections. Some parts of the methodology are discussed in the Results, which is not consistent with the purpose of the section. Similarly, some results are overlooked in the Results section and rather included in the Discussion. The Introduction needs some refinements, to better highlight the aim of the work and its novelty with respect to previous mentioned works.

Thanks to the reviewer for the encouraging comments. We modify the manuscripts according to your suggestions.

Further details are given below.

Specific comments

Abstract

L 18: This sentence is not clear, please rephrase.

Thanks to the reviewer. We rephrase the sentence adding the definitions of temperature amplitude and temperature variation to clarify the meaning.

Introduction

Rather than focusing on limitations of the previous method, it seems that this is addressing a different aim. The mentioned works concentrate on the probability occurrence of extremes in the meteorological variables leading to slope failures, using a non-parametric approach to link meteorological anomalies to landslides occurrence. In contrast, this work shifts the focus to long-term trends and frequency over time, specifically investigating how these trends influence rockfall occurrences, especially considering the impacts of warming and changing weather patterns. I think that the primary aim is to assess how the variation in climate variables over time and space affect changes in rockfall frequency, rather than identifying the role of specific climate extremes as triggering events. I invite the authors to refine this part to better highlight the purpose and novelty of the work.

Thanks to the reviewer for comments about the aim of the work.

We add the following sentence:

"However, during periods of climate change, variations in frequency occur across the entire range of meteorological variables, not just at their extremes. These changes can also influence the onset of rockfalls. The aim of this study is therefore to calculate the spatial and temporal frequency variations of various climate variables in the eastern Italian Alps in order to understand climate evolution and its impact on the distribution of rockfall frequency at different elevations. To this end, we propose a new method that builds on Paranunzio's (2015) methodology to include the frequencies of both anomalous and non-anomalous climate variables affecting rockfall events. This refined method was applied to a comprehensive database of rockfall events within the study area."

L62-65 This part is more suited in the methodology section.

Thanks to the reviewer. We simplify this part without explaining the corresponding technique in detail. The new version is:

"This method, based on ranking climatic variable values (in ascending order for a specific scale aggregation) and computing a probability of anomalies, links extreme climate values with rockfall events, but does not fully account for their frequency"

Case study

A concise overview of the climate of the study area is suggested. Some text from the discussion should be moved here to better contextualize the study area from a climatological point of view.

Thanks to the reviewer. We had added a briefly climate context.

"The region's climate is alpine with continental characteristics and exhibits significant local variations due to microclimates. The Belluno Valley and the Po basin in the Friuli Venezia Giulia Region are influenced by humid breezes from the Adriatic Sea (Desiato et al., 2005). In contrast, the internal mountainous areas experience a typically continental climate, characterised by cold winters and mild summers. Rainfall mainly occurs as brief summer thunderstorms, whereas autumn rainfall is more prolonged (Coro et al., 2015; Frattini et al., 2008)."

L 111 Please provide references for these datasets if available.

Thanks to the reviewer. We add references to the different datasets.

L115 Are you thus considering all landslide events in the region regardless of the elevation? Please clarify.

Thanks to the reviewer. We have a dataset containing 5628 landslides but this dataset is partially incomplete. Consequently, in the work here proposed we consider only 2971 for which we have all required information. Regarding the elevation, we stratify the analysis as a function of elevation classes to account for different conditions.

Method

As far as I understand, the method focuses on the frequency of meteorological values within their characteristics value ranges in the period preceding slope failure occurrence. A more detailed explanation of these "characteristics value ranges" and how they are defined should be provided.

Thanks to the reviewer. We forgot to specify the meaning of "characteristic value ranges". In the manuscript this range stands for the interval between the maximum and minimum of the timeseries regarding the considered meteorological variable. We specify this concept in the manuscript as follows:

"Differently from Paranunzio et al. (2015, 2016), which focused on identifying anomalies in meteorological variables timeseries, this method emphasizes the frequency of statistical samples of meteorological variables within their characteristic value ranges defined as the interval between the maximum and minimum values obtained from the used timeseries."

The methodology is partially based on Paranunzio et al., particularly the time series sampling approach at different time aggregation scales. Then it differs by applying a Bayesian method to assess the relative influence of a variable to act as a trigger of a rockfall in terms of conditional probability. Thus, it seems that the computation of the non-exceedance probability, using a defined alpha level (as stated in the Introduction in reference to previous works) for the detection of potential anomalous values (in statistical sense) is not fully addressed in this paper. However, the outcomes of this approach are then presented in the discussion (L 553 on, Fig. 21.). This creates some confusion, as the linkage between the methodology and the results is not clearly explained. The paper should provide a clearer explanation of how this method connects to the presented results.

The reviewer is right. We did not introduce a modification of Paranunzio et al. method because we do not compute the probabilities of anomalies of meteorological variables in another way. We used the approach of Paranunzio et al. to generate statistical samples of variables, that is the aggregation scales approach, and we integrate the meteorological variable used by Paranunzio et al. (2015, 2016), by adding the following variables: temperature amplitude, icing and freeze/thaw cycles.

In the proposed method we considered the frequencies of all computed statistical samples.

L 127 The concept is not clear to me: meteorological variables like e.g. temperature are continuous variables, thus what does the premise" this method focuses on the frequency of meteorological values" mean exactly?

Thanks to the reviewer for your suggestions. Meteorological variables are time continuous variables but they are measured with sampling frequency obtaining the recorded timeseries. We manipulated these timeseries obtaining the timeseries of statistical samples, that is the same aggregation scales used by Paranunzio et al. (2015), then we computed the frequencies of the statistical sample's values. In the manuscript the term meteorological variable refers to recorded data.

The manuscript is modified as follow:

"Differently from Paranunzio et al. (2015, 2016), which focused on identifying anomalies in meteorological variables timeseries, this method emphasizes the frequency of statistical samples of meteorological variables within their characteristic value ranges defined as the interval between the maximum and minimum values obtained from the used timeseries."

Eq (4) Define *j*

Thanks to the reviewer. Index *j* is the summation index as specified in the summation symbol.

L 208 In my opinion, this statement is based on a flawed assumption. The method illustrated in Paranunzio et al. is a statistical approach based on the detection of meteorological anomalies (percentiles). As such, this allows to remove possible bias in the absolute rainfall estimates. Paranunzio et al. compute the probability distribution using the climate data recorded at the reference stations as they are and did not transpose the temperature or precipitation measurements from the meteorological stations to the geographical location and elevation of the rockfall detachment zone. This is because the application of a constant lapse rate (as in the case of temperature) would only shift the values, without influencing the probability estimate associated with V. Therefore, I think that it is not accurate to claim that the method presented in this work addresses an issue overlooked in previous methods. Rather, they provide an alternative approach to handling spatial variability in absolute values instead of percentiles. This point needs to be clarified.

The reviewer is right, the method takes some element of Paranunzio with a different approach to assess the variation of climate variables during time in a large region. We remove that sentence to avoid any misunderstanding.

L 221 Generally, an environmental lapse rate which considers air temperature decreasing with height at a rate of approx. 0.6 °C/100 m is used, but this does not take into account that the warming rate increases with elevation (see some suggested references below). It is worth to briefly discuss it in this section.

Pepin N, Bradley RS, Diaz HF et al (2015) Elevation-dependent warming in mountain regions of the world. Nat Clim Chang 5:424–430. doi:10.1038/nclimate2563

Nigrelli, G., Fratianni, S., Zampollo, A., Turconi, L., & Chiarle, M. (2018). The altitudinal temperature lapse rates applied to high elevation rockfalls studies in the Western European Alps. Theoretical and Applied Climatology, 131, 1479-1491.

Thanks to the reviewer for this consideration and for the suggested references. The above-mentioned papers investigate the temperature variation with altitude. Some investigated the meteorological variables considering their evolution in an hourly frequency, whereas others considered all interactions acting at high elevations such as water vapour, clouds or albedo which generates thermo-dynamical effects due to mutual warming between air and rocky outcrops.

It is author's opinion, for the aim of this work, the above mechanism are too complex to be investigated with the considered variables that takes in to account only daily mean values, thereby recording these mechanisms globally and contemporary.

The linear approach adopted in this work takes into account the action of all above mechanisms (Angot 1892; Dodson and Marks 1997; Barry and Chorley 2009) in a simply manner and globally.

The issue is reported in the manuscript as:

"This simple linear approach, which is based on a constant vertical gradient, has been used despite the fact that is expected that warming in mountain regions depends on elevation (Pepin et al., 2015; Nigrelli et al., 2018; Pepin et al., 2022)."

L 212 The Delaunay method assumes a smooth transition among points, but temperature gradients can be non-linear, especially in regions characterized by microclimates or highly varying topography (as raised in the previous point). The authors should be cautious of how elevation is included in the model, especially in mountain regions with high complex topography and when weather stations with high elevation difference are used. Moreover, this triangulation assumes uniformity in space, this means that stations should be distributed in a reasonably uniform manner. In the case of sparse station networks or if the stations are unevenly spaced, as often occur in complex terrain like mountain regions, the method could not accurately represent the spatial variation of the variables, leading to skewed results. Also, the sensitivity to outliers should be considered (that is, the fact that interpolation process could amplify these errors).

Thanks to the reviewer. We are aware that the complexity of the topography in mountain regions significantly affects the climate, generating micro-climates with non-linear variations in climate variables across the landscape. In fact, as shown in Figure 1b, the meteorological stations in our study area are irregularly distributed and decrease in number at higher altitudes. However, this problem also affects other approaches used in the literature, such as using the nearest meteorological station (Allen and Huggel, 2013; Paranunzio et al., 2015; Nigrelli and Chiarle, 2023), when the stations are located tens of kilometres away from the landslide points.

We believe that triangulation partially compensates for this issue since it uses time series data from a greater number of meteorological stations around the landslide point. Weighted linear interpolation is a strong approximation, but it is the simplest approach in the absence of more detailed information. More complex weighting according to distance could be introduced without invalidating the approach.

The issue is reported in the manuscript as:

"This triangulation approach partially compensates for the problem of sparse weather stations that may be distant from the landslide points. However, it assumes an even spatial distribution of weather stations and a linear trend in the variables, both of which may not be accurate in complex terrain."

Figure 2 Define A and B

Thanks to the reviewer. We add in the figure caption the meaning of point A and B, as follows:

"Points A and B are the inclined projections of the point P along the edges $\overline{S_1S_3}$ *and* $\overline{S_1S_2}$*."*

Results

L 239 Did the authors set a minimum record length in the period 1970-2019?

Thanks to the reviewer. In fact, we consider only the meteorological stations with timeseries without any data voids during the period from 1970-2019. We modified the manuscript as follows:

"For the purpose of this work, three sets of meteorological stations were considered.

Set A comprises all 277 selected stations and was used for the Bayesian method to analyse the frequency of climate variables.

Set B contains 18 stations chosen from the original 277. These stations were specifically selected because they have a complete time series spanning the entire period from 1970 to 2019 with no data gaps. Results for mean air temperature and precipitation are presented at a 90-day aggregation scale, while results for freeze-thaw cycles are presented at 7-day scale. This enabled the observation of detailed short-term changes while avoiding overlap with other months. Additional results are provided in the supplementary materials for completeness (S1).

Set C consists of 12 weather stations with complete time series. These stations were selected to analyse long-term trends at different elevations (below 1000 m, between 1000 and 2000 m, and above 2000 m a.s.l.). To ensure the selected stations were homogeneous, four stations were chosen for each elevation range. Two distinct periods were considered: 1970–2019 for stations below 2000 m and 1985–2019 for stations above 2000 m a.s.l.."

Figure 4-5 It is not clear to what trend the arrows refer to, please clarify.

Thanks to the reviewer. The arrows stand for the frequency trend observed in that sub-interval of the considered variable. We update the figure caption to specify it

L 315-316 "which delay summer and advances winter" sounds misleading, please rephrase.

Thanks to the reviewer. We modify the manuscript as follows:

"These variations observed at different elevations can be attributed to the linear decrease in temperature with increasing altitude. This delays the end of the summer months and brings forward the end of the winter months at higher elevations. Similar patterns can be seen at other aggregation scales, as shown in the supplementary materials (S1.5)."

L 356 Not sure the aim here is to assess a correlation in statistical sense.

Thanks to reviewer. We modify correlation with relationship.

L 364 Not clear if the authors are referring to total precipitation or precipitation intensity (also in Figure 11).

The reviewer is right. It is not the rainfall intensity but the total precipitation. We modify the figure label accordingly.

Discussion

Some key findings are presented in the Discussion rather than the Results section, which diminishes the clarity of both parts. The Results section should be dedicated solely to presenting the outcomes of the analysis. Outputs should be moved in the dedicated section, leaving comments and comparison to other works here. As an example, Section 5.1 Climate which addresses changing climate patterns and long-term trends over the last decades in the area is not suited to the Discussion section in its current form (additionally, a climatological introduction of the study area should be included in the Study area section). Similarly, some parts of the methodology are presented for the first time within this section, which contradicts its intended purpose.

Thanks to the reviewer. We modify the manuscript to improve the results and discussion sections. Some of the text was rearranged accordingly.

L 506-519 The RAPS method is mentioned in the discussion for the first time, but it should be included in the methodology, since it supports results and conclusions of the work.

Thanks to the reviewer. We move the mention to RAPS method in the methodology.

L 537 How did the authors measure the correlation?

The reviewer is right. We identify a relationship, and we do not measure a correlation. The text is changed.

Figure 17-20: these and related description are more suited for the results section (e.g., L 452-457, L 485-489).

The reviewer is right. We move it in the results section.

Figure 21 As in the previous comment, results of this analysis (L 560 on) this should be anticipated in the Results section and briefly contextualized in the Discussion.

The reviewer is right. We move it in the results section.

Technical corrections

L 1 Alpine areas are undergoing "a high change" in…

L 12 An anticipation of "both the onset of summer and the end of winter"…

L 15 over "the" last…

Figure 11a) Correct "conditional"

Figure 10 Please indicate the elevation ranges in d)

Thanks to reviewer for the technical corrections. We modify the manuscript accordingly.