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Comment on nhess-2021-128

Anonymous Referee #2

Referee comment on "Multi-method monitoring of rockfall activity along the classic route up Mont Blanc (4809 m a.s.l.) to encourage adaptation by mountaineers" by Jacques Mourey et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-128-RC2>, 2021

The authors present an empirical study of rockfall activity and potentially responsible environmental conditions as well as a link to the affected part of our society for a study site in the French Alps. They describe seismically detected rockfall rates and link the seasonal and diurnal patterns to several possible drivers and triggers. The general scope of the manuscript addresses indeed a timely and valuable research gap. However, I am afraid that the actual conduction of the study and especially the quality of the manuscript are a fair bit from suitable for prompt publication. The presented study is in general very premature and unorganised, and in many cases simply speculative due to unsupported claims, breaks in argumentative logic and lacks of reasoning. I shall note that most of these shortcomings can be accounted for in a thorough revision, and that the structural flaws can be managed, which would eventually render the study in principle for publication.

The title is actually a good example to illustrate the flaws mentioned above. While rockfalls are indeed addressed, I did not find any robust analysis of the vulnerability. There is no quantitative exposure analysis or an estimation of the change in exposure with changing boundary conditions (season, weather conditions, climber passing windows etc.). Likewise, I do not see a deep expression of interdisciplinarity. Most of the material presented stems from evaluation of instrument data deployed on site. There is neither a real link to adjacent scientific fields nor a scientifically valid analysis/evaluation of the social scientific aspects of the study. Finally, although this is just an example, the title gives a lot of attention to the site, more than 50 % of the words used. To me this reads mostly like a study by Mont Blanc enthusiasts.

Fundamentally, I missed a clear hypothesis that the authors wish to test in pursuing their research. Accordingly, the introduction did not gain sufficient momentum to motivate and justify the study and to provide the right background. Sadly, the interesting link to the societal relevance (l. 38-44) is not really picked up later in the article. The scope (l. 65-66) thus simply appears to be: collecting a site specific catalogue of rockfall events and test if their occurrence pattern is in agreement with drivers and triggers that the community has reported on already a long time ago. Overall, the study was quite disappointing in just reporting findings that the scientific community has already embraced for years to decades. At least this is the implication of the discussion. Without more emphasis on novel findings, the material reduces itself to a case study. I encourage the authors to revise their text to avoid this (mis)interpretation and make clear where the study contributes new, original findings.

Section 2 (rockfall triggers) is very exhaustive and not really aligned to the context of the study. It reads like a review of rockfall triggers, but I could not really find where it points the reader to existing knowledge gaps (hence research opportunities) or where it motivates the methodological approach employed in the study. I suggest to significantly shorten this section and add concise support for the actual study. Why is it important to mention all these drivers and triggers? And how does this background contribute to a better emplacement of the study? In addition, I advise the authors to clearly distinct between drivers and triggers, as these are two fundamentally different terms and they should not be used interchangeably or mixed in their appearance throughout the text.

Section 3.1 is quite long and reads like a glossy promotion of Mont Blanc for tourists. Either establish a clear link to the questions pursued by the study or remove the section. In other words, this section is only needed if better motivated and linked to the objectives.

The study contains several argumentation weaknesses. One example is the link between number of accidents and rockfalls occurrences. The authors have argued that the number of accidents scales with the number of climbers on track, which is logical. But how have the authors constrained the link between rockfalls and accidents? There is not even any information if the accidents are due to rockfall injuries, at all. See my detailed comments for further argumentation issues.

The general reasoning (l. 17-19) is pulling the straw man argument. The authors claim that the processes in their study area are "intense" (whatever that means) and claim that yet there has been limited research in this area. This is not a valid argument. Many other regions of the Alps and other mountains in the world have been studied intensively, so why engaging with just another case study – especially since it seems to show the same trends as most of the other global sites, already studied before? Please revise and provide a proper justification for the study. Just saying we worked there because no one else has, is not enough. You can easily pull the climbing route motivation, here.

The description of the methods is not always sufficiently clear to be reproducible. For example, the parameters or criteria used to define rockfall events from the seismic time series is vague. Specifically, l. 177-179 leave a lot of questionmarks: which waveform or spectral criteria were used? What did the control events look like? How were they constrained in the field? What software was used? And so on. Similarly, the beam forming method needs to be described more rigorously and also with a few more sentences of information for people unfamiliar with this technique. Which filter windows were used? Which wave velocity (or slowness)? How much time before onset and after event end were added? How were the signals pre-processed in general? Were there always data from all stations available?

I do not see how the authors were able to extract rockfall volume from the seismic data. I am not even sure how they extracted the energy of the impacting rock mass. How has the spatially mobile source been taken into account? Which wave attenuation model has been used? See for example the description of Le Roy et al. or the scaling efforts by Hibert et al. (all cited) for a proper way of rigorously describing an approach. I strongly suggest to expand that section and either explain how volume has been constrained or leave the link to volumes. In a similar manner, I am not sure the description of how oblique imagery was converted to aerial extent of snow allows reproduction of the results with the currently provided additional information. Please make sure, the readers can reconcile your analysis steps to be in a position that allows you to draw proper conclusions. A further example of unclear methodological clarity occurs in l. 234-235. Please give the numbers you used to constrain your model. Which values have been taken from "geotechnical surveys" and which reference needs to be added here? It is frustrating to be asked to take these words at face value without any chance to check for their appropriateness, not to speak of the model code used for the thermal modelling.

The presentation of results is often biased by unmotivated and apparently arbitrary classifications of continuous data into clusters. See for example l. 253-257. Why not simply plotting histograms, density estimate plots or boxplots of for example the energy of the events? A similar example is the definition of three seasonal periods (fig. 8, l. 318-321). Why three and not four or five? Or, why designating groups from continuous data first of all?

The discussion, especially the drivers of rockfall activity (l. 375-395) is very speculative and is lacking support by the own findings. This flaw is already reflected by the wording ("It is likely that this difference...", "probably all the more active...", "It is likely that nocturnal...", "It probably leads to the cementation..." and so on. I understand that the measured variables do not allow to pin down these unknowns. This is fair enough but then the discussion should not dive too deeply into this direction. If the data do not allow to constrain the mechanisms of rockfall initiation then the discussion should not put too much emphasis on this question. Another very speculative, or at least not well constrained, claim is given in l. 434-436: I do not agree that the data allow to claim that climbers trigger rockfalls. A further quite speculative section is at l. 470-476; this discussion about potential future trigger dynamics is simply beyond your presented data. I suggest to remove this part (and other parts that are not direct derivatives of your own data).

The language is a fair bit from acceptable for publication. I started to mark typos, awkward phrases and wrongly used references but gave up after a few pages. I strongly suggest the authors seek the advice of a native speaker and, more importantly, that they check their material for the numerous small technical issues (spelling, punctuation, consistency, referencing, figure font size).

l. 1-3, the title is way too long and focused on the case study. I suggest to reduce it by about 50 %, focusing on the research question, not the study site.

l. 14, what are "rockfall destabilisations"?

l. 17-18, what means "intensity of the geomorphological processes at work"? Please use a clear scientific language.

l. 24, quantify the term "frequent", you have the numbers at hand. Make the abstract as conclusive and informative as possible.

l. 27, how do you know the climbers are not aware of their risk? Certainly this is not possible by the methodological approach presented in this study. Have you used interviews to be able to make this claim?

l. 32, what means "deep"? Revise.

l. 40, remove one "to climate change".

l. 42, what are "well thought-out plans"?

l. 52, "most accident-prone...". Without any comparison to other sites this statement cannot be made.

l. 55, explain why/how climbers are vulnerable in this specific case.

l. 67-69, this laudable goal is not touched upon in the study (except for two brief points in the discussion that sadly lack a reference). I suggest to either expand the study into the adaptation part or leave this sentence out, here.

l. 75, use "Alpine" not "alpine", also at later occurrences

l. 76, "McColl", not "Mc Coll"

l. 78, what is the difference in this context between "climatic" and "meteorological"?

l. 83, "Moore" not "Moores"

l. 91-92, repetitive, consider removing. Almost the same point has been made already above.

l. 96, what means "streaming" or water? Check terminology

l. 97, "drag force", not "flow force"

Fig. 1, small grey text is barely readable. Also, where would convective rock heating come from in a solid medium?

l. 132, define MBM or, ideally do not use such acronyms first of all.

l. 143, Are you sure that "couloir" is a proper technical English term?

Fig. 2, actually this aerial image makes it really hard to understand the situation. I had to use it in combination with fig. 6 back and forth to understand your study area. I encourage the authors to use a topographic map or at least a hillshade map to better illustrate/justify their instrumentation scheme. It would also help to clearly state where the climbers are at risk. From fig. 2 and 6 it looks like it is the passage between C1 and C2.

I. 212, "area in m^3 "? How would that work?

I. 252, "significant activity", this statement needs comparison to other sites. With just that number of 1 event per 37 minutes I would not dare to say this is or is not a significant rockfall activity. Actually, this rate also needs to be normalised by area. Please provide an estimate of the source area, otherwise it is really hard to judge how significant that value is.

Fig. 5, is the day time in CET or CEST or UTC? Also, in the right y-axis add the normalisation by hour to the axis label.

I. 281, "recordings were recorded"?

Fig. 6, interesting modelling exercise, but how do the temporal reconstructions match up with your few month long empirical data set? Why do we need this look back into the recent past?

I. 298, where does the uncertainty range come from? Mention this in the methods before, and ideally, propagate this uncertainty also to the 41.9 and 58.1 % values later in that sentence.

I. 303, "sometime significantly" (add an "s") and please quantify, i.e. define what you mean with significantly.

Fig. 7, where do the blue vertical errors come from. How do you know these are rockfalls potentially caused by climbers? These arrows appear absolutely subjective and arbitrary to me.

I. 330-343 and 349-360, I do not really get the point here. What is the point in mentioning that all the findings are in agreement with previous findings? What is new then in this study other than confirming already confirmed knowledge? Please shorten significantly and/or focus on those points that add new insight or are not in agreement with the commonly expected patterns, which were based on often longer and/or better instrumented surveys.

Fig. 8, consider using line plots instead of bar charts.

I. 364, use "covaried" instead of correlated, or quantify this correlation.

l. 370-375, so can you clarify if the cause is insolation or conduction with a time lag? If not, make clear that you cannot decipher the relative importance of these two triggers.

Fig. 9, avoid overplotting of the axes/graphs.

l. 439, why 50 %? Did the number of 10 % in the methods section not rather point at 90 % false detections?

l. 441, "machine learning", not "deep learning"

l. 456-457, this is quite arm waving. Of course it always helps to put more sensors. But how many crack meters do you want to deploy in that large area? And how do you know where to put them? This is one of the main limitations on instrumentation driven research in high mountain terrain. Actually, I think we have a quite good understanding of the triggers of mass wasting processes. Nevertheless, it will remain a stochastic process at the catchment scale.

l. 464, the statement of permafrost degradation needs a reference.

l. 466-467, also here a reference is needed.

l. 469, "percentage points", please use 4.5–5.0 %

l. 473, provide a reference after "Drias-climat"

l. 489, a better estimation of the vulnerability should take into account the time of climbers spent in the window of rockfall trajectories and the rate of events happening as a function of daytime, season and trigger conditions. Currently, I can only see broad tendencies for increased rockfall activity like melting season and late summer, and afternoon to night.

l. 490-491, this implies that the accidents are actually related to rockfalls. Is this a given? From the text it rather reads like the accident rate scales with number of climbers on track.

l. 496, I disagree that this study has demonstrated that it has included these criteria in the analysis.

l. 498, I also could not find recommended adaptation methods in the discussion.

l. 502-503, please give the references (URLs) to these outreach activities.