

The reviewer comments are written in black and the author's responses are written in green.

Reviewer 1

The article "Assessing the relationship between weather conditions and rockfall using terrestrial laser scanner to improve risk management" presents the results obtained during one year of rockfall observation using LiDAR. Three zones along Route 132 in Haute-Gaspésie (Canada) were studied. The peculiarity of the analysis presented is that instead of performing scans with a periodic frequency, these have been made related on weather conditions, trying to adjust as much as possible the triggers with the possible environmental effects that cause them. The article is long and well written, although this reviewer considers that linguistic strategies have been used that make it difficult to read the text quickly and comprehensively. Examples of this are

Authors: thank you for your relevant comments and for your in-depth reading of this paper. Your comments will for sure improve the quality of our manuscript. I will not give you a long answer because we have agreed with most of your advices and so we applied them. We have corrected all the minor errors that you had identified and we have also done these major changes.

a) the overuse of a lot of coding which makes it difficult to understand [245-260, 311-317]

Authors: in order to improve the reading fluency, we have reorganised in-depth the section "methods". We have added some subsections and we have moved Figures 3 and 4 (and their description) in the "result" section because, they actually are some results. This major new structure seems to us more consistent and helps the understanding of the text: all the codes/acronyms are now only used at the end of the result section and sometimes in the discussion section.

b) the use of the terms rockfall and rockwall together in same sentences [e.g. 55]. In this case, the reviewer suggests using other terms such as rockface, rock-slope, cliff for future works, which, as well as helping to improve the positioning of the paper (Google scholar), will be more consistent to the bibliographic references [37-38, Abellán et al. 2014 -> rock slopes].

Authors: change done

Also, in terms of writing, the reviewer suggests reviewing the use of the separation of thousands. As an example: [367 vs 111,213] (Occurs in more cases). In addition, it also suggests homogenisation of the units (mm/yr) or (mm/a) [340 vs Figure 7]. Same as the term for this result. Please always use Erosion rate (Figure 7) or Retrate rate (348) to refer to these values.

Authors: all these changes done

Finally, this reviewer asks for the following typing error to be resolved: Figure 9(a) and the text [285, 291] refer to a data collection on 23 February, while Figure 3 refers to 24 February.

Authors: change done

In addition, the author suggests that the following minor errors be modified:

Line 37: Try to remove one challenging

Authors: change done

Line 43: Combining -> maybe it's better use "comparing".

Authors: change done

Line 46: I understand the idea...but maybe is a strong sentence and not up to date.

Authors: change done

Line 63: This sentence needs a reference.

Authors: change done

Line 103: Very impressive numbers! Valore to show it adding the relationship events/years/km (16 rockfalls/year/km)

Authors: change done

Line 112: Consider 350° as a pure north

Authors: change done

Line 125: Reference required

Authors: change done

Line 134: Figure 2(b)

Authors: change done

Line 137: Please, add the M3C2 quote (Lague et al 2013)

Authors: change done

Line 239: Use 5 and 20, or five and twenty

Authors: change done

Line 283: Add space between Figure 4 and Figure 9

Authors: change done

Regarding the scientific aspect this reviewer would like to notify the authors that an in-depth revision of the article should be considered based on the following major points:

a) In the methodology it is not described or referenced which workflow has been used to go from the M3C2 comparison to the "rockfalls". There are different strategies such as applying clustering algorithms (DBSCAN...), filter values etc. In no case can the reader know which threshold value has been used to show the red/green points in Figure 6, A1 or A2. This value that allows separating Positive Change and Negative Change from noise is crucial to identify the accuracy of the method.

b) Furthermore, although the magnitude (volume) is a key element in all the scientific discourse of the article, at no point is it described how it is calculated. There are different published methodologies, but none is referenced.

c) This reviewer has not found anywhere (method, discussion...) if all the red clusters observed in Figure 6 have been considered as rockfalls or not. There are few errors associated with the vegetation identified in Figure 6, A1 and A2. Have all clusters been considered or only some of them? What criteria have been used for this selection?

Authors: (common response for a), b) and c)). You are right. All the needed information is in our revised version of this manuscript. We described with lots of more details our methodology from LiDAR acquisition to rockfall volume calculation.

g) The classification of periods is a bit complicated. Although the reviewer appreciates the effort of the authors (and appreciates Figure 4). I would make more effort to explain how the periods have been classified. For example, the LiDAR capture from 19 March to 21 March is considered WIN. FT, but it is the second period with the highest rainfall intensity according to Figure 9.

Authors: we understand your point. We explained better the choices we done for the freeze-thaw cycle period with rainfall. For the others periods, we considered that our explanations were clear enough.

d) The reviewer considers it an error that figure 10 is presented in the results in the way it is done. This result is neither introduced in the introduction nor explained in the methodology. It appears suddenly in the results and neither the colouring nor the classification can be discussed because there are no references. If the authors consider it to be a relevant result, the reader should be led to it, finding it natural and consistent with the reading to obtain this result.

Authors: you are absolutely right! We have added some paragraphs in all sections to prepare the reader to our figure 10 (the matrix which is one of the aims of this paper).

e) The conclusions should be revised. "Line 457: This study proposed the novel technique of quantifying rockfall volume using LiDAR during...". This study does not propose any novel technique for quantifying volume. In fact, it does not explain how it is done.

Authors: change done.

"Line 462: "which is among the highest in the world". This is neither referenced nor presented in the main text and needs to be referenced.

Authors: more references are now presented in the discussion and we added them also in the conclusion (but maybe it is better to remove them for the conclusion section if they are already presented in the discussion section?).

f) I find missing in the main discussion the analysis of why a per phenomenon analysis is better than a periodic one (Late motive of the article that requires further development in the discussion). In general discussion can be improved.

Authors: relevant comment. We added more details about this in the discussion section.

Minor changes:

Line 64: Be careful with the terms hazard and risk. Rockfall hazard can only be mitigated by changing the physics of the rockfall. The work that you have implemented helps to mitigate the Risk.

(risk = hazard x exposure x vulnerability)

Authors: change done, thank you for the reminder.

Line 97: Natural Hazard -> Well used!

Authors: ok!

Line 221: Please, change "pre-rupture movement" -> "pre-failure deformation"

Authors: change done

Line 432: You are introducing into the discussion a singular result that has not been presented before.

Authors: we understand your point. Nevertheless, this result is not from our study since it does not come from our LiDAR surveys. Moreover, it allows us to discuss about the efficiency of our results summarized in the matrix which is our main and final result. We would appreciate to keep it in this section because we believe that it is interesting and useful to discuss about this event at the end of our paper, also to support our results.

Regarding the results, this reviewer wishes to notify that:

a) At no point are the results presented separated by study areas. This separation is key to identifying if patterns are reproduced equally in all areas. Presenting everything together implies that no comparative bias can be applied. The article would gain a lot of force if the results are presented for each study area and the result of the comparison of the 3 areas leads to the same conclusions.

b) At no point are the raw results presented. Number of rockfalls identified and their magnitude for each study area and for each LiDAR comparison. Although the reviewer agrees with the "cooking" of the results, he considers that the article should start from the raw results and from there carry out/apply the corresponding analysis.

Authors: (common response for a) and b)). We understand your advice to present our results separately for the three study areas.

First of all, we want to mention that we have already presented individually for the three study sites, 1) the rockfall spatial distribution (appendices) as well as 2) its frequency and 3) the rock slopes erosion rates (Fig. 5).

Nevertheless, we used the whole database to study the influence of weather conditions on rockfalls. This approach has proved necessary to avoid misinterpretation of the occurrence or non-occurrence of rockfalls during short microclimatic periods (e.g. heavy and high intensity rainfall or winter freeze-thaw cycles). We have added a paragraph in the “method” sections to explain our choice for the reader.

Figures (minor changes at authors' choice):

Figure 2: Please, change "Cloud autocorrelation" in c,d and e. Change to names more appropriate to the process (c,d -> merge?, e-> 3D Comparison). And explain the methodology better. M3C2 comparison, how to isolate the clusters etc.

Authors: good idea, change done

Figure 5: The fourth group with the most rockfall in the histogram is not enlarged (and is the group where the rockfalls associated with the highest risk occur). List or show how the rockfalls in this group evolve (this is the only graph where we can see the rockfalls you have identified).

Authors: Ok, I will give more details about the rockfall distribution of this group in this section

Figure 6: The colour scale does not help to identify the comparison. For experience, different colour scales (one for positive change and one for negative change) will allow the reader to better appreciate how the deformation evolves.

Authors: I understand your point but we did not use these colour scales in our protocol (to identify rockfalls) and our aim at this stage of the manuscript is not to discuss about the rockfall magnitude.

Figure 9a: Is it possible to have double freeze-thaw for the same period? Specify because for captures between 19/03 and 27/04 there are double readings of values. Filter or specify it, please.

Authors: I am not sure to understand your point. Between the two dates that you cited (19/03 and 27/04), three periods are presented. Both of them are for “Spring thaw” and during spring thaw, diurnal freeze-thaw cycles occurred almost daily. So several freeze-thaw cycles during these periods are indeed the norm.

Figure 9b/c: Although I understand the intention and I think it can be well solved, adjust the heights of the bars 9.3, 6.3, 13.8, 8.5.

Authors: we tried both of them and we think that the graph is more readable with our first option. 13.8 is a very high value compared to others.

Figure 11: Consider the real need or not to add an image at the end of the whole discussion, when this rockfall has not been presented individually throughout the text.

Authors: we understand that our last figure (Fig. 11) is located in the “discussion” section even if it could be viewed as a result. Nevertheless, this picture (and its description) allows us to discuss about the efficiency of our results summarized in the matrix which is our main and final result. Moreover, it is not a result of this study since it does not come from our LiDAR surveys. We would appreciate to keep it in this section because we believe that it is interesting and useful to discuss about this event at the end of our paper, also to support our results.

Reviewer 2

In this manuscript, the authors use repeat TLS surveys at targeted times to explore the link between a variety of weather conditions and rockfall activity. Overall, I found this an interesting and relevant study, and I really like the approach of timing surveys to specifically target certain weather conditions. I found the freeze-thaw discussion particularly interesting - nice to see the link between thaw characteristics and timing and the rockfall occurrence and magnitude. The risk assessment matrix in fig. 10 is also interesting – it gives a succinct overview of your findings it’s clear how this provides a nice framework for risk management based on weather conditions. The manuscript is wellwritten, clear, and nice to read.

Authors: thank you for your relevant comments and for your in-depth reading of this paper. Your comments will for sure improve the quality of our manuscript. I will not give you a long answer because we have agreed with most of your advices and so we applied them. We have corrected all the minor errors that you had identified and we have also done these major changes.

The previous reviewer mentioned the heavy use of the codes for the weather conditions in places; I do agree that this can take some getting used to and be a little clunky in some places, but I’m not sure what a good alternative would be.

Authors: in order to improve the reading fluency, we have reorganised in-depth the section “methods”. We have added some subsections and we have moved Figures 3 and 4 (and their description) in the “result” section because, they actually are some results. This major new structure seems to us more consistent and helps the understanding of the text: all the codes/acronyms are now only used at the end of the result section and sometimes in the discussion section.

My only major comment is with the description of the methods. I agree with the previous reviewer that there is important information missing about how the volumes were calculated, how the level of detection was determined (what are the thresholds for positive and negative change in fig. 6?), what the uncertainties are, and how real rockfalls were distinguished from other changes (i.e. there are positive changes from snow accumulation, so there must be negative snow-related changes as well in some scan pairs?).

Authors: you are right. All the needed information is in our revised version of this manuscript. We described with lots of more details our methodology from LiDAR acquisition to rockfall volume calculation.

For the precipitation and temperature gauges, can you give the locations? Were the borehole/thermistors in one of the scanned walls? If so, which one?

Authors: change done

You might mention in the introduction that there are also an increasing number of seismic based studies that link rockfall activity to triggering conditions. These can get very precise timing, but not precise volumes.

Authors: I understand your point but, as we focus on this study only on the relation between rockfalls and weather conditions, we have chosen to only say that: "Many factors can contribute to the development of rock instabilities, but two are recurring in the literature: precipitation and freeze-thaw cycles".

Figure 1: the site photos are pretty dark – could these be lightened a bit, to better see the cliffs?

Authors: change done

Figure 3: I really like this figure!

Authors: thank you!

Figure 11: this might also be nice for the introduction/motivation - based on the dates, I would guess that this event did provide some motivation for the study!

Authors: we understand that this figure is located in the "discussion" section even if it could be viewed as a result. Nevertheless, this picture (and its description) allows us to discuss about the efficiency of our results summarized in the matrix which is our main and final result. Moreover, it is not a result of this study since it does not come from our LiDAR surveys. We would appreciate to keep it in this section because we believe that it is interesting and useful to discuss about this event at the end of our paper, also to support our results.

Some typos:

Line 145: slope

Authors: change done

Line 180: from

Authors: change done

Line 363: 2019

Authors: change done