Referee #1 comments and reply to "Climate anomalies associated to the occurrence of rockfalls at high-elevation in the Italian Alps" by R. Paranunzio et al.

We acknowledge the Reviewer for his/her valuable and detailed comments. We'll take into account the suggestions that he/she gave us in order to clarify the purpose of our work and to improve our paper. Hereinafter, we numbered and divided the replies in three parts:

1. reply to the general comments of the Reviewer on the manuscript;
2. reply to the specific comments;
3. reply to the technical comments;

The reviewer's comments are in *italic* while our replies are in normal font.

1 General comments

REVIEWER: Paranunzio and coauthors apply a statistical approach to identify temperature and precipitation anomalies associated with 41 recent slope failures in the Italian Alps. Such a systematic and robust approach is most welcome, as several previous studies in this field have tended to be rather inconsistent or less rigorous in how they have quantified weather and climate extremes. While this is an important step, I feel an opportunity has been missed in this study to improve our understanding of the underlying processes that link climate driving with slope failures. In this regard, the manuscript is largely speculative and offers little new insight. I believe the manuscript can make a far greater contribution if major revisions were undertaken either:

1) *To significantly increase the sample size used in the analyses. As the authors indicate, an inventory of 41 events is comparable to those used in previous studies, so this means the current study faces the same statistical limitations as previous efforts, and all results need to be treated cautiously. This is unfortunate, because the analytical method developed by Paranunzio and coauthors offers great potential for applying overlarge datasets, which may then reveal robust patterns occurring over large regions. The authors mention that lack of consistent collection of climate data could prevent such analyses of merged inventories, but I don't see this as being a limiting factor in the European Alps.*

or

2) *If the focus of the study is to remain only on the 41 events from the Italian Alps, then considerably more detail about these events should be tabulated and included in the manuscript, for example, geological conditions (joint density, factures, lithology etc), failure*

type, presence of ice/snow in the failure area etc. You should also provide estimated (extrapolated) temperatures at the elevation of your detachment zones. Yes there will be uncertainties with these extrapolations. However, you will then be much more confidently able to link melt or freezing related processes to these failures. Such information should not be difficult to compile for a listing of only 41 events, and would provide more evidence to support the currently speculated causes of the failures.

RESPONSE:

1. As pointed out by this Referee, to increase the sample size could definitely improve the robustness of the results of our statistical analysis and deepen our understanding of the links between climate parameters and landslides. In a future stage, efforts will certainly be done to combine similar datasets at the European and global level, as suggested by this reviewer. However, as mentioned in the paper (see Discussion, P 11 L 32 and following), this task is not easy and requires considerable efforts, that are beyond the possibilities (and the purposes) of this work. With regard to climate data, we had to make considerable efforts for collecting the data, considered that most of the climate data needed for this work are not open. Furthermore, data are treated and collected in a different way by the various regional agencies (e.g., different time of data reading, different ways of estimating mean temperature etc.): a significant amount of time is needed for standardization before starting data analysis and we suppose that the problem could be even more complex at the European scale. With regard to rockfall data, the published datasets (many of which have been mentioned in the text) are inhomogeneous, because they have been compiled for different purposes and/or analysed with different approaches. Creating one single large homogenous data set is certainly crucial but, according to our experience, it cannot be done simply by combining the data from the literature, but rather would require the direct involvement of the scientists who have collected such data. We hope that the publication of this work may intrigue those colleagues who are working on similar processes and stimulate discussion and data sharing process.

   For all these reasons, in this paper we choose to concentrate our attention on the events occurred in the Italian Alps, for which we had an easier and more direct access to data. In conclusion, the Reviewer raised a crucial point, that we definitely want to address in future works, but that we think cannot be considered in a reasonable time for this work. The reviewer comment, in any case, made it clear that we have better specify the aim and the motivations of our work throughout the text.

2. Welcoming this Referee's suggestion, we will revise Table 1 (or Appendix A) in order to include all the available information about the *geological and structural setting* of the detachment zones, with the aim to give a better picture of the general conditions of the slopes that have been affected by failure. The information about the lithology can be compiled for all the detachment zones, with a variable degree of accuracy, depending on the availability of site-specific studies, or of large-scale geological maps only (1:100.000 geological maps are available for all the Italian territory). The information on the structural characteristics of the failed masses (joint density, factures, etc…) is instead fragmentary, because in this case specific studies and/or field surveys are necessary. Such information is usually available only for events with related literature, when a detailed study has been carried out on specific events, while for most of the events included in our inventory information has been obtained from newspapers, which typically do not provide this type of information. However, the geological and structural setting, as predisposing factors, mostly impact "where" a slope failure occurs, rather than "when", which is instead the focus of this study.

   With regard to the *type of failure*, throughout the text we referred to "rockfalls" for the sake of simplicity, but this term includes both rockfalls and rock avalanches: in the revised Table 1 we will specify the type of failure, as suggested by the Referee.

   With regard to the *presence of ice* in the failure zone, we will mention this information in the revised Appendix A, if known from the documentation available for the event.

   With regard to the *presence of snow* in the failure zone, it is certainly of interest for our study. It can be regarded as a preparatory factor (or eventually a triggering factor in combination with rain and/or temperature raise) to instability and it may play a key role in slope failure initiation. However, this information is only occasionally reported: we will add this information in Appendix A, if available.

   More generally, with reference to the second option proposed by the reviewer, we would like to emphasize that our study is not intended to be a case-by-case study: our approach aims to be more general, to help catch a possible climatic signal, which may be related to landslide initiation. The focus of our work is neither the characterization of the failure areas nor the interpretation of the causal mechanisms, rather how climate parameters could prepare and trigger, at different time scales, slope failures: we will revise the text in order to clarify our aims and motivations.

   Finally, in relation to temperature extrapolation, we decided not to transpose the values at the altitude where the failure occurred because:

- The application of a standard lapse rate does not take into account the remarkable spatio - temporal variability in the alpine environment at high altitude (Diaz and Bradley, 1997). A study concerning the spatial and seasonal variability of the vertical temperature gradient has been carried out in the Conca di Cervinia area, (NW Italian Alps), to illustrate the uncertainty in estimates of the thermometric conditions at high elevation rock falls sites, depending on local site characteristics, season and the type of parameter considered (i.e. minimum or maximum temperature). Vertical temperature gradient showed values between 0.5°C/100 m and 1.5 °C/100 m (Chiarle et al., 2014).
- The method is based on the identification of anomalous values on a statistical basis (Table 3); a rigid transposition does not affect the estimation of the probability associated to variable *V*. Our results would therefore be completely unaffected by the choice of transposing the temperature *T* values to the failure site.

For these reasons, we decided not to include extrapolated temperature in the text as we thought that this could be misleading, rather than of help in understanding thermal conditions at the failure sites.

2 Specific comments (P page, L line)

2.1 P 1, L 22

REVIEWER: I don't really agree that your study points towards the possible role of climate change in triggering slope failures. In contrast, the fact that there is so little difference between warm and cold anomalies would rather suggest climate warming is less important than some other studies might suggest. You may simply delete the word "change" from this sentence.

RESPONSE: We agree with the referee, as in fact we focus on the role of climate factors in triggering slope failures. Indeed, the role of climate change in the slope instability is not so evident and clear. In this light, our paper represents only a preliminary work and suggests an approach that can help discriminating those processes that may be somehow related with climate change, from those that appear unrelated. We thus welcome the reviewer's suggestion and we will delete the word "change" from the above sentence in the revised manuscript.

2.2 P 2, L 14

REVIEWER: I am a bit confused when you speak of "absence of evident rainfall". This suggests that you are already excluding some events that are clearly linked to precipitation triggering – why would you want to do this when the point of the analyses is to identify such climate triggers?

RESPONSE: The issue of rainfall thresholds for the initiation of landslides has been largely addressed by the scientific community. Various studies focusing on global/regional thresholds and related rainfall intensity-durations concepts have been proposed since many decades (e.g., Caine, 1980). Guzzetti et al. (2007) shows an exhaustive review on that topic.

Our method is a statistical-based preliminary work which is less complex and exhaustive with respect to previous studies on rainfall-triggered landslides, thus we would not add a consistent contribute in that sense. On the other hand, only in recent years the scientific community started considering also temperature as a potential trigger of slope instability, mainly in relation to global warming. With respect to rainfall-induced landslides, few authors have explored links between rockfall occurrence and temperature, in combination or not with precipitation. However, temperature is a key factor at high elevation sites, where the cryosphere plays a major role in geomorphological dynamics, including slope failure. For these reasons, we preferred to focus on processes occurred in the absence of an evident rainfall trigger, for which climate warming in the last decades has often been deemed as responsible.

2.3 P 2, L 25

REVIEWER: I missed some general overview of the geological and geomorphological setting of the study region which of course may have a significant control on the distribution of slope failures discussed later.

RESPONSE: We agree with the Referee, a general geological and geomorphological overview of the study area can be of help and will be added in the revised manuscript.

2.4 P 3, L 3-4

REVIEWER: At what elevation are these temperature values from? Without such information the values are not of much use.

RESPONSE: Reported temperatures refer to areal values, extrapolated using known observations recorded from the weather stations located in the area of interest. We will clarify this in the revised text.

2.5 P 3, L 13-15

REVIEWER: This clustering of events in 2004 is interesting and I expected to see it discussed again later in the context of your results. Were these events linked to a particular climate anomaly in 2004?

RESPONSE: According to climatic reports at regional scale (e.g., ARPA Veneto, http://www.arpa.veneto.it/temi-ambientali/climatologia/dati/commenti-meteoclimatici and Ufficio

Idrografico - Provincia Autonoma di Bolzano climatic reports, http://www.provincia.bz.it/meteo/climareport.asp) May 2004 has been particularly cold with respect to average 1961-1990 for the same period and late snowfall has been recorded in the first days of the month. At high elevation we find the highest differences up to 5° C colder than average 1961-1990 (e.g., Plateau Rosa, Northwestern Italian Alps, 3480 m a.s.l. - source: 3Bmeteo). Summer and early Autumn 2004 did not show particularly climate anomalies instead. Thus, since it is difficult to motivate why such a large number of events occurred in 2004 in the absence of a particular climate anomaly, we could delete the sentence.

2.6 P 6, L 2

REVIEWER: I find it very unlikely that "recent global warming" has had much (or any) influence on the distribution of permafrost relative to the mapping of Boeckli et al. Depends on what you mean by "recent", but certainly the distribution of permafrost unlikely to have moved beyond the 360 m uncertainty range.

RESPONSE: We thank the Referee for the comment, this sentence could be misleading and thus we decided to remove it from the text.

2.7 P 6, L 25

REVIEWER: This is where I begin to question if these results can be considered statistically significant, given we are dealing with only 41 events, and where application of your methodology to a larger combined inventory could lead to some truly interesting and robust findings.

RESPONSE: We invite the Reviewer to see our response to the general comment.

2.8 P 8, L 35-36

REVIEWER: Obviously this finding needs to be treated cautiously given the small sample size, but, nonetheless this warrants more discussion given that it would suggest the possible role of global warming is not at all clear. In fact, if this finding is robust, global warming could be expected to cause fewer events being triggered by cold anomalies, which would largely offset any expected increase in events caused by warm anomalies.

RESPONSE: We agree with the reviewer and we will deepen the discussion on this point in the revised text.

2.9 P 9, L 16-24

REVIEWER: This attribution of the rockfalls to causal mechanisms is very speculative, and would need to be supported with more details about the specific conditions in the failure zones of these *events (including extrapolated temperatures), and with cited literature on these processes. I also don't find the logic regarding the different elevation of the western and eastern Alps particularly convincing. I could argue in contrast that marginal (around 0 degrees) and slowly degrading permafrost at depth is likely more prevalent in lower elevation zones, while at high elevation the permafrost is likely colder and less susceptible to recent warming.*

RESPONSE: With regard to the first point raised by the Referee, we will add more information about the specific conditions in the failure area, according to the available documentation. Cited literature on the processes is listed in Table 1, but references will be added in the text as well.

Concerning permafrost degradation, we partially agree with the Referee, since warm permafrost could be found at different elevations, depending on the direction, i.e. at higher locations in southern and lower locations in northern aspects (Fischer et al., 2012). On the other hand, it is true that the linkage between causal mechanisms and the different elevation of the two sectors of the Alps is not adequately motivated and convincing, thus the text will be modified.

2.10 P 9, L 30

REVIEWER: As with previous comment, the logic that permafrost thawing would contribute only at highest elevations seems questionable, and is inconsistent with previous studies that highlight the importance of permafrost in elevation ranges where marginal conditions prevail (down to ca 2500 m on shaded slopes).

RESPONSE: We are aware that previous studies (e.g., Fisher et al., 2012) suggested the importance of the marginal permafrost zone area, which is considered to be most susceptible to changes and slope failures processes, in agreement with Davies et al. (2001), who found that ice in rock discontinuities becomes less stable few degrees below 0° C. However, based on our results, rockfalls linked to long-term warm anomalies occurred mainly at high elevation sites, above 3000 m. Such anomalies could have played a major role in the preparatory phase rather than in the triggering of the events. In this light, referring to active layer thickening and extension instead of permafrost degradation is more correct, as suggested by the Referee in the following comment. We will modify the text, trying to clarify our motivations in the revised paper.

2.11 P 9, L 33-35

REVIEWER: Short-term temperature anomalies will not lead directly to thawing of permafrost due to the slow response of temperatures at depth. Rather here you should highlight the link between short-term warm anomalies and active layer thickening, with references to appropriate literature (e.g., several papers by S. Gruber and co-authors).

RESPONSE: We agree with the Referee, short-term warm anomalies could affect the active layer thickness and not lead directly to permafrost thawing. We will modify the text as suggested, including references to appropriate literature (Harris et al., 2009; Gruber et al., 2004; Gruber et al., 2007) in the revised manuscript.

2.12 P 9, L 36-37

REVIEWER: But there is no reason that a ST warm anomaly causing precipitation to fall as rain rather than snow will on its own lead to a failure right? Such an event would also need to coincide with a large precipitation anomaly for there to be any influence on the underlying slope stability. Do you see in your results any such anomalies coinciding? Also, providing extrapolated temperatures to the elevation of the detachment zones would provide much more support for speculated processes such as these.

RESPONSE: In our opinion, infiltration of water into the bedrock fractures due to rainfall or particularly when near-surface ice/snow is available for melt (because of rain-on-snow process or high temperatures melting early snowfall) may reduce the shear strength of the rock mass potentially leading to the failure. The Reviewer comment made it clear that we have to deepen the discussion on this point in the revised text; references to existing literature will be added as well (e.g., Allen and Huggel 2013). Regarding temperature extrapolation, we explained in the response to general comments why we have preferred not to include translated values in the text.

2.13 P 10, L 32-34

REVIEWER: Merging and providing a combined analyses of these datasets (at least for the European Alps) would be very exciting, and I encourage the authors to consider this. In my view, this will provide the best opportunity for further advancement of understanding in this field.

RESPONSE: We invite the Reviewer to see our response to the general comment.

2.14 P 11, L 3-6

REVIEWER: Could gridded global scale datasets and reanalyses products be used to overcome the inconsistencies in ground based climate data?

RESPONSE: Gridded data could be used to overcome these inconsistencies, but much depends on the spatial and temporal resolution of the dataset. Gridded temperature datasets usually include data on large spatial coverage and at long-time scale (e.g., monthly scale), while we based our analysis on aggregation of daily records. We will add some consideration on that topic in the revised manuscript.

2.15 P 11, L 14-17

REVIEWER: Previously you describe only a "slight difference" in the role of warm vs. cold temperature anomalies. So is this really a sufficient basis to support the hypothesis that global warming and cryosphere degradation is impacting on slope stability? Especially as your study does not specifically assess or quantify cryosphere degradation in or around your failure zones. In fact, some of the causal processes you speculate (rain vs. snow etc) are not linked at all with cryosphere degradation. Rather, I would suggest that the results of this study, and particularly the large amount of events associated with cold anomalies would indicate that the relationship between climate change and slope stability might not be so straight-forward.

RESPONSE: We agree with the Referee when he/she argues that *"the relationship between climate change and slope stability might not be so straight-forward".* The role of climate change is not obvious, as we have already discussed in the response to the general comment and at 2.1. Nevertheless, the focus of our work is about climate variables impact (and not climate change) on the occurrence of slope instability events, since our method deals with possible climate signals, which could be related to landslide initiation. We will modify the text, trying to clarify our motivations and taking into account the suggestions of the Referee.

3 Technical comments

3.1 P 1, L 1

REVIEWER: "associated to" > "associated with"

3.2 P 1, L 11

REVIEWER: "occurred at" > "occurring at" (numerous other instances also – please check)

RESPONSE: "occurred at" has been replaced by "occurring at" as suggested by the Referee.

3.3 P 1, L 16

REVIEWER: I would add "SIGNIFICANT temperature anomalies in 83 %...".

RESPONSE: "SIGNIFICANT" has been added as suggested by the Referee.

3.4 P 1, L 35

REVIEWER: Delete/move the Stocker et al. reference, as citing at the end of the sentence implies that Working Group I of IPCC made a link between cryosphere degradation and slope failures.

RESPONSE: The reference has been deleted as suggested by the Referee.

3.5 P 1, L 35

REVIEWER: "change in" > "influence on"

RESPONSE: "change in" has been replaced by "influence on" as suggested by the Referee.

3.6 P 2, L 11

REVIEWER: "that can be deemed responsible" implies you are able to establish clear linkages. I would suggest "that may be responsible"

RESPONSE: The sentence has been replaced as suggested by the Referee.

3.7 P 9, L 6

REVIEWER: "prevail on" > "prevail with"

RESPONSE: "prevail on" has been replaced by "prevail with" as suggested by the Referee.

Cited literature

Chiarle, M., Coviello, V., Arattano, M., Silvestri, P., and Nigrelli, G.: High elevation rock falls and their climatic control: a case study in the Conca di Cervinia (NW Italian Alps), Engineering Geology for Society and Territory-Volume 1: Climate Change and Engineering Geology, 439, 2014.

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