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Interactive comment on “Regional trends and controlling factors of fatal landslides in Latin America and the Caribbean” by S. A. Sepúlveda and D. N. Petley

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Response to Anonymous Referee 1

The authors thank the anonymous referee to his valuable comments and suggestions. The limitations of the work due to scale and dataset size have been stated more explicitly as suggested, both in the introduction and with further discussion. The aim of this work is to provide insights of the main factors controlling the occurrence of fatal landslides at a continental to national scale, thus the treatment of some parameters is not carried out in detail as commonly done in landslide studies at local scales. This work

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should give orientations of areas where more detailed studies should be performed to improve prevention to fatal landslides, with some insights of physical and social factors that influence their occurrence, as well as how research may help on the risk reduction.

Answer to main specific comments. Text corrections and suggestions for more clarity have been made in the manuscript.

Page 2781, line 12-13. This is a general comment. The authors have used the 30 m SRTM DEM. Despite the know problem, this is a rather accurate source of topographic (morphometric) information. However, the authors have stated that their ability to locate the fatal landslides geographically is limited to a few kilometres. Given this accuracy, determining (and using) terrain information from a 30 m DEM may not be very useful, or sensible. The authors should clarify this point, here and/or in other section of the text. See also comment on page 2784, line 26, below.

R: The referee is right. We use a 30m resolution DEM but given the regional scale of the study we do not attempt to define the slope angle for every single landslide.

Page 2782, lines 25-27. A possible mix of apples and oranges, here. I think the authors are missing meteorological phenomena (e.g., hurricanes, tropical storms), with rainfall characteristics (e.g., the intensity of the rainfall. Intense rainfall is typical of hurricanes, tropical storms and other storms.

R: The sentence was rewored. All those cases are triggered by heavy rainfall, from which a part have been clearly identified as hurricanes-tropical storms episodes, thus the rest is related with other type of storms.

Page 2783, lines 19-22. It is unclear to me why there should be, or the authors expect a “rollover” in their probability density plot, a part from one due to under sampling of the small events. The cited work of Malamud et al. (2014) did not show a rollover for all of their landslides. For rock falls, the statistics did not reveal a rollover, for example. This should be clarified.

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R: The rollover on comment would be mainly by undersampling. The citation was revised for more clarity.

Page 2784, line 26. See also previous comment on page 2781, line 12-13. The authors state that they have used the local slope calculated from the 30×30 m SRTM data to account for “relief”. This is rather questionable. First, how do the authors have attributed a value of terrain slope to the single landslides? Taking the slope at the exact coordinates of the landslide? If it so, how do they have coped with the fact that the local slope may be quite different than the regional slope, which is the one that measure “relief” somewhat? Second, given the error associated with the location of the fatal landslide, which is acknowledged by the authors, which point is used to represent the landslide? Even in mountain areas terrain slope can vary largely even across short distances. My recommendation here is to use a different topographic measure to identify “relief” areas, and particularly relative (local) relief in a reasonably sized window. For example a size that corresponds to the uncertainty (error) associated to the mapping of the landslide.

R: This work is at national-continental scale. We neither attribute slope angles to the single landslides, nor map the landslides in detail, just their location. The used slope data allows identifying large areas of higher or lower relief, to compare with the landslide distribution at the working scale. We do not attempt to carry out any specific analysis at local scale, for which the comments are aimed.

Page 2785, lines 5 to 8. Although I am convinced that geology is important for landslide occurrence, I wonder how can you really inform any model or speculation on the role of geology on the occurrence of landslides based on very small, synoptic scale geological information has the one used in this work.

R: We agree that at this scale is not possible to model the role of geology, so we are just showing the landslide distribution against the regional geological map and stating that is not possible to analyze further at this scale.

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Page 2786, lines 16-18. Why is the scatter large? Is it because the societal index used is uncertain, or because the natural phenomenon (landslide) difficult to explain, at this scale? I wonder if the authors can comment on this.

R: We think that the complexity of the landslide phenomena cannot be directly related to single societal indexes such these at this scale. A sentence stating this comment has been included.

Page 2786, lines 24-26. I am not fully convinced by the argument that “these three factors should be considered as primary controlling factors of fatality-inducing landslides in the study region” holds. The three mentioned factors are slope gradient, precipitation and population density maps. Clearly all of them are related to the occurrence of fatal landslides. However, given the scale of the information used, the three parameters are proxies. And this is what is not convincing in the argument made. A proxy may not be a “primary controlling factor”. The authors should comment, and clarify the issue. As a side note, the reference given to support the statement (i.e., Parker (2010) is an unpublished MS thesis, which is therefore difficult to examine for many of the potential readers. Is there any better reference that can be added here?

R: We agree that given the scale of the analyses, they are not necessarily the primary factors controlling the landsliding. The sentence was reworded, avoiding the “primary controlling factor” concept. In this first order analysis, the factors that best explain the observed distribution are topography, annual precipitation and population density.

Page 2787, line 3. The statement “It is generally accepted that research can play a key role in reducing the impact of natural hazards” is rather strong, and not fully supported. It would be good to back up this (strong) statement with some reference, or evidence.

R: The statement was reworded, adding a reference with examples of how increase in research correlates with decrease in landslide losses.

Page 2788, lines 5 - 7. Again, I am not fully convinced that there is a simple link – as

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the authors seem to indicate – between research (and publications) and the number of landslide fatalities. This is because not all the publications are good and useful, or can be applied. Also, there must be a lag time between research (and publication), its possible application, and the positive effect of a reduced number of fatalities.

R: We don't think there is a simple link and of course there are many other variables apart from research to help reducing the landslide losses. Referee's comments about quality and lag time are correct, although in our case quality is somewhat constrained from using only ISI papers for the analyses. A new section of discussion was added commenting on these issues about the role of research for disaster prevention.

Page 2788, lines 13-15. What about education? Isn't it important as research, and possibly more than research?

R: See previous answer. We are not proposing research as the solely factor to reduce losses, of course education is very important, but we are not analysing it.

Page 2790, lines 1 – 2. “. . . showing that there is only partial coincidence with our dataset from one decade later.” Is this because of the natural variability of the landslide phenomena, or is it due to data availability? Clearly, the long-term effect will be different.

R: The difference in the long-term effect is recognized in the same paragraph in the discussion, the 10 year window is clearly not enough to predict in the long-term.

Notes on the Figures.

R: Suggestions for figures improvement are accepted, using bar charts, colours and enlarging font sizes to improve readability

The reviewed version of the manuscript is included in the Supplement

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/3/C1024/2015/nhessd-3-C1024->

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