Responses to Reviewer #1

I started to read the manuscript with great interest as in the abstract the paper claims to create a new method to assess landslide hazard and puts inventory at the center of landslide hazard assessment. However, I am not satisfy those claims are substantially supported by the manuscript and my major observations are as follows:

We thank the reviewer for his critical reading of the manuscript and for his detailed comments. We are pleased to hear that the abstract has attracted great interest. We will improve the paper to ensure this interest remains high throughout the manuscript.

1. The literature review/introduction part of the manuscript is very weak as it does not properfly define the rational behind this research and does not explain the research gaps (why this work needs to be done) and what are the existing gaps in the scientific literature. For example, recent works on physically based analysis for small regions and statistical/ML approaches for large regions has significantly well defined the landslide hazard in terms of exceedence probability, susceptibility and hazard itself. Gap on those works must be stated and your work must be well posed to solve those gaps. (e.g. why your work is needed when the work of Guzzetti 2005 on probabilistic approach or recent improvements on that can define landslide hazard).

1. The scientific advances in landslide hazard estimation cited by the reviewer concern physically-based analysis and statistical/ML approaches. Our work, however, uses a semi-quantitative heuristic approach, which, unlike modeling approaches, is little explored in the scientific literature. The method we propose does not express landslide probability in modeling terms (probabilistic or deterministic), but defines landslide hazard areas based on landslides that have already occurred and the potential evolution zones for individual landslides or groups of landslides. Within these areas, it defines the hazard level based on the magnitude, velocity and frequency of landslides that have already occurred and are expected. While highlighting certain limitations, we maintain that our method represents an important advance in the field of semi-quantitative landslide hazard estimation because: i) it utilizes and optimizes information on landslide intensity and recurrence contained in inventory maps; ii) it takes into account the spatial and temporal dimensions of hazard; iii) it establishes a logical and reproducible procedure; iv) it overcomes the subjectivity of traditional geomorphological approaches; v) it provides maps directly applicable to land-use planning. We will introduce and appropriately argue these points in the new version of the manuscript. However, the presentation of a new method for landslide hazard assessments is not the only result of this work. Indeed, as the title suggests, the main objective of this paper is to demonstrate and measure the effect of different mapping approaches on landslide hazard estimation. In the new version of the manuscript, we will better highlight this additional result and its scientific relevance.

2. The method defined for this work needs to be clarified as method section ends abruptly at the section comparing with PS points. I did not understand what is the end goal of your landslide hazard assessment. does it provide landslide hazard zonation (like class low medium high) or does it provide quantitative measure. From the introduction section my impression is that you have semi-quantitative approach for defining hazards.

2. The reviewer is right. In the new version of the manuscript we will define the method more explicitly in all its steps so that the end goal of our landslide hazard assessment is clear. This will also require adding new text and a new figure in the results section.

3. Can we define landslide hazard just based on the frequency of past landsldies? Major issue that I have with the approach is that you assume the landslide will occure in the same zone if it

has occured in the past observation and vice versa. I would asuume the landslides often change the terrain characterstics changing the landslide dynamics and frequency, thus changing the hazard.

3. Not exactly. Our procedure for defining landslide hazard takes into account the magnitude, expected velocity, spatial distribution, and finally, the frequency of past landslides. The underlying concept of our method is: learning from the evolution of past landslides to estimate the characteristics (and therefore the hazard) of future landslides. This concept is based on the widespread evidence that past landslides worsen the geotechnical conditions of the affected terrains and disturb the morphological stability and geo-hydrological circulation of the affected slopes, thus generating and maintaining conditions of instability in the evolution area of the considered landslides, until a new equilibrium is reached. The information contained in the geomorphological landslide inventory maps we use in this work covers an indefinite time window, but realistically "photographs" the landslide trend — still evolving — over the order of (at least) hundreds of years. It can therefore be assumed that similar conditions of instability will persist in the future for a similar period.

4. How is the landslide volume/kinetic energy estimated? is it through sterio-images or based on the area of landslide polygons. Since area can be a proxy of volume why not use area itself, which can be measured with higher precision and accuracy?

4. In the main text it is perhaps not clear enough that this information is reported in the ancillary materials at the end of the manuscript. In the new version of the MS we will indicate more clearly that this part is specified in the ancillary materials. However, in summary, we estimate the intensity of a landslide as the combination of expected velocity and magnitude. As a proxy for velocity, we use the typological classification of landslides derived from photointerpretation analysis, establishing classes of landslides as slow (slides and complex landslides), rapid (earthflows and debris flows), and fast (rockfalls). As a proxy for magnitude, we use volume (derived from the area through empirical relations). We consider volume to be the most appropriate measure of magnitude because it takes into account both landslide area and depth.

5. Your workflow primarily maps frequency and then treats it as hazard for planning ("frequency counting", "landslide frequency zonation" and "LHZs" are the operative layers). That's consistent with the classic geomorphological tradition, but it does not explicitly model when landslides are likely to recur. Therefore, it looks more like landslide inventory analysis to understand their spatial frequency.

5. It is important to point out that the hazard determined by our procedure is based not only on spatial frequency, but also on the magnitude and expected velocity of landslides. However, the reviewer's observation is correct: our method does not explicitly model when landslides are likely to recur. This is because the spatial frequency we determine derives from the analysis of the geomorphological inventory, which (as mentioned in point 3) does not cover a defined time window.

Actually, in this paper, we also apply the procedure to a multitemporal inventory, which contains landslides that occurred from 1955 to 2013. In this case, the time window is defined and a frequency rate can be established. However, in this time frame, we observe only shallow, small-scale landslides, and consequently, the landslide hazard would appear to depend solely on these landslides. In reality, this isn't the case. What actually happened in this time frame is that the small, shallow landslides reported in the multi-temporal inventory occurred paroxysmal and therefore left clearly visible traces on the landscape, easily mappable by aerial photographs. On the other hand, the evolution of medium-large, deep-

seated landslides, which is typically very slow, continuous, and/or seasonal, leaves no visible changing on the landscape and is therefore not mappable by aerial photographs. However, we know that medium- and large-scale landslides create the greatest hazard conditions in our study area. In particular, what we observe is that the LHZs (Landslide Hazard Zones) for medium-sized landslides with the highest spatial frequencies (although not referred to a defined time frame) are those with the most evident damages to structures and infrastructures and most dynamic PSs.

Basing on these evidences, we maintein it is more effective and realistic to determine the hazard based on the spatial frequency of all the landslides - even if measured over an indefinite time window - rather than limiting the hazard analysis to landslides from the multitemporal inventory, which ultimately are not representative of the landslide evolution of the area (except to a minimal extent).

6. The manuscript introduces PS within LHZs "to identify active settlements," but the role of PS in the hazard inference remains vague (screening? validation? conditioning?). Spell out: (i) PS processing settings; (ii) deformation thresholds that define "active"; (iii) how PS density/velocity modifies frequency classes; (iv) how you treat layover/vegetation decorrelation; and (v) how PS evidence is integrated with inventory (e.g., Bayesian updating of activity state vs. a qualitative overlay). Without this, PS risks being read as a visual add-on rather than a quantitative component.

6. Actually, we consciously decided not to include PS data as a quantitative component of our procedure. This is because it is known that PS data coverage across a territory is partial because it depends on land cover. This intrinsic characteristic of PS data is incompatible with our procedure, which, by definition, must necessarily produce a landslide hazard zoning using a homogeneous method across the entire territory, avoiding bias. However, where available, PS data represent an additional, updated, and independent source of information on the state of landslide activity that deserves to be used. We therefore chose to use PS data to complement the procedure's output. In other words, PS data cannot change the specific hazard (indicated in our procedure by a positional index) of a LHZ, but they add information on the state of landslide activity within an already classified LHZ. This additional information can be used by decision makers to determine the absolute hazard level of that territory, basing not only on the specific hazard provided by our procedure, but also on the PS data. In the new version of the manuscript, we will define the method more explicitly, including the role of PS in the hazard inference.

Regarding the points that the reviewer asks for clarification, it is specified that:

i) We used PS data already processed in the framework of the Doris EU project. The specifics of the processing of these data are available in the project reporting (https://cordis.europa.eu/project/id/242212/reporting) and in the published papers focusing on interferometry analysis related to the project, which are already cited in the manuscript (Bianchini et al., 2012 and 2015; Cigna et al., 2013; Raspini et al., 2016; Ciampalini et al., 2015);

ii) We consider as inactive ground motion values below the threshold of |2| mm/year. The value coincides with the minimum stability threshold used by Ciampalini et al., 2015 for the PSs of the entire Messina Province, which includes our study area. The value also is comparable to the lowest noise value that can be evaluated by observing different time series of ERS, ENVISAT and COSMO data randomly sampled in the study area;

7. The paper discusses reasoning behind frequency mapping but does not present a formal validation (e.g., chrono-validation, forward validation against a held-out recent period, or cross-regional transfer). I would consider some quantitative validation by some sort of chrono validation.

temporal landslides (occurred in the period 1955-2013) within the LHZs designed basing only on the occurrence of pre-1955 landslides. The information content of the multitemporal inventory can be particularly effective for validating the hazard of fast landslide, since even small fast landslide can have high magnitude (depending on the volume of the maximum expected boulder involved). This allows to complement the validation done with PS, which by definition cannot detect fast landslides. In the new version of the manuscript we will dedicate specific new text and a new figure to describe the two validation tools (multitemporal landslides and PS) of the output of our procedure.

8. How would you assess the uncertainties in the hazard zones, and how better is it compared to geomorphic zonation by expert based analysis where expert looks at the geomorphology and defined the landslide zones. I think this needs to be discussed extensively.

8. In our procedure, the Landslide Hazard Zones (LHZs) are defined as areas of possible (or probable) short-term evolution of existing landslides with similar characteristics (i.e. of type, volume, depth, and velocity). A LHZ is therefore a "landslide scenario" designed around a landslide or group of landslides and delimited using geomorphological criteria. Therefore, the uncertainty associated to each LHZ can be considered as coinciding with the difference between the area of the landslide polygon(s) within a given LHZ and the area of the LHZ itself. Basing on our dataset, this difference (the uncertainty) depends on the size of the landslides, and is greater the smaller the landslides. However, this excess area is not distributed equidistantly in all directions around the landslide(s) because it depends on the local morphology and the evolutionary trend of slope considering the (i) partial reactivation of portion of existing landslides through new landslides of younger generation, (ii) the lateral, head (retrogressive) or toe (progressive) expansion of the existing landslides, and (iii) the possible occurrence of new landslides of similar type and intensity. This is the main difference with the traditional geomorphological approach. In fact our procedure does not establish areas susceptible to landslides based on the geomorphologist's experience. Instead, our procedure defines the limits of the potential evolution of landslides or groups of landslides of an inventory map, primarily basing on the proximity and generational relationships identified between these landslides. We believe our approach limits operator (geomorphologist) subjectivity by constraint the choices underlying the hazard analysis to the data and metadata associated with a landslide inventory. This is a central point of our work. As a matter of fact, as indicated in the title and as shown throughout the manuscript, we apply the same procedure to inventory maps of varying detail, demonstrating the impact of the underlying data on the procedure's outcome, regardless of the operators (geomorphologists) and their experience. In the new version of the manuscript, we will reiterate this point and discuss it more extensively.