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<u>*Review of the manuscript „Analysis of a landslide multi-date inventory in a complex mountain landscape: the Ubaye valley case study"*</u> *(Schlögel et al.)*

The presented paper „Analysis of a landslide multi-date inventory in a complex mountain landscape: the Ubaye valley case study" focuses on the generation and analysis of a multi-date landslide inventory. The resulting data set was generated for a case study located in the Southern French Alps by interpreting and analyzing a variety of sources (e.g. orthophotos, SAR images, geomorphological maps, historical records). The resulting synoptic landslide inventory was investigated spatially and temporally by multiple analyses.

General comments (Overall quality): From my perspective, the content of the paper is partially up to international scientific standards and addresses important issues in the field of natural hazards. I agree with the authors that the generation of a reliable multi-date landslide inventory is highly important to gain further insights into the spatio-temporal evolution at regional scale. The methodological procedure generally builds on existing knowledge whereas especially the consideration of multiple, completely different data sources (e.g. from orthophoto interpretation to SAR images) represents an innovative and challenging aspect. At the same time, the combination of these data sources might introduce a variety of interrelated uncertainties, which might be propagated (unintentionally) into the subsequent statistical analyses. Thus, I think that the presented empirical relations should be interpreted with more caution as landslide inventory based biases might arise from multiple sources. In this context, it seems valuable to quantify (or at least comprehensively address) potential biases involved within each analysis step to further explore the significance of the results.

From a formal point of view, I think that the structure of the paper lacks clarity. The observed mixing of chapter contents highly hampers the readability of the manuscript. Thus, I conclude that a thorough reorganization of the paper appears crucial to enable an in-depth judgement of its scientific quality. From my perspective, four major modifications (see specific comments) are necessary in order to enable a thorough review of the manuscript: (i) restructuring of the paper; (ii) revision of the main objectives of the study and highlighting of the purpose of the conducted analyses; (iii) addressing of limitations and potential biases; (iv) specifying and/or disclosing of underlying criteria/decisions (e.g. thresholds, grid resolutions etc.)

Specific comments (suggestions for improvements are marked in **bold):**

(i) In my opinion the structure of this paper is confusing, as chapter contents are mixed constantly. For instance the results chapter 4.4. (*"Return periods of landslide events: temporal probability assessment")* mainly describes the methodology applied, whereas the results section 4.5 *("periods of landslide activity and identification of landslide triggering"*) appears not to correspond to any methodological section. Such tendencies are present throughout the whole document. I recommend a **restructuring of the paper** in terms of clarifying the sequence and hierarchy of the executed working steps. I suggest that every methodological chapter should clearly be associated to a results chapter.

Furthermore, I advise a **re-naming of headings**. For instance, the methodological chapter 3.3.4 *"Calculation of landslide time recurrence"* is associated with the results section 4.4 *"Return periods of landslide events: temporal probability assessment"*. Suggestion: 3.3.4 *"Temporal probability assessment"* <-> 4.4 *"Results of the temporal probability assessment"*.

I also suggest **inserting a discussion section** to attribute limitations and uncertainties of this study (see point iii).

A **separation of the chapters "materials" and "methodology"** might further increase readability.
I recommend **skipping chapter 4.5** as it highly distracts the readability of the manuscript and does not significantly contribute to the main objectives of this study (by standing alone). From my perspective, the most relevant information of this chapter (4.5) **should be shifted to a** (not yet existent) **discussion**

part or integrated into other chapters. From my perspective, chapter 4.5.3 *"relationships between landslide activity and triggering events"* goes far beyond the scope of this work and cannot be comprehensively addressed within the framework of this study.

I further recommend to **recheck the manuscript on redundant information** (e.g. Page 2057, Line 1-3: I think that this information is already given in figure 3; Page 2060: From my perspective these enumerations are partially mentioned in table 2).

In my opinion, an extensive **shortening and concise rephrasing** of text passages as well as a **"merging" of figures** and tables might further increase the quality of the paper. For example: Merge Fig. 2 and Fig. 3 to provide a concise overview of the spatial and temporal coverage of data sources.

(ii) From my perspective, the objectives of this study are not formulated concisely. The objectives ("i") and ("ii") appear to be quite similar, whereas several findings seem not to be associated with any of the formulated objectives (e.g. chapter 4.5.3). I highly recommend a thorough **revision of the objectives** (e.g. all scientific questions addressed within this study should be covered by the objectives). I got the impression that multiple analyses conducted within this study appear not to be connected to each other. I suspect that this might partly be traced back to an inconsistent structure of the paper (see point i) and/or an inadequate formulation of the objectives. In this sense, I also recommend to **provide an overview of all conducted working steps** in the form of a flowchart (e.g. input data, analysis steps, end product) to increase transparency. I also suggest **clarifying the purpose of each analyses** step within each methodological chapter.

A **final assessment** (discussion chapter) **of methodological innovations and limitations** might be beneficial to reveal the scientific significance of this paper.

(iii) The explanatory power of empirical results is known to be highly dependent on the completeness and reliability of the underlying input data. I think that limitations and inventory based biases are not sufficiently addressed within the presented paper. Even though, single sentences or text segments (e.g. ("difficulty to map the small size events", "underestimation of small events", "some of the landslides are omitted in the database because of their size") indicate that uncertainties might be present. I personally miss an in-depth confrontation with this topic.

The authors state that the reliability of their landslide inventory is highly dependent on the knowledge/skills of the scientist and the quality (e.g. spatial and temporal resolution) and type (e.g. orthophotos vs. geomorphological mapping) of the data sources used to identify/map landslides. As I completely agree with this statement, I suggest that the **empirical results obtained within this study should be interpreted with more caution**. Example: *"the majority of slopes oriented to the N, NW and W is (sic!) more affected by landslides, suggesting a dependence with longer persistence of snow cover…"* (Page 2064, Line: 26-27). Is it also possible that this observation might be influenced by a spatial varying coverage of SAR images as *"the interpretable slope portions are those oriented to the N, NW, W, SW and S…"* (Page 2058, Line 19-20)? Is it possible that landslides located on East/South exposed slopes are less visible due to a higher proportion of forest cover (connected to a lower visibility of landslides; see next section)? Are there other confounding factors which might distract the observed relation? I recommend being very **cautious in deriving causal relationships** from empirically observed associations.

From my perspective, systematic and spatially varying biases (e.g. due to heterogeneously distributed land cover units, spatially varying coverage of documents) are of crucial relevance for this study as the landslide database was generated from different data sources (e.g. orthophotos vs. InSAR) and for a study area highly influenced by humans. I suspect that the resulting synoptic landslide inventory might be affected by a large variety of uncertainties, as every data source (e.g. orthophoto vs. SAR vs. geomorphic mapping) has both, advantages and drawbacks. I think that such **uncertainties** highly influence the validity of the results and **should be thoroughly addressed within this paper**. In this context, I recommend to also formulate the empirical results of this study more cautiously as a high (or low) proportion of mapped landslides within one area might not necessarily correspond to a “real” higher (or lower) proportion of landslides on this location. Examples of systematic inventory based biases can be found in Brardinoni et al. (2003) and Bell et al. (2012). For instance, Bradinoni et al. (2003) showed that landslide inventories mapped from orthophotos might be considerably less complete on forested areas. On the other side, Bell et al. (2012) discussed, that the visibility of landslide features in time (landslide persistence) might be dependent on the degree of human impact (e.g. land levelling) on the landscape. Thus, mapped landslide inventories were expected to be more complete on areas less influenced by human activities (e.g. forests, remote areas). From this perspective, a **provision of information on past land cover changes and the spatial distribution of land cover units** might be insightful. From my perspective, land cover might also act as a proxy for one source of uncertainties.

I think that the uncertainty index introduced in this study already addresses potential limitations involved within the study. However, I suggest **embedding this qualitative uncertainty index into a broader conceptual framework** with a clear structure and a definition of the underlying criteria (maybe by designing a decision tree?). The previously recommended insertion of a discussion section might enable a thorough confrontation with the limitations of the conducted analyses.

Bell, R., Petschko, H., Roehrs, M. and Dix, A.: Assessment of landslide age, landslide persistence and human impact using airborne laser scanning digital terrain models, Geografiska Annaler: Series A, Physical Geography, 94(1), 135–156, doi:10.1111/j.1468-0459.2012.00454.x, 2012.
Brardinoni, F., Slaymaker, O. and Hassan, M. A.: Landslide inventory in a rugged forested watershed: a comparison between air-photo and field survey data, Geomorphology, 54(3-4), 179–196, doi:10.1016/S0169-555X(02)00355-0, 2003.

(iv) I recommend **specifying the criteria** used to set classification thresholds, grid resolutions and area selections (e.g. selection of 110 SAR signals from 340; subdivision of the study area into three geomorphic units; landslide density classes and underlying grid resolution; landslide-threshold (250m², 0.04%). In the case of a certain criterion being based on expert opinion, I suggest formulating corresponding findings cautiously (e.g. 110 SAR images were expected/estimated/assessed to correspond to landslide events → not: “110 SAR images correspond to…”). It might be valuable to adopt similar formulations throughout the paper. I find it necessary **defining crucial expressions/terms** used within the manuscript (e.g. magnitude and intensity of landslides; relict, dormant and active landslides)

Since I am not an English native speaker, I do not intend to judge the language quality (e.g. grammar).