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Reply on RC1

David Graham Milledge et al.

Author comment on "Automated determination of landslide locations after large trigger events: advantages and disadvantages compared to manual mapping" by David Graham Milledge et al., Nat. Hazards Earth Syst. Sci. Discuss.,
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Thank you for your thoughtful and detailed review. Your comments are much appreciated and we are convinced that they have helped us to considerably improve the manuscript. We hope that the responses below, together with the modifications in the manuscript address all your concerns. We respond to your major comments here then provide detailed responses to all comments in the attached response document.

MC1.1: The writing is per se quite clear but I'm to say that I had to read the paper several times to pull all the things together. The description of some steps arrives too fragmented, making the reader somehow supposing what will be next or giving the reader the possibility of taking wrong directions/expectations (e.g. I had to wait almost till the end to understand what the 'trade-off' of the time series was and how the length was selected, and the connection with natural processes).

The reviewer's concern here is that the material is introduced in a fragmented way risking misinterpretation or allowing them to develop incorrect expectations. To address this general point we have thoroughly edited the paper to make our explanations more coherent and to clarify the structure. We deal with the reviewer's specific example (explanation of time series length) in detail where it is raised again in minor comments below.

MC1.2. 3.1 is maybe too generic in the introduction of the algorithm, in how to prepare the time series, what the distributions are, and that a fitting method is used to find the parameters (they sound to me all parts of the method). I suggest trying & anticipate some concepts.

This section started from very basic concepts and we appreciate the reviewer's advice to expect that the reader already knows some of this background (or that they can easily find it out). We have now modified the section, removing some of this unnecessary background and instead focusing on the algorithm from the start of the section.

MC1.3. I also think that in the first 4 paragraphs some of the preparatory steps, comments, results, and interpretations (see detailed comments) are 'one way,

not necessarily wrong but too biased to demonstrate the outperforming of the automatic method. In the discussion, this is a bit relaxed.

We believe the reviewer was concerned about a potential bias in the paper, with the first four paragraphs as an example, and other examples picked up in detailed comments below. Specifically the reviewer's concern was that prior to the discussion we implied that outperformance in relation to one metric equated to outperformance in general. The bias was not intentional and we are thankful for the opportunity to clarify our writing. We have done so by: 1) specifying the key characteristics of landslide inventories and associated metrics of inventory quality in the introduction, and specifically differentiating between prediction of landslide location and geometry (L58-61 and L132-135); 2) introducing both location and size metrics within the 'performance metrics' section of the methods; 3) examining both metrics individually in the results, and then in Section 5.4 of the discussion clearly stating that ALADIN is comparable to manual mapping for location metrics but is worse with respect to size; 4) summarising findings in relation to the full set of metrics that we have examined in the abstract.

MC1.4. In fact, the basic assumption that manual mapping is accepted as the most accurate method to map landslides is taken in a too broad sense and it is not critically reviewed neither contextualised.

Rather than argue whether manual mapping is most accurate (or perceived to be so) we have revised the section to frame manual mapping as the most common method. There is a distinct lack of studies in the literature comparing manual and automated mapping and we see this as one of the contributions of this paper.

MC1.5. The preference is in most of the paper given to the automatic mapping considering only some performance indices, but it does not take into consideration elements like the purposes of producing landslide inventories (in particular just after an event), the time needed to have long and adequate temporal series of satellite images to stabilize the signal in ALDI (at least one year if the sampling is consistent). The inability of the method to trap correctly small landslides is shown as a very secondary aspect, and the fact that, despite their presumed low qualities, manual inventories were used to tune the model (also the general one) is not remarked (without them ALDI could not be tuned). This is, as correctly stated by the authors, without a real benchmark

This is a very good point, and we are thankful for the suggestion to consider the purposes of producing landslide inventories. We have added a few sentences in the introduction to define and explore these purposes and two paragraphs in the discussion (Section 5.4) to examine which purposes ALADIN would be more or less suited to.

The reviewer identifies a two of important limitations to the scope within which ALADIN can be used. First, the minimum mappable landslide size is still quite large (900 m²); this was already discussed in the limitations section (Section 5.3) but is now introduced more clearly in the description of size-frequency results and re-visited in (Section 5.4). Second, ALADIN requires more than two years to have passed since the event, precluding its use in disaster response. We now emphasise this in our discussion of potential applications in Section 5.4.

Finally, the reviewer points out that the method has been trained using manually mapped inventories, albeit with a treatment of their uncertainty. However, we note that our global model can now be applied without the need for further manual mapping (i.e. no training data required) because the sites tested to date provide parameter sets that can be used 'blind' at new locations. We tested this 'blind' application using a holdback test and found that our conclusions hold even when global or holdback parameter sets are used.

MC1.6. Nevertheless, I see some potentialities in the method (when better contextualised, and without unbalanced comparisons) to say that, given some inventories, it is possible to run it to update, or extend, or give homogeneity to the preexisting inventories (after many years of data acquisition), indicating the way of correctly using this type of product (for sure not in an emergency since it takes years to have the post-event time series).

We thank the reviewer for their suggestions and have added text in the discussion to expand on these potential applications and to highlight areas where manual mapping remains more appropriate. We have sought to address the reviewer's concern that the method be 'better contextualised' by explaining more explicitly when ALADIN maps would or would not be useful (Section 5.3 on limitations, and 5.4 on performance of ALADIN relative to manual mapping). We have sought to address the reviewer's concern around unbalanced comparisons by providing a more balanced evaluation of both landslide location and size detection in the revised manuscript.

In the detailed comments, I raise some issues related to some methodological steps that should be better explained or clarified.

Thank you for these helpful comments.

MC1.7. Last, some of the elements in fig 4 (distributions), 8, and 9 are very difficult to catch.

We have edited these figures for clarity, and we have included descriptions of the key features in the text, so that the reader can understand our interpretations. We are not sure what specific elements the reviewer is troubled by, but we are happy to revise the figures further if more detail can be provided.

MC1.8. I recommend for major review, and I strongly suggest the authors for a more adequate and multi-perspective contextualisation (maybe starting from the title, the outperformance is not absolute, but eventually relative to some choices).

We agree that the outperformance is not absolute. In some respects manual mapping outperforms ALADIN, but in others ALADIN outperforms manual mapping. In addition, as discussed above, performance in reproducing landslide location is distinct from performance in reproducing landslide size or geometry. The title is a point of concern for both reviewers and we have sought to relax the title slightly to reflect their concerns. In particular, we believe that the reviewers' concerns are primarily that the term 'outperforms' needs contextualising. There is not sufficient space to do this in the title so we instead choose a more general summary of the paper's content.

"Automated determination of landslide locations after large trigger events: advantages and disadvantages compared to manual mapping"

The new title also reflects what we believe to be a more balanced examination of the performance of manual and automated mapping recognising the contexts in which one or other might have an advantage. We thank the reviewer for their recommendations, both major and minor, which have prompted a thorough revision of the paper.

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2021-168/nhess-2021-168-AC1-supplement.pdf>