We thank the referee for providing a review of our manuscript. We note that the tone of the review perhaps reflects a misunderstanding of our primary aim: to focus upon the process, timing and decisions made in generating a rapid landslide assessment immediately after an earthquake. Reviews of this type of experience, and the lessons gained from it, have not been published to our knowledge. Based upon our own effort following the 2015 Gorkha earthquake, we argue that such a discussion is of value for those engaged in similar activities. Importantly, our research shows that the availability of imagery is only one of many constraints in providing a timely and useful assessment of landsliding that is of value to disaster response practitioners on the ground. Moreover, while protocols for the use of earth observation data for other geohazards are well-established, those for rapidly assessing landslides remain absent. Importantly, our inventory, and the submitted paper that describes its creation, are not intended to compare to other earthquake triggered landslide datasets. To make the unique focus of our manuscript clear, we suggest amending our title to: '*Satellite-based emergency landslide mapping: Experience and reflections from the 2015 Nepal earthquake*'.

Below we provide responses to each of the comments made by the reviewer.

(1) The paper does not presents a typical research based on the analysis of data, but rather a speculation on the problems related to landslide mapping in emergency condition.

We define our aim on P1L19: '*we share the lessons learned from the Gorkha earthquake, with the aim of informing the approach taken by scientists to understand the evolving landslide hazard in future events and the expectations of the humanitarian community involved in disaster response*'.

The paper reports on our experiences. While this may not constitute a typical presentation of data, in our view it is not at odds with the interests of the journal readership. Where limitations in the method are identified, they are based on our own experiences rather than speculative. The prioritisation of image attributes to enhance the efficiency of manual mapping (Fig. 1), and the chronology of landslide assessment outputs (Table 2) are two of a number of instances where we feel our experiences are of value to those responding to future landslide disasters. Our discussion is intended to initiate further discussion around the timeliness of landslide mapping relative to the rapid rollout of a typical disaster response (Section 4.2), and the inevitable time-limited choices that have to be made (Section 4.3). The authors have been involved in generating landslide inventories for numerous previous earthquakes for scientific ends. However, in attempting to supplement the humanitarian response following the 2015 Nepal earthquakes, we found no guidance, documented experience or reflections upon the best practice for rapid landslide assessment within the timescales described here.

(2) The first and main problem is that the paper is based on a single case study.

It is true that our paper builds upon a single case study but we do not believe that it constitutes a problem. A discussion of what is feasible and useful to generate in the aftermath of a disaster is important to initiate, in order to provide a more efficient, coherent, and useful response. We present general and transferable observations that we anticipate will be more widely relevant beyond a single case study.

(3) The four figures presents four successive steps of advancements of the inventory, and have been published online first, and they are probably still available in the HDX site (https://data.humdata.org/).

The reviewer refers here to Fig. 2 – Fig. 6. The data displayed are avaialble on HDX as polylines, and was previously disseminated to inform the post-disaster response in 2015. The maps presented in the manuscript based upon this data are entirely new, and have been formatted to fully represent the development of the mapping (e.g. density maps showing landslides per square kilometre are represented using a consistent colour ramp).

(4) The real problem of the paper is that the general conclusions on the emergency mapping of landslides are rooted in this specific case study. While they pretend to be "general", they are indeed "specific".

We disagree with this statement for the reasons outlined above. To reiterate, we strike a balance between learning from a detailed case study, and drawing general and transferrable conclusions. We draw the reader's attention to the findings described in Table 2, which have no specific reference to Nepal. With the possible exception of cloud cover (see '5' below), the decisions associated with image selection in Figure 1 are also not specific to this case study, and will be the same for any situation requiring rapid landslide mapping, either now or into the foreseeable future.

(5) One of the conclusion is that manual mapping is not fast enough (session 4.2), and a faster approach is needed (Robinson et al, 2017 is cited as an example). Indeed, the delay in mapping in Nepal was mainly due to the clouds that covered the sky soon after the earthquake. This required a few days to be have good images available. However, this is not always the case. For instance, if good images were available since the first day, one could have mapped hundreds of landslides within 4 or 5 days (consider that a good geomorphologist could map tens of landslides a day).

We refer the reviewer to Table 2, where a detailed chronology of the landslide assessments generated is described. Following the first cloud-free imagery, the production of an initial landslide assessment and inventory was available within approximately five days, reflecting broadly the timescales outlined in the reviewer's comment above. Our paper highlights that, while this is possible, a set of decisions is still necessary in terms of defining image selection, mapping protocol and outputs. Some of the most useful outputs from this assessment were derived in the first days after cloud-free imagery, which were provided without mapping individual landslides (landsliding extent, southernmost landsliding limit, hotspots of landslide impacts). We therefore argue that setting out to generate a detailed landslide-by-landslide inventory may not always be the most beneficial activity to those responding on the ground. Given the importance attached to cloud-cover in post-disaster landslide mapping, we intend to clarify its impact on the content of table 2 as follows:

P15L5: *The various means of landslide assessment that have been discussed above are summarised in Table 2. This provides a chronology of outputs that clarifies what we have found possible to achieve within the timeframes of the UN Situation Analysis and MIRA report. The timescales of what is possible will vary between events, predominantly as a function of cloud cover for landslide mapping, but the suggested timescales in Table 2 are broadly independent of this. For example, following the first cloud-free imagery after the Gorkha earthquake, the production of an initial landslide assessment and inventory was available within approximately five days, as reflected in the description of a full point inventory.*

Where an earthquake triggers thousands of landslides, we emphasise that manual mapping alone cannot respond quickly enough to meet the needs of the initial disaster responders, whether clouds are present or not. We do not advocate for landslide modelling instead of manual mapping. We highlight that initial mapping can help to refine landslide modelling (hence the reference to Robinson et al., 2017) while also providing valuable information on the scale, extent, and distribution of landslide impacts across the entire affected area within the first 72 hours (even using only small gaps in cloud). Uniquely, our discussion also draws attention to the needs of disaster response, and how they shift quickly from a broad overview to increasingly local and specific details of individual failures. A method to map thousands of individual failures as efficiently as possible has therefore been described. We also highlight the importance of manual mapping in the identification of secondary hazards, such as landslide dams. We agree that every disaster will be different and so some of the issues raised will inevitably not always play a role. However, it is important to discuss the usefulness and feasibility of various post-earthquake landslide assessments with regard to the timescales, priorities and expectations involved.

(6) Hence, manual mapping is not the issue. The issue is how good the weather is (for instance) …

We refer the reader to our previous responses and have clarified the impact of weather on the paper's wider applicability in response to the review by Odin Marc. Given that our review focusses on SEM for landslide assessment, it is important to note that the settings that are relevant here are steep and mountainous, increasing the potential for cloud cover. As noted on P4 L26, conditions that are ideal for landslide mapping are often not present or coincident in mountainous regions. This is particularly so if widespread landsliding has been triggered by a storm event, to which the manual mapping method described is also applicable. In light of this, we intend to clarify the importance of cloud cover as follows:

P5L10: *Given that landslides typically occur in steep and mountainous regions, often following prolonged rainfall, the potential for cloud cover in imagery is a key consideration for associated SEM. The Nepal Himalaya, for example, are obscured by cloud between mid-June and mid-September each year, during which time an estimated 90% of annual fatal landsliding occurs (Petley et al., 2007).*

(7) … and how lucky we are in having satellites ready to take the images on time.

A key finding from the manuscript is that it cannot be assumed that, once an image is captured, a landslide inventory or assessment will become readily available (Section 4.4). This is a problematic assumption that raises expectations of both those producing landslide assessments, but also those who could use them. Based on the reviewer's comment, we have sought to clarify this at the end of the second paragraph of Section 4.4:

P14L17: *Our effort demonstrated that once imagery is available, mapping can be rapid (two to three days), given suitable capacity. However, we have also found that it cannot be assumed that a landslide inventory or assessment will become readily available once an image is captured. This is a problematic assumption that raises expectations of both those producing landslide assessments, but also those who could use them.*

While beyond the focus here, the return period of satellites is well known and is ever-decreasing. Sentinel-2, for example, has the potential to considerably increase the likelihood that imagery will be captured across an area within five days of a triggering event. We argue that the experiences presented and guidance for future events derived from our effort in Nepal will gain more importance in a context where satellite imagery is more readily and rapidly available. The need to make choices about the best imagery to use, and the value of different approaches to mapping remains irrespective of the frequency of image capture.

(8) Another conclusion (session 4.3) is that linear mapping was a good compromise between velocity and the need to assess the landslides size, even roughly. Part of reason for this choice is that the georeferencing of Google Crisis maps was very poor, hence hampering a meaningful mapping of polygons. Again, this is not always the case.

We assume the reviewer is referring to our decision to map using polylines as opposed to points or polygons. The locational accuracy of imagery for landslide mapping can be low in places, given that landslides occur in steep terrain where the angle of incidence to the sensor may be high. This has the potential to influence object characteristics (e.g. area) such that comparison between sites or cumulative statistics are of questionable value. Our decision to map polylines therefore relates to the incidence angle of high-resolution imagery in steep terrain, not to the georeferencing quality of online imagery (including Google Crisis). It is therefore a property of the situation described, which is very likely to be repeated, rather than a peculiarity of this particular event and the actors involved. We reiterate our primary reason for using polylines, which is that the approach retains information on landslide scale, location, and intersection with assets, yet is considerably more efficient to map. This is described in detail in Sections 2.3 and 4.3. In the context of disaster response, speed is of the essence.

(9) A second significant problem is that part of the speculations are not supported by any analysis. For example, the potential of crowd-sourced information. I agree that this may be relevant in the future, but the case study does not say anything about that.

Our focus and analysis constitutes a reflection on the mapping process that was undertaken after the Gorkha earthquake. While others have used our dataset to analyse landslide distributions (e.g. Valagussa et al., 2016), this is not our intended aim. Crowd-sourced information was a supplementary component of the Gorkha earthquake response, and is likely to increase in importance as this technology develops. While we were not able to incorporate this into our mapping, we feel it is important to identify the potential benefits and limitations.

(10) A third example regards the comparison of the inventories (session 4.1). The authors state that, even if their inventory has less landslides than the others, it still holds value as a rapid assessment etc. etc. Again, this could be true but it is speculated without any analysis of the

data. In such case, data analysis could have been done by trying to overlap polygons to identify positional mismatch and overlapping ratio.

We agree that the relative intensity and spatial distribution of our inventory could be compared to other coseismic landslide inventories collated for this event. We have since undertaken a geospatial comparison with the inventory reported by Roback et al. (2017) that allows us to qualify these uncertainties directly. While we feel that a full numerical analysis would detract from the focus of the manuscript, and is something that is well-suited for further analysis elsewhere, we will report the analysis that we have undertaken in the Supplementary Materials (see below), and report the following statements at the beginning of Section 4.1 to qualify the statements with regard to the comparison of inventories:

P11L9: *Comparing our rapidly-derived inventory with subsequent, independently collated inventories (Martha et al., 2016; Roback et al., 2017; Tiwari et al., 2017) shows that our inventory underestimated the total number of landslides by up to ~ 19 000. When compared for every 1 km^2 of the landslide-affected area, our inventory underestimates landslide number by an average factor of 1.8, which is broadly consistent irrespective of landslide density. However, the spatial pattern and relative intensity closely adheres to those described in both Martha et al. (2016) and Roback et al. (2017). The overall extent of the mapped landslide affected area are broadly similar (Fig. S1), covering the same geographical footprint. In addition, the locations of highest density landsliding and the southernmost limit of landsliding is consistent between the inventories.*

We again refer the reviewer to the primary purpose of our paper, to provide reflection of the generation of landslide assessment in the immediate aftermath of a large earthquake. In this context, the comparability of these datasets reinforces the value of the rapid assessment presented and discussed here.

(11) The third problem is that large part of the paper is not transferable to other similar emergency situations. For instance, the data selection (session 2.2), and the mapping platform (session 2.3) are very site specific and may be different for other case studies. Hence, a long description of these issues are not relevant, and may be strongly reduced.

This statement appears to summarise the reviewer's previous comments. We therefore refer the reviewer to the response above.

References used in this response

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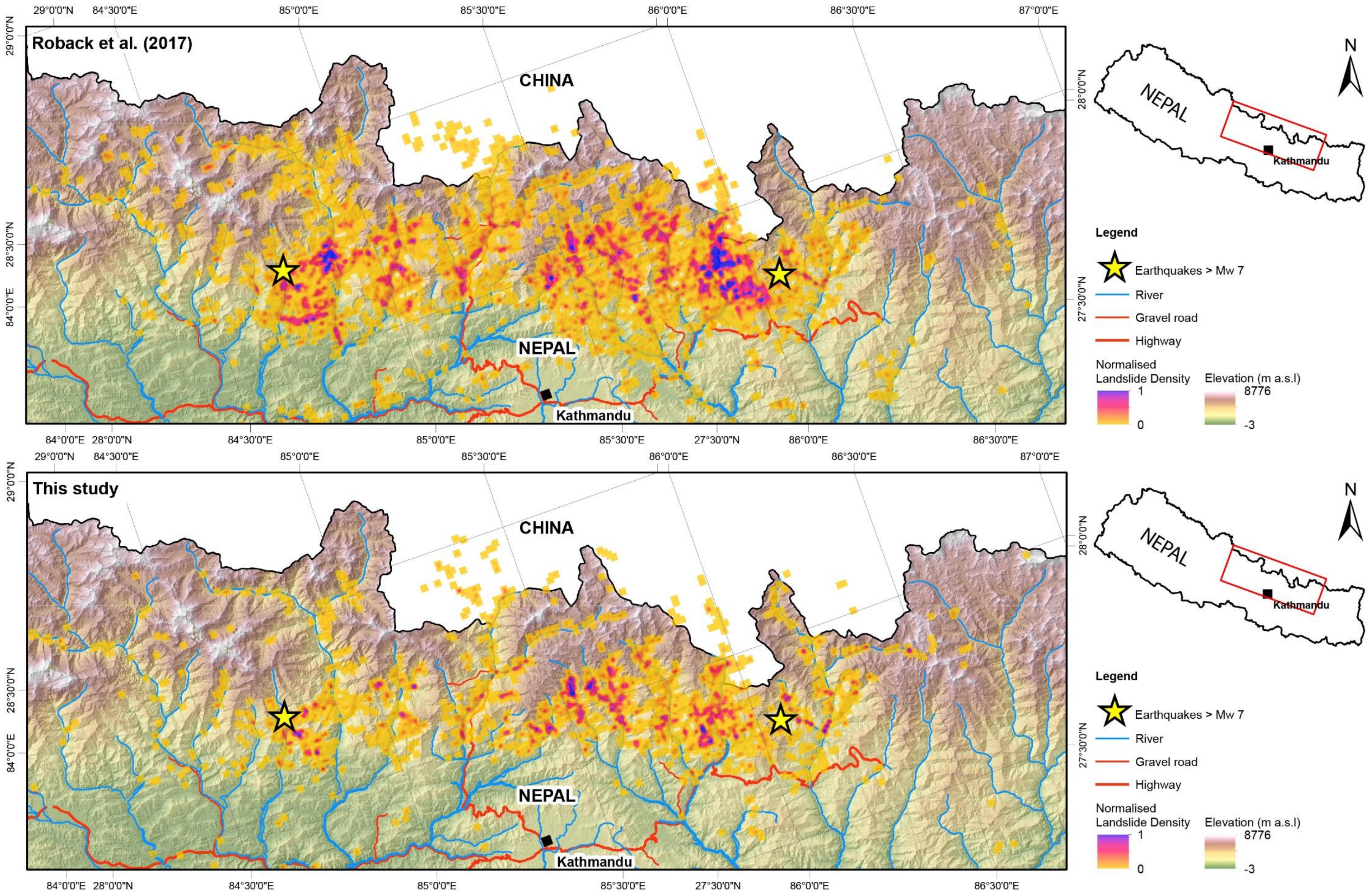
Supplementary materials

Fig. S1. The number of landslides in both inventories was counted for 1 km^2 grid cells occupying the same spatial extents. The result is a map of landslide density, represented as the number of landslides per square kilometre. Given the fivefold increase in the number of landslides mapped by Roback et al. (2017), each grid cell was normalised by the maximum density of landsliding for that inventory (27 for the Durham inventory, 84 for the Roback inventory). This provides a comparable spatial distribution of landslide intensity. This distribution is of greater importance in the context of disaster response than the absolute number of landslides, which inevitably varies with the method of mapping and the level of detail involved.


Roback et al. (2017)
CHINA
NEPAL
Kathmandu
29°0'0"N
84°30'0"E
85°0'0"E
85°30'0"E
86°0'0"E
86°30'0"E
87°0'0"E
28°30'0"N
84°0'0"E
28°0'0"N
27°30'0"N
84°30'0"E
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86°0'0"E
86°30'0"E
N
NEPAL
Kathmandu
Legend
Earthquakes > Mw 7
River
Gravel road
Highway
Normalised
Landslide Density
1
0
Elevation (m a.s.l)
8776
-3
This study
CHINA
NEPAL
Kathmandu
N
NEPAL
Kathmandu
Legend
Earthquakes > Mw 7
River
Gravel road
Highway
Normalised
Landslide Density
1
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Elevation (m a.s.l)
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