Response to reviewer 2

We thank the reviewer for taking the time to read and evaluate our manuscript. The following is a line by line response to their comments (Reviewer comments in bold type).

In this paper, the authors propose a SAR-based technique to estimate the possible time-window of landslides mapped as a part of seasonally generated inventories. To test their methods, they use two rainfall-triggered landslide event inventories and one post-seismic inventory including landslides that might have been triggered by the aftershocks of the 2015 Gorkha earthquake and/or rainfall events that occurred following the event. In this context, I should stress that the paper focuses on an interesting research question for sure and it appears as a nice fit for the journal and, in particular, for this special issue.

However, the authors are able to come up with a time estimation only for 20% of landslides with an accuracy of 80%. Therefore, I doubt if this is successful research in the end. Frankly speaking, I am not sure and just hesitating to say that the results are promising. However, what I can say is the output of this research is not fulfilling what the authors are promising in the abstract/conclusions.

First, we will revise the abstract and conclusions to make sure that it is clear that our method will only provide landslide timings for around 20% of the landslides in an inventory and that we do not promise a full inventory with timings for all landslides. This was not previously clear and we thank the reviewer for identifying this, which has also been identified by Reviewer 3.

Our abstract previously ended: " *our methods allow 20% of landslides to be timed with an accuracy of 80%. This will allow multi-temporal landslide inventories to be generated for long rainfall events such as the Indian summer monsoon, which triggers large numbers of landslides every year and has until now been limited to annual-scale analysis.*" We will change this to "*our methods allow 20-30% of landslides to be timed with an accuracy of 80%. Application of our methods could provide an insight on landslide timings throughout events such as the Indian summer monsoon, which triggers large numbers of landslides every year and has until now been limited to annual-scale analysis*" . This removes the words "multi-temporal landslide inventories", which were misleading since we cannot provide a complete inventory where all the landslides have timings assigned.

Similarly in the conclusions section, we will change "*These methods will allow us to generate multi-temporal landslide inventories for long rainfall events, unlocking comparisons between rainfall data, hydrological models and triggered landsliding*." to "*These methods will provide information on the timings of some individual landslides and allow spatio-temporal clusters of landslides to be associated with peaks in rainfall during long rainfall events, unlocking comparisons between rainfall data, hydrological models and triggered landsliding."*

However, while this is a drawback, we do not believe that it renders the work unsuccessful. First, landslide inventories often include thousands of landslides, meaning that by timing 20% of these, we obtain landslide timing information on a statistically useful number of landslides. Similarly to the large number of studies deriving rainfall threshold our methods would allow us to constrain the rainfall characteristics which have preceded the triggering of landslides (even with a time window of 6-12 days, at the scale of the monsoon it would be a substantial improvement), or to determine whether a subsample of monsoon-induced landslides display spatio-temporal clusters which could be interpreted in terms of triggers.

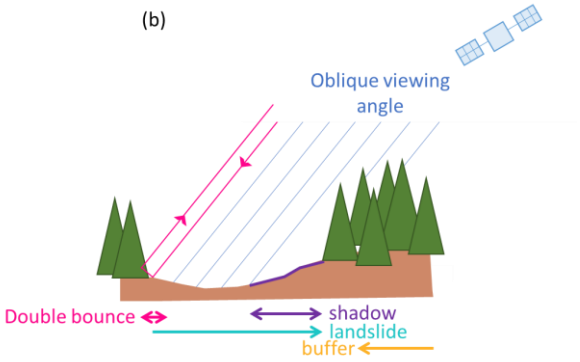
We have also made some adjustments to the methods, which somewhat improve the number of landslides for which we can assign a timing from from 20% to 30% in favourable cases. This does not change the overall message of the manuscript, since we keep the same 80% accuracy and still do not assign a timing for the majority of the landslides in an inventory, but it represents an improvement to the method. All following replies and alterations to the text therefore incorporate these new sensitivity levels.

(i) A new method based on bright spots observed at the edges of landslide polygons

As described in Section 2.4.3 of our original manuscript, shadows are cast by trees at the edges of landslide scars due to the imaging geometry of the SAR sensor. (See figure below, altered from Figure 2b of the original manuscript). On the opposite side of the scar, we may observe a bright patch in the SAR amplitude

images due to double bounce scattering between the exposed soil and trees on the far side of the landslide scar, and the focussing of the microwave energy into a small area (Villard and Borderies, 2007). Similarly to the shadow method, we compare the pre-event and post-event SAR time series and identify pixels which have experienced a strong increase in amplitude (we found a threshold of >5dB to perform best in this case). Incorporating this method means that, increases the final number of landslide that are assigned a timing by 3% in Hiroshima, 2% in Zimbabwe and 7% in Trishuli.

A description of this method will be added to Section 2.4 (SAR amplitude techniques for landslide timing) as " 2.4.4 Method 4: Geometric bright spots"



(ii) Increase in size of the landslide polygons

As described at lines 185-195 and 317-326 of our original manuscript, there may be a spatial mismatch between the optically-derived landslide polygons and the SAR imagery. Previously, we increased the size of landslide polygons by 10m (1 SAR pixel) for the geometric shadows method to try to account for this. However, this effect may also affect the results from other methods, and decrease the number of landslides for which we are able to assign a time window.

Therefore, for landslides that are not assigned a date by at least 2/4 of our methods, we now increase the size of the landslide polygon using a 20 m buffer for all methods and repeat, with the aim of trying to assign landslide timings to some of the landslides that experience this spatial mismatch. This improves the number of landslides we are able to detect in each case study event by 4% in Hiroshima and 5% in Zimbabwe and Trishuli, and will be added to the next version of the manuscript.

Since this will now be described in its own subsection in the methods section, text referring to this at lines 185-195 and 317-326 of our original manuscript will be removed.

This being said, one could consider this paper as a step towards developing better tools along this research direction and in this regard, could be still valuable. And yet, authors do not clearly present their work. Unfortunately, the manuscript is not well written. I had to read some parts more than once to understand the authors' point. The figures are not well designed either. I have many comments that I hope the authors find useful to improve their work.

We will take on board the individual comments throughout the manuscript and the figures and hope that by addressing these, the manuscript will be made clearer.

Last but not least, I would like to test the code/tool they developed but unfortunately, it is not available. This is a preprint with DOI, so I did not really get why it was not shared already.

The code will be provided as a supplement to the next version of the manuscript as requested. I apologise for omitting it, I was not aware it was necessary at this stage.

Overall, I recommend a rejection to give adequate time to the authors for a comprehensive revision for the manuscript for clarity, pulling some of the speculation and assumptions to the discussion, adding more definitions of terms, and framing the paper in hypotheses. This should help the reader

understand what you did and why you did it. Because in the current version, authors do not really help the reader to find their way through the manuscript.

Below I've included line-by-line suggestions and highlighted all these points.

Line 18: "to emergency response coordinators". I would say rainfall-induced landslide inventories are rarely used by emergency response coordinators as they are generated at least weeks or months after an event. But if you are referring to some kind of indirect usage of the dataset (for instance, as an input to develop a landslide early warning system or something) please be more specific.

Line 18: "physical and empirical" there are also statistically-based models exploiting the very same dataset

Lines 18-19: Could you please cite relevant literature.

We have added references to Jones et al., 2021, (estimate of eroded sediment volumes); Ozturk et al., 2021 (landslide polygons used to train and validate regression models) ; Wu et al. 2015 (physical model of rainfall-triggered landsliding tested on optically-derived landslide inventory)

Jones, J.N., Boulton, S.J., Bennett, G.L., Stokes, M. and Whitworth, M.R., 2021. Temporal variations in landslide distributions following extreme events: Implications for landslide susceptibility modeling. *Journal of Geophysical Research: Earth Surface*, *126*(7), p.e2021JF006067.
Ozturk, U., Saito, H., Matsushi, Y., Crisologo, I. and Schwanghart, W., 2021. Can global rainfall estimates (satellite and reanalysis) aid landslide hindcasting?. *Landslides*, *18*(9), pp.3119-3133.
Wu, Y.M., Lan, H.X., Gao, X., Li, L.P. and Yang, Z.H., 2015. A simplified physically based coupled rainfall threshold model for triggering landslides. *Engineering geology*, *195*, pp.63-69.

Line 20: "the size location" the size, location
This typo will be corrected in the revised manuscript.

Line 22: "the size and location" the size, location and timing too. As you said occurrence dates of landslides could not be accurate in some cases via optical images but also, as you said, if we have cloud-free images it is doable.
Optical images *always* be used to get info. on the size and location of landslides, but the timing is not always constrained because of cloud cover, as we go on to describe in the following sentences. We feel it would be confusing to include timing in the list here because it would immediately be contradicted in the following sentence.
Line 24: "Williams et al., 2018; Robinson et al., 2019". These examples are from earthquake-triggered landslide events. But you focus on rainfall-triggered landslides. So, please replace them with some examples of rainfall-triggered landslide events.

It is true these papers focus on earthquake-triggered landslides, but they both focus on areas that also experience frequent rainfall-triggered landsliding. Therefore observations on the usability of optical satellite images for earthquake triggered landslides are also relevant for rainfall-triggered landslides. Furthermore, while we focus on rainfall events, the methods we propose could be applied to any event where the timing of triggered landslides is unknown due to cloud cover. This also includes sequences of earthquakes and storms, for example Lombok, Indonesia (Ferrario, 2019), Papua New Guinea, (Tanyas et al. 2022) and Gorkha, Nepal (Martha et al. 2017).

We will add a sentence on this at line 34 of the original manuscript.

New references:

Ferrario, M.F., 2019. Landslides triggered by multiple earthquakes: insights from the 2018 Lombok (Indonesia) events. *Natural Hazards*, *98*(2), pp.575-592.

Tanyaş, H., Hill, K., Mahoney, L., Fadel, I. and Lombardo, L., 2022. The world's second-largest, recorded landslide event: Lessons learnt from the landslides triggered during and after the 2018 Mw 7.5 Papua New Guinea earthquake. *Engineering Geology*, *297*, p.106504.

Lines 28-29: This line needs to be rewritten. Also, why did you prefer the term "landfall", why not "landslide"

A typhoon 'making landfall' refers to the time when the storm moves over land after having developed over the ocean. We are not using 'landfall' as a synonym for 'landslide'. With this, the sentence no longer needs to be rewritten.

Lines 30-31: Is that the case? Landslides triggered by each of those typhoons were mapped separately or not? It is not clear from the line if you indicate what already happened or this is just a hypothetical remark.

We will change *"If no cloud-free optical satellite imagery is acquired between these trigger events"* to *"If no cloud-free optical satellite imagery is acquired between such successive trigger events"* to make it clear this is hypothetical.

Lines 33-34: "This limits analysis of these landslides to the annual scale (e.g. Marc et al., 2019a; Jones et al., 2021)." But, for instance, Marc and others generated monsoon-induced landslide inventories and to do that you just need pre- and post- monsoon images. So you do not need cloud-free optical satellite images through the monsoon. Please remove this reference and also please be more specific about the limitations of generating seasonal landslide inventories.

Since these multi-temporal inventories are compiled from images before and after each monsoon season, we can only associate them with a given monsoon season and not with any particular peak in rainfall within that monsoon season. In this case, annual and seasonal almost mean the same thing, since almost all the landslide occurring annually in Nepal occur during the monsoon season. To make this clearer, we will change « *This limits analysis of these landslides to the annual scale »* to *« This limits analysis of these landslides to the seasonal scale and prevents association of individual landslides or spatio-temporal clusters of landslides to specific peaks in rainfall.*

Line 35: "Current alternative methods of landslide timing are generally not widely applicable." Please rewrite this line, is not clear what you mean. What are those alternative methods? And why do you think they are not widely applicable (any reference for this?). You haven't said anything about any alternative methods yet. Please first describe them and then you can evaluate those methods based on the literature.

The evidence for this statement comes in the following sentences. We will restructure this paragraph so that this sentence is given as a conclusion at the end. To address this and the following comments on lines 36 and 43, we will rewrite this paragraph, including more recent references :

Previous text: *Current alternative methods of landsliding are generally not widely applicable. Landslides that occur close to inhabited areas or that damage important pieces of infrastructure may be described in news reports or social media (e.g. Kirschbaum et al. 2010) Information on the timing of such landslides can also be generated through interviews with local residents (Bell et al. 2021). Rainfall intensity-duration thresholds have previously been derived for landslides dated in this way (e.g. Dahal and Hasegawa, 2008) and for landslides whose timings and properties are known through monitoring and field surveys (e.g. Guzzetti et al. 2007 ; Ma et al., 2015). However, this is unlikely to be the case for the majority of landslides in an inventory, and will be biased towards populated areas. Seismic recordings of landslides can also provide highly precise information on their timings, but will mostly record large landslides and require multiple seismic stations to allow timing of an individual, localised landslide (e.g. Yamada et al. 2012; Hibert et al. 2019)*

New text: *Landslides that occur close to inhabited areas or that damage important pieces of infrastructure may be described in news reports or social media (e.g. Kirschbaum et al. 2010; Franceschini et al. 2021) Information on the timing of such landslides can also be generated through interviews with local residents (Bell et al. 2021) and through citizen science initiatives (Sekajugo et al. 2022). Rainfall intensity-duration thresholds have previously been derived for landslides in this way (e.g. Dahal and Hasegawa, 2008) and for landslides whose timings and properties are known through monitoring and field surveys (e.g. Guzzetti et al. 2007 ; Ma et al., 2015). However, such information on landslide timing is unlikely to be available for the majority of landslides in an inventory, and is usually biased towards populated areas and areas accessible by road (Sekajugo et al. 2022). Seismic recordings of landslides can also provide highly precise information on their timings, but will mostly record large landslides and require multiple seismic stations to allow timing of an individual, localised landslide (e.g. Yamada et al. 2012; Hibert et al. 2019). Current methods of obtaining landslide timing information in the absence of cloud-free optical satellite images are therefore not widely applicable.*

Line 36: But this is not the method Kirshbaum and others or if you take a look at more recent literature Franceschini and others (DOI 10.1007/s10346-021-01799-y) used, this is the source information for them. Please tell us the method they used.

See changes to text above

Line 43: "will" Why did you switch to the future tense

See changes to text above. *"will be biased"* changed to "*is usually biased*"
Line 49. Please put a full stop before giving the example.

A full stop is not needed here as this is all one sentence. However, it was broken up by our placing a list of references halfway through. These will be move to the end of the sentence

Previous text : "*Numerous studies have shown that SAR data can be used to detect the spatial distribution of landslides in the case where their timing is already known (e.g. Aimaiti et al. 2019; Burrows et al. 2019, 2020; Ge et al. 2019 ; Konishi and Suga 2019 ; Masato et al., 2020 ; Mondini et al. 2021 ; Yun et al. 2015) , for example in the case of earthquake-triggered landslides."*

New text : "*Numerous studies have shown that SAR data can be used to detect the spatial distribution of landslides in the case where their timing is already known, for example in the case of earthquake-triggered landslides (e.g. Aimaiti et al. 2019; Burrows et al. 2019, 2020; Ge et al. 2019 ; Konishi and Suga 2019 ; Masato et al., 2020 ; Mondini et al. 2021 ; Yun et al. 2015)."*

Line 57: "timed landslide information" this is the first time that I have heard this term and it sounds weird, please rephrase it. And please do it not only here but through the manuscript.

We will change "*timed landslide information"* to "*information on landslide timing"*

Line 61: "three potential landslide timing methods" you haven't said anything about these methods yet, so it is not clear what these methods are.
To improve clarity, we will change "*We use Sentinel-1 time series over inventories of landslides whose timing is already known to test three potential landslide timing methods individually and in combination"* in the previous version of the manuscript to "*We present four methods to constrain landslide timing using Sentinel-1 SAR time series and test these on inventories of landslides whose timing is already known."* in the new manuscript version

Line 63: "Case study events" does not sound right. Please revise it. e.g., Case studies or Landslide inventories
We will change this to « Case studies »
Line 66: Why did you take 20 pixels as your threshold? Why not 10? It could be better to do it without any filtering first. And then, you can identify the threshold for the landslide size that your method works well.

Statistics we use, such as the standard deviation of pixels within the landslide, require the landslide to contain multiple SAR pixels. As shown in Figure 5d-f (original manuscript) larger landslides are both more likely to be assigned a time window, and more likely for this time window to be correct. Furthermore since landslide-area distributions obey a power law**,** decreasing the threshold to 1000m^2 would greatly increase the number of landslides required to be processed.

Line 68: "inventories of landslides" landslide inventories. Btw, you do not really need to cite Emberson et al. (2021) for the Hiroshima inventory because it was already available, right?

We will change « *inventories of landslides* » to « *landslide inventories* »

The Hiroshima inventory was not mapped by Emberson et al, but it is included (along with the Zimbabwe inventory) in the landslide inventories analysed in that study. The reference to Emberson et al. here (line 68 of the original manuscript) refers to both of these inventories. However, elsewhere in the manuscript, we will change the reference for the Hiroshima inventory from Emberson et al. (2022) to the association of japanese geographers, 2019 (e.g. at Line 414 of original manuscript)

Line 70: "heavy rainfall event which took place from 28 June to 9 July 2018" I guess these are the dates that they were able to acquire pre- and post- event images to map landslides, right? But was the study area also exposed to heavy precipitation during the entire period? It would be useful to see the amount of precipitation (as time series) that each of your study areas received during the periods under consideration.

No, these are not the dates of the acquired images, they are the days between which there was heavy rain over Hiroshima as specified by Hashimoto et al. (2020). We will attach rainfall time series (NASA GPM product) for Hiroshima and Zimbabwe as a supplement so that the extents of the event are visible. We agree this would help with visualising the timelines of the events.

Line 74: "Planetdove" Planet Scope?

Yes, Planet Dove are the satellites, PlanetScope is the name of the constellation so it is better to use this. Thank you for correcting the mistake.

Lines 74-75: "images acquired on 20 and 24 March….the majority of landsliding occurred between the 15-17 March" You mean, they did not examine pre and post images to identify landslides solely triggered by the rainfall event, is this correct?

Apologies, this was not clear, we should have specified that the images acquired on the 20th and 24th of March were post-event images.

"*This inventory was compiled as part of the study of Emberson et al. (2021) using Planetdove optical satellite images acquired on 20 and 24 of March"* at line 74 of the original manuscript will be changed to "*This inventory was compiled as part of the study of Emberson et al. (2021) using post-event PlanetScope optical satellite images acquired on 20 and 24 of March."*

Line 77: I thought you focus on rainfall-triggered landslides as also you indicated in the title of your manuscript. Why do you use the co-seismic landslide inventory of Roback et al. (2018)?

Ok, now I understand what you have done. You removed landslides triggered by the mainshock and work with the others. However, you do not know if these landslides were triggered by the aftershocks or rainfall events. Also, it is not clear when they were triggered. The confusing thing is you mentioned that you focus on "inventories of landslides whose timings are known a-priori to test and develop landslide". However, this does not one of those inventories.

This is not correct. As stated at line 87 of the original manuscript "*we also removed all landslides whose trigger was specified by Roback et al. (2018) to be something other than the mainshock"* i.e. we remove landslides triggered by aftershocks or by rainfall and keep only the landslides triggered by the mainshock. Since the sentence was not clear before, we will change to "*we also removed all landslides specified by*

Roback et al. (2018) to have been triggered by an aftershock or by rainfall, and use only those triggered by the mainshock in our analysis." For landslides triggered by the mainshock, their timing is known, which should resolve the confusion here.

Why do not you simply pick another rainfall-induced landslide event inventory?

The reason we do not use a rainfall-triggered landslide inventory here is that Nepal is a country for which the methods we develop here will be especially useful due to its extensive annual monsoon-triggered landsliding and long periods of cloud cover. It is also particularly challenging for SAR applications due to its steep topography, which results in distortion of the SAR images. It is thus particularly important to test the methods in this environment. Unfortunately, rainfall-triggered landslide inventories of known timing are not available here, so we use an inventory of earthquake-triggered landslides instead.

To improve the clarity, we will make the following change (Line 79 of original manuscript)

previous version : « *It is therefore useful to test landslide timing methods in this area, and, since well-timed landslide information is not widely available, we used earthquake triggered landslides...*

new version : *The steep topography of Nepal also makes it particularly challenging for SAR applications as it leads to distortion of the SAR imagery. It is thus important to test landslide timing methods in this environment, but inventories of rainfall-triggered landslides of known timing are not available. Therefore we instead used earthquake-triggered landslides…*

65 timing methods.

Apologies, but I cannot see any error on line 65

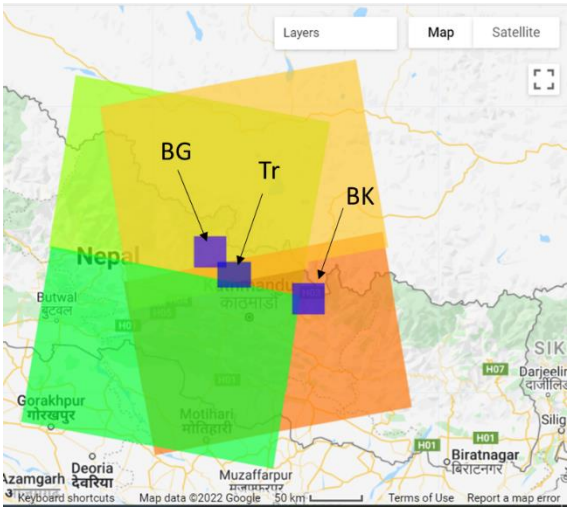
Line 81: "we used earthquake-triggered landslides, which can be assumed to occur concurrently with the ground shaking" you already said it above, please remove this.

We have removed the second part of this sentence, which as you point out, had already been stated at line 50 of the original manuscript.

Lines 81-83: "since the inventory of Roback et al. (2018) covers a large area, with different areas having different Sentinel-1 coverage, we focused on triggered landslides within three large valleys" This does not explain why you focus on these three rectangular areas (actually one of them has a weird shape). You can cover a larger area mapped by Roback et al. (2018) and one Sentinel 1 image should be covering at least an area covering two of those rectangles. So how did you identify these rectangles really?

The valleys were selected since Marc et al. 2019a have mapped landslides in these valleys during several monsoons following the earthquake and therefore will be of interest for future studies.

In fact, the Sentinel-1 time series is different for each Valley, making it complicated to combine these (See Fig.1). Although tracks 19 and 85 cover both Trishuli (Tr) and Buri Gandaki (BG), track 85 is much more complete over Trishuli, which is further South than Buri Gandaki, which is further north. The two valleys lie in the same track but within different scenes and several scenes are missing on track 85 for the Buri Gandaki Valley. This is probably because the satellite had not long been operational at the time of the earthquake. It is therefore not possible to combine them into a single time series. I attach a map below to illustrate this. Scenes on the descending track (19) are shown in green. In this case, BG and Tr are in the same scene (Bhote Kosi (BK) is outside and belongs to track 121 instead). On track 85 (shown in orange), BG and BK are in different scenes, while Tr lies in an area where the two overlap. Since data for Tr can be supplied from either the northern or southern scene, it has a more complete time series than either BG or BK. It is for this reason that data from track 85 is only used in Trishuli, while BK and BG, we are limited to the descending tracks (121 and 19 respectively).



Finally, the squares on Figure 1c do not show the exact extents of the inventories but show the locations instead, as in figures 1a and 1b, where the study areas are also not square. In order to be clearer, we will replace the squares, which are not particularly informative, with plots of landslide density for each event. This should make the extents of each inventory clearer.

Lines 82-83: "large area & large valleys" please be more specific; either tell it directly or not mention it at all.

The area covered by the inventory of Roback et al is 28,000 km^2, this will be given in brackets in the revised manuscript.

« large valleys » will be changed to « *valleys »* in the updated version of the manuscript

Line 83: "valleys see large numbers of rainfall-triggered landslides" it does not sound correct, please fix the language.

"see" will be changed to *"experience"* in the next version of the manuscript

Line 84: "the timing of which would be one of the key applications of our method" Please remove this line. You already indicated your motivation.

This will be removed in the revised manuscript.

Line 89: You are using the inventory mapped by Roback and others but for some reason, you are citing Marc and others. Too much self-citation, remove Marc et al. (2019)

Roback et al. do not draw any conclusions about landslides triggered by the 12 May aftershock, since most of the images they used to map the landslides were acquired between 2-8 May (i.e. between the mainshock and the aftershock). Therefore their inventory does not provide information landslides triggered by aftershocks and we cannot cite their paper here. However, landslides triggered by the aftershock have been observed in Marc et al. (2019a) and Martha et al. (2017).

Line 90: "close enough" not clear what this means. What would be close enough? What were the PGA or PGV values at those valleys? Did Martha and others map no landslide at those valleys and say this based on their observations? Or is this just an interpretation?
To improve clarity, we will change "*Of the three valleys we consider here, only Bhote Kosi was close enough to the epicentre to be affected by that event*" to "*Of the three valleys we consider here, landslides associated with this aftershock have only been observed in the Bhote Kosi (Martha et al. 2017), which was the closest to the epicentre.*"

Figure 1: If you have such a plot (i.e., panel d) then please indicate ascending and descending images in panel d. This will be specifically important to see the dataset you used in the Gorkha case

where some of the ascending images should be missing. Please properly indicate what those abbreviations stand for (e.g., H in panel a and so on).

Figure 1 will be redesigned to be made clearer in the next version of the manuscript. We will use the change suggested elsewhere in this review (the comment on line 141) to change e.g. H083A to H_asc. With this labelling, it should be clear which tracks are ascending and which are descending

It is not clear how did you define pre-, co- and post- event image acquisitions. Do you explain this later in the method section? But you already refer to the term "co-event pair" in line 91, so the reader needs to know what it means. For instance, you indicate that in the Hiroshima case the heavy rainfall event occurred between 28 June and 9 July. Therefore, landslides were triggered (or mapped, as I mentioned this is confusing anyway) during this 10-day period. And you also mention that landslides were most likely triggered between 6 and 7th of July. Then why do you have such a large time period for "co-event pair"

I apologise, we have generated confusion by using co-event (no quotation marks) to refer to the true pair of images spanning the landslide timing (as in line 91) while using "co-event" (with quotation marks) to refer to a "co-event" period that we have defined for testing the methods (i.e. a series of images that contains the correct co-event pair). Evidently, these are too similar and we will change to an alternative e.g. "trigger pair" to refer to the pair of images that span the trigger event and "co-event series" to refer to the series of images we have designated as co-event for testing purposes.

Also, why do you represent the real event date as a single day? You mentioned above some time slots that landslides were most likely triggered. Why do not you indicate them also in panel d?

Yes, that is correct. We have selected day 0 to correspond with the peak in rainfall, however the rainfall events were longer than 1 day, therefore the line should be thicker in places (as in e.g. Fig. 4). This will be changed in the next version of the manuscript.

Line 92: "these two earthquakes can be considered as a single triggering event in Bhote Kosi" You can not consider them as a single event. However, if you cannot differentiate landslides possibly triggered by different factors for a given period of time, this needs to be indicated as a source of uncertainty in your analyses. I do not know what could be the consequences, but obviously, this needs to be discussed later on in the manuscript.

While it is true that in terms of the spatial distribution of landsliding, we cannot consider them as a single trigger, that is not the aim of this study. Here we are trying to identify the timing of a landslide whose location we already know. Both the mainshock on 25 April and the aftershock on 12 May lie between our SAR acquisitions on 24 April and 18 May, it does not matter if the landslide happened at the time of mainshock or the aftershock, an assigned timing of 24 April – 18 May is right in either case.

I think this confusion has again arisen from confusion between our defined "co-event" 6 month time series and the correct co-event image pair. (i.e. the same as at line 91). As previously mentioned, we will be more careful to distinguish between these two in the next version of the manuscript.

We will replace "*these two earthquakes can be considered as a single triggering event in Bhote Kosi"* with "*these two trigger events are blended by our methods into a single time window*" to make this clearer.

Lines 131-133: Could please explain how you defined these time windows (i.e, 6, 3 and 2 months)? What is the logic behind it?

6 months is the approximate length of the monsoon season in Nepal (e.g. May – October), and we expect that our methods will be useful applied to monsoon-triggered landslides. Altering the length of this window is later explored in Fig 7. / Sect. 4.3 of the original manuscript.

The periods of 2 and 3 months were selected to strike a balance between having enough images to calculate reliable statistics and having too much time elapse over the course of the time series (which could lead to incorporating landslides from previous monsoon seasons etc.). Using more images also increases the computation cost.

Line 131: “approximately six months” Later on you are saying two cases you took it as 6 months and in another one like 5 months. No need to repeat the same things. Please remove “approximately six months”

We will rewrite lines 131-138 of the original manuscript to reduce repetition and explain the choice of 6 months, 2 months etc.

Line 139: “In this figure” Which figure? Figure 1d? Then say it, please.

“this figure” at line 139 of the previous manuscript will be changed to *“Fig 1d”* in the revised manuscript

Line 141: “ascending track 72 over Zimbabwe will be referred to as Z072A” this is not a good idea. Why don’t you refer to it, for instance, as Zimbabwe-asc or Z-acs. Or something like we can easily understand what you are referring to.

Yes, there is no need to include the track number here since there is never more than one ascending or descending track used in any event. Therefore we will write the acronym as e.g. Z-asc. This will be changed throughout the text and figures of the revised manuscript

Line 143: Please make a kind of introduction and tell us that you will introduce three different methods for some reason. And please indicate that reason too. It is difficult to follow the text. You are explaining your method (which is ok, I do not have any complaints) but if we do not understand why you are providing this information, we cannot follow you.

We will add an introduction to Section 2.4 to make the section easier to follow

Line 150: “pixels that are dissimilar to those within the landslide, for example pixels located on the opposite side of a ridge, in a river or with different surface cover” Could you be more specific? How do you define similar and dissimilar pixels? Based on what? Based on land cover? Or do you have some other criteria you take into account?

I see, in the next lines you are explaining those variables. But please first tell us what we are talking about (i.e., what you mean by dissimilar pixels) and then you can mention that you removed them.

This is described in the following lines. We will restructure this paragraph to make it clearer

Line 151: “three surfaces” three variables might be better

“three surfaces” in the original manuscript will be changed to *“three variables”* in the revised manuscript

Line 154: “amplitude variability” is this the third one? You mentioned the first and second variables but which one is the third?
Yes amplitude variability is the third. We will change

Second, we used a stack of N pre-event SAR images i (Fig 1) to calculate the mean amplitude $A_{mean,j}$ and amplitude variability $\Delta A_{mean,j}$ for every pixel j through time.

In the original manuscript to

“*Second, we used the mean amplitude $A_{mean,j}$ (Eq. 2) and third the amplitude variability $\Delta A_{mean,j}$ (Eq. 3) of each pixel j through a stack of N pre-event SAR images.*”

in the revised manuscript

Figure 2: Please fix the label of the panel (c) and please also indicate the label (c). Remove label (b) from panel (a).

Sorry for the oversight, we will fix the labels on this figure. In the revised version of the manuscript

What do you mean by “vegetation removed”? Do you mean because of landsliding? If it is the case,

no need to indicate this.
We will remove this label from the figure.

Is the blue bar not centered for some reason? Did you do this on purpose? Or is this something you need to fix? And please indicate the corresponding panels while referring to "blue bars".
The blue bars represent the duration of the rainfall event, but the time series is centred on the peak in the rainfall, which is not necessarily halfway through the duration of the rainfall event.

We will add indications to the corresponding panels when describing the blue bars in the figure caption
Line 160: "this"?
We will change "*this landslide*" to *"each landslide"*
Line 164: "When combining methods, we found" This is still your method section and you haven't said anything about other methods yet. This is to say that I do not understand what you are referring to?
The combining methods is done in section 3.1, so this sentence refers to that. However, we agree it does not belong here and will move to Section 3.1
Lines 160-166: Based on what you explained here how we should interpret Figure 2c?

"A step change in the difference between the median landslide amplitude and the median background amplitude is then used as an indicator of landslide timing." Based on your interpretation, could you point out the timing of the landslide in Figure 2c. Which one is a step increase or a step decrease? Other than the signal received from the shadow area, I do not see any significant change in overall fluctuations of amplitude values associated with rainfall events (indicated by the blue bar in fig2c).

The step change is observed in the difference between the landslide and background time series. It is true that it is hard to see when considering the median landslide amplitude on its own. In Figure 2c, you can see that the distance between the orange and teal lines increases after the rainfall event (when the landslide happened). However, it is true that is not the clearest way to display this. We will redesign this figure incorporating the plots below, which show the time series for each method.

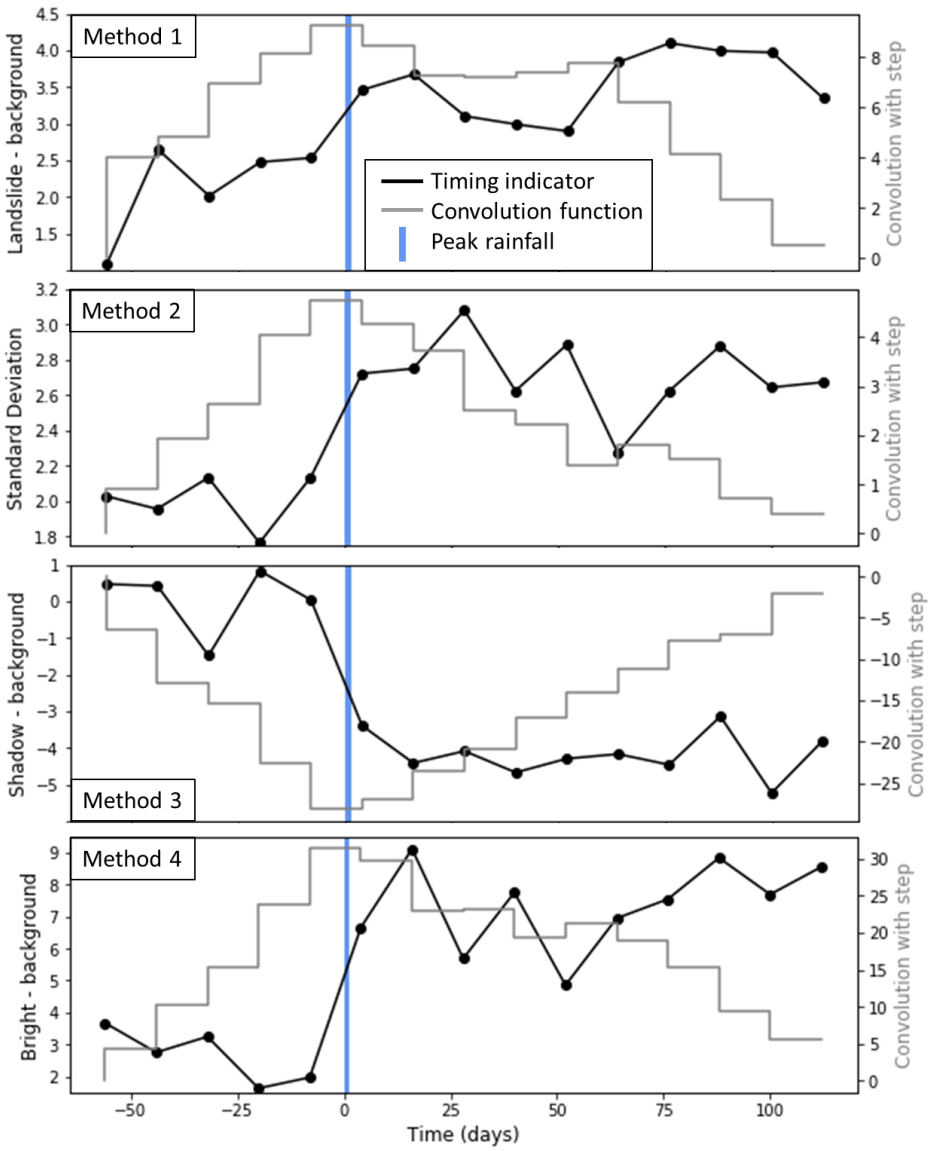


Figure 1 Example time series for each method described in Section 2.4 for a single landslide from the Hiroshima data set using SAR data from Sentinel-1 track 019D. The blue bar shows the duration of the peak rainfall associated with this event (6-7 July 2018).

The time series above are for a different landslide to the example shown in the original manuscript The landslide selected in the previous manuscript version demonstrated Methods 1 and 3 well, but for Method 2, the time step change was not clearly visible. This was raised in the next comment from the reviewer:

Lines 168-171: The same comment as above, please explain how you interpret Figure 2d. I do not see a specific change in the trend associated with the blue bar other than some fluctuations.

Therefore, we have decided to change to a different landslide polygon for which this trend is clearer. (We also include here the time series for Method 4, which was not included in the previous version of the manuscript)

We also add the convolution functions (in grey on the new figure), which demonstrate how the peak / trough in the function corresponds to a step change in the landslide timing detection method. This peak / trough is how we select the step change.

Lines 185-188: I can not see any connections between these two lines. Could you be more clear about what you mentioned about uncertainty in landslide mapping in the first line?

We will add a connecting sentence *"Small spatial mismatches between landslide polygon locations and SAR pixel locations could lead to pixels on the edge of landslides being excluded."*

Line 196: "Step change identification" As usual, please help the reader to follow you. You have just mentioned three methods to identify the timing of landslides. And I guess you are going to combine these three methods to get the best result out of all, right? This is also not clear and needs to be indicated. And here you keep going with another step of your methodology. I think it would be great if you make a flow chart explaining your methodology. You can briefly describe each and every step of your method at the beginning and then we would have an idea about what is going to be in the next step. I know what I am suggesting is a super smart thing, is quite a traditional way of presenting your method but it is also a good way of doing this.

Section 2.5 will be rewritten in response to this comment, the comment at line 198 and comments made by reviewer 3 in order to make the methods section easier to understand.

Line 197: "Sects. 2.4.2, 2.4.1 and 2.4.3" just say above
This will be changed to "above" in the next version of the manuscript.

Line 198: "The step function was made up of a series of -1s and 1s of twice the length of the co-event time series" Do twice the length of the co-event time-series means 12 months? And why?

This does not mean 12 months, it merely means twice the number of points in the SAR times series. (e.g. for a series of 12 SAR images, we convolve with a series made up of 12 -1s followed by 12 1s i.e. a series of length 24). The output of this convolution is a series having the same length as our SAR time series with a peak at the location that best agrees with the step function. In this way, we can automatically detect the location of a step change in the SAR measurements.

We are rewriting this section in response to this comment, the comment at Line 196 as well as comments made by reviewer 3 in order to make this section easier to understand.

Can't you make a figure to explain what you have done at this step?

We hope that by adding the convolution functions to the panels we will add to Figure 2 (see response to comments at line 160-166 , it will be clearer how this process works and how the peak / trough in the convolution corresponds to the step increase / decrease in the landslide timing method.

Lines 216-217: "the correct date by chance for a method with no skill" is not clear!

We will change "*the correct date by chance for a method with no skill*" in the original manuscript to "*the correct date randomly by a method with no skill (i.e. assigning a random date pair)"* in the revised mansucript

Table 2: What do those percentages stand for? For instance, in Hiroshima (H083A), you have 540 landslides and based on Pixel Variability you correctly identified the occurrence dates of 181 landslides, right? This means you correctly identified 33% of them. Then where did 59% come from? Obviously, the percentages indicate something else but I did not get what it is. I am sorry maybe it is my fault that I could not get it but this is not clear for sure.

This has been misunderstood. In fact, we have 540 landslides in Hiroshima, 181 of these landslides are assigned a timing by the Pixel variability method and 59% of these 181 are correct (i.e. 107 correct).

In order to make this less confusing, we will remove the percentages and instead provide correct / assigned in each column of the table. The table caption will also be updated to reflect this and to make this less confusing.

Actually based on these numbers and what you present in Figure 5, you can make an estimation for only a small fraction of the examined landslide population, right?

Yes this is correct.

You mentioned about confusion matrix, then why don't you present your results based on that structure?

The confusion matrix in Table 1 of the original manuscript was designed to assess how the size of the peak in the convolution function can be used to predict whether or not an assigned date is correct and to allow the calculation of the F1-score.

After the thresholds for each method have been selected and we move to combining predictions from multiple methods and tracks, there is no way to divide the landslides into TP, FP, FN, TN because we have three categories: landslide assigned the correct timing, landslide assigned the incorrect timing and landslide not assigned any timing.

Lines 243-245: "Out of all the non-masked landslides in each inventory, 23% were assigned a date in Hiroshima, 21% in Zimbabwe and 14% in Trishuli and of these, 80% of the estimated dates in Hiroshima were correct, 73% in Zimbabwe and 81% in Trishuli (Table 2)." So as you also indicated in the abstract, these are the percentages of correctly predicted landslides:

Hiroshima ~18%

Zimbabwe~15%

Trishuli~11%

Then how about the rest? Then I do not think what you mentioned in your abstract is convincing:

"This will allow multi-temporal landslide inventories to be generated for long rainfall events such as the Indian summer monsoon, which triggers large numbers of landslides every year and has until now been limited to annual-scale analysis."

Landslides could occur on different dates over a monsoon season in a given area of interest. And we would miss a great majority of them if we use this technique. Therefore, I do not think we can confidently argue that multi-temporal inventories can be generated based on this method (This method does not mean to generate multi-temporal inventories anyway). I am not saying this method is useless but is also clear that this could be just a small step towards what you are arguing in your abstract.

Yes we have changed the abstract and conclusions to better reflect this and have also slightly improved these success rates by altering the methods. See response to this comment at the beginning of this document.

Line 250: "Factors affecting performance of each method" This does not sound like your results. You should move this section to the Discussion section.

Yes this can be moved to the discussion section in the revised manuscript.

Line 383: " Application to future events" Please merge this section with the conclusion section, no need to have this heading, the paper is already too long.

We prefer to keep this section separate from the conclusions, since the removal of one heading will not really change the length of the paper.

Line 384: As you said you are just estimating the time window that landslide might have occurred. So you are not estimating the exact occurrence date of landslides. You should clarify this also in your title.

Yes. In the next version of the manuscript, all references to landslides being “dated” or “assigned a date” (which implies a precision of one day) will be changed to assigned a timing /date pair/ time window or similar

In line 384 of the original manuscript (and elsewhere), we will change “*dated*” to “*timed*”

We will give the revised manuscript a new title to better reflect the results in the manuscript e.g. “*Using Sentinel-1 radar amplitude time series to constrain the timing of individual landslides: a step towards understanding the controls on monsoon-triggered landsliding”*

Line 406: “generate multi-temporal” You are not generating landslide inventories. You are just trying to label existing landslide inventory in terms of their time of occurrences.

In the conclusions section, we will change “*These methods will allow us to generate multi-temporal landslide inventories for long rainfall events, unlocking comparisons between rainfall data, hydrological models and triggered landsliding*.” to “*These methods will provide information on the timings of some individual landslides and allow spatio-temporal clusters of landslides to be associated with peaks in rainfall during long rainfall events, unlocking comparisons between rainfall data, hydrological models and triggered landsliding.”*

Lines 414-415: “Google Earth Engine and Python codes used in generating the time series and detecting landslide timings will be provided if the manuscript is accepted for publication” The authors should share the code so we can check how it works really.

The code will be provided as a supplement to the next version of the manuscript as requested. I apologise for omitting it, I was not aware it was necessary at this stage.