We thank the reviewer for taking the time to read and evaluate our manuscript. We will prepare a line by line response, but here we respond to some of the major comments made on the manuscript (Quoted in bold in this document).

First in the review introduction, two major points are raised (In bold), that are also referred to later in the review

"**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% 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.

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 characteristic which have preceded the triggering of landslides (even with a time window is 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.

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 on the text and the figures, and hope these will make the manuscript 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.

Then, later in the review, we identify the following major points to be addressed (Omitting smaller corrections to the text and figures, which we will address in a later, comprehensive response).

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 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 "*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 "*<u>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</u>.*"

For landslides triggered by the mainshock, their timing is known, which should resolve the confusion here. We detail below why we test our methods not only on well-timed rainfall triggers (Hiroshima and Zimbabwe) but also on the Nepal earthquake.

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.

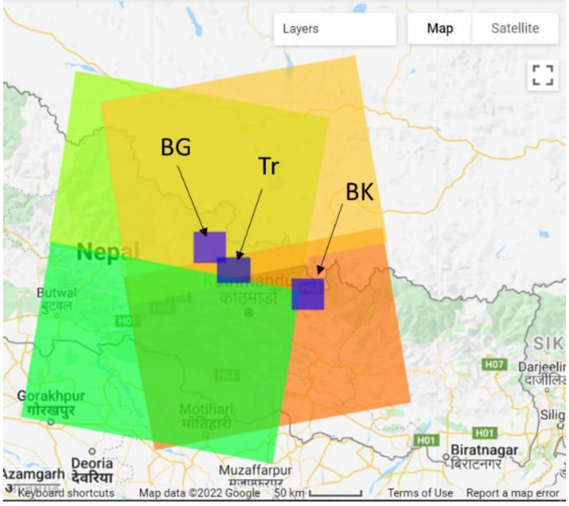
It is also particularly challenging for SAR applications due to its steep topography, which results in distortion of the SAR images. It is thus particularly important to test the methods in this environment. Unfortunately, rainfall-triggered landslide inventories of known timing are not available here, so we use an inventory of earthquake-triggered landslides instead.

This is stated in lines 78-81 of the manuscript and will be emphasized more in the revisions.

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 and Buri Gandaki, 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 (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.

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. Possibly "true co-event pair" and "designated co-event series".

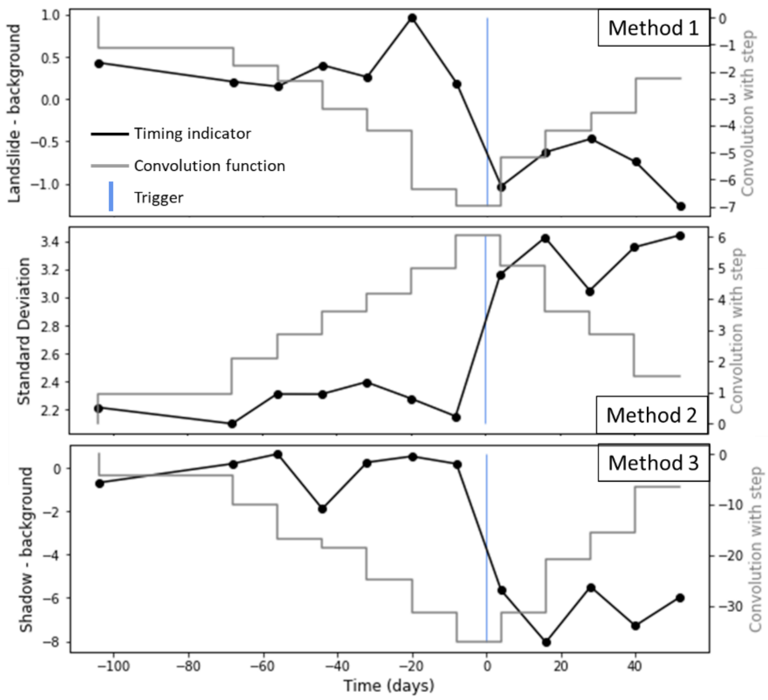
Line 92: "these two earthquakes can be considered as a single triggering event in Bhote Kosi" You cannot consider them as a single event. However, if you cannot differentiate landslides possibly triggered by different factors for a given period of time, this needs to be indicated as a source of uncertainty 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

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 shows the time series for each method.



The time series above are for a different landslide to the example shown in the original manuscript (One from the descending track 19 SAR time series over Trishuli, Nepal). 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 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.

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 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.
Can't you make a figure to explain what you have done at this step?
We hope that by adding the convolution functions in the above figure, 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.