Referee comments on "Earthquake-induced landslides in Norway"

Referee nr. 1 (Mihai Micu):

We thank the reviewer for the thoughtful comments and suggestions to our manuscript. We agree with the main points made by the reviewer and have revised our manuscript according to the suggestions. Some of the revisions proposed are difficult to implement due to the intraplate location of our study area, as we describe below. We include our responses to the individual comments in the following (reviewer comments in *grey italics*, our response in black).

Cover letter:
The manuscript I've reviewed is very well written, in a clear and concise manner. Some comments have been added in the attached document.
My only doubt is that the entire judgment is based on a very reduced number of landslides, quite scattered (the authors mentioned this as a potential shortcoming).

We acknowledge that the total number of landslides we find in our study is low, but still find the results important and worth publishing, especially since this is the first systematic study of EQIL in Norway and because we thereby more than double the number of EQIL in intraplate regions with information available in the literature.

Comments in manuscript:
Page 1: A series of pictures with the (described or similar) landslides would be very helpful in order to understand the morphology and the magnitude of the slope processes.

Unfortunately (to our knowledge) there are no photos of Norwegian EQIL. However, we have included photos of a rockfall and a landslide in clayey soil (new Figure 2) to clarify the processes as suggested by the reviewer.

Page 3 (L70): Maybe a map/sketch with the seismo-tectonic context would make the localization of this intraplate domain more easily understandable.
We have added the mid-Atlantic plate boundary and major faults to Figure 1 to make the seismotectonic context clearer to the reader.

Page 3 (L72): Hypocentral depth should be mentioned. As well, some brief information about preferential seismic wave propagation, faults/ruptural surfaces distribution, structural or lithological amplification/attenuation should be given.
Due to the limited data available, it is not possible to determine the depth of the 1819 earthquake. The same is the case for identification of fault plane, wave propagation etc. The event is described in more detail in section 3.1 and with that in mind we find the description in the introduction is sufficient. However, we have added a comment on the general depths of Norwegian earthquakes as this information was missing. We also add a brief discussion of event depths in the discussion section.

Page 3 (L78): The names mentioned below should be placed on the map, which is otherwise "blind". This refers also to the rest of the maps below, which should be linked with the associated names.

We added numbers to Figure 1 marking the most seismically active areas. We also added placenames to the EQIL locations in Figs 5 and 9.

Page 3 (L89): How many?
We have specified that the present land uplift in Norway is up to 4 mm/yr, increasing from West to East.

Page 3 (L90): is…
We are not sure what the reviewer means with this comment, to us the sentence seems grammatically correct.

Page 3 (L90): As a triggering factor, but what about a preparing factor?
The study of Olesen et al. (2013) discusses the contemporary stress field in Norway, and the authors argue that postglacial rebound plays a minor role in generating crustal stresses and thus earthquake activity today.

Page 4 (Fig 1): …or maybe complete this figure with the tectonic framework.
We have added the mid-Atlantic plate boundary and major faults to figure 1.

Page 5 (L106): Citation needed.
A reference to Hermanns et al. (2012) has been added.

Page 5 (L111): s
We do not think it would be grammatically correct to add an "s" to construction work, so we decided to leave the text as it is.

Page 5 (L122): Is this value in agreement with a particular reasoning?
We answer that question in the following section "Data": "*Whereas smaller events may trigger landslides as well, we select the lower magnitude threshold of M=4.5 in order to have a systematic overview of the landslide-triggering-potential of earthquakes nationally."*

Page 5 (L130): From this point of view, you should explain if you considered only co-seismic or also port-seismic events and what was the reasoning behind this.
We consider mostly co-seismic events, but also include post-seismic events in cases where the time delay does not exceed a few days, and when the link between earthquake and landslide is convincing. These considerations are described in more detail under the descriptions of the individual events.

Page 6 (L159): Apparently, there is one occurred after 6 days?
It is true that we mention that landslides continue beyond the 5-day period near Bullaren following the 1904 earthquake. However, this is a series of landslides that started within the 5-day period. We still find it important to mention that this landslide activity continued beyond the first 5 days after the earthquake.

Page 6 (L159): The reasoning behind the values (5, 250) should be explained.
The time and distance limits are selected as plausible maxima based on previous observations and considering that landslides occurring outside those limits would be difficult to directly associate with the considered earthquakes. A sentence on the reasoning behind the values has been added.

Page 6 (L159): Would such words as "seismic + shake" make any difference?
Including those terms does not lead to identification of additional events.

Page 7 (L165): How was the antecedent rain period defined?
We have revised Figure 6 (Figure 5 in the initial submission) and the discussion of the role of antecedent rainfall. See also our response to the related comment further down.

Page 8 (Fig 3): As in "earth slides"?
The correct terminology is “landslide in clayey soil”. The terminology has been updated throughout the paper.

Page 9 (Table 1): A correspondent with a type of displaced material (earth/soil/debris/rock) would make the comparison easier. Otherwise, an explanation of "clay slide" as a certain typology is needed
Please see the response above. We now use the more correct term “landslide in clayey soil”.

Page 9 (Table 1): For a better understanding, it would be advisable to express all the magnitudes in only one scale (Mw, for example).
In principle, we agree with the reviewer that it would be better to use only one consistent magnitude scale. However, because the considered events are mostly historical and Norway is a region of moderate seismicity, we do not have consistent magnitude assignments for the events. We do not want to add further uncertainty by applying magnitude conversions and therefore present the most reliable magnitude estimate for each event.

Page 13 (L227): Meters would be advisable.
A comment has been added that this corresponds to about 6 m.

Page 13 (L235): Would you consider less then one month of antecedent precipitation as relevant? Is this based on some previous observations? If not, the reasoning behind not enlarging the analysis sequence would be needed. Same, for the others.
In the revised manuscript, we consistently display the precipitation for the 30-day period prior to each earthquake. We have also revised the text such that the difference between antecedent precipitation and the direct triggering of landslides by precipitation should be more clear.

Page 13 (L250): The surface would make more sense…
The text has been revised to “Western shore”.

Page 19 (Fig 8): Is there any PGA map which can be used for comparison purposes?
The event occurred before a seismic network was developed in Norway, so there is no PGA map for the event.

Referee nr. 2 (Reginald Hermanns):

We thank the reviewer for his thoughtful and constructive comments and suggestions to our manuscript. We agree with the points made by the reviewer and have revised our manuscript according to the suggestions. We include our responses to the individual comments in the following (reviewer comments in *grey italics*, our response in black).

The contribution: Earthquake-induced landslides in Norway by Mathilde B. Sørensen, Torbjørn Haga, Atle Nesje is an important contribution for studying earthquake-induced landslides (EQIL) on intraplate margins. It takes benefit of 41 years of instrumental and 200 years of historical earthquake records and one of the best national landslide data sets in Europe. The study is justified because of historic earthquakes triggering wide-spread landsliding in Norway. The study is in depth and no other data with high certainty could be added. I see a bit of deficiency including results of more recent studies in this field of geosciences in Norway and a deficiency in the full free available data sets.

Especially the susceptibility of the slope to landslide processes is discussed. In Norway landslide susceptibility maps exists covering the entire country: see "NVE Atlas" for rock fall, snow avalanche and debris flow/floods/ avalanches and more detailed susceptibility maps for quick clay slides at: Mulighet for marin leire (MML) | Norges geologiske undersøkelse (ngu.no). Locations of historic landslides should be compared to those maps. This would indicate if the Norwegian landslide hazard mapping program takes sufficient care of EQIL or if additional products are required. If so, this investigation would get a much higher importance to the Norwegian society but also to landslide sciences.

This is a very important point, and we agree with the reviewer. We discuss now, for each identified EQIL, the landslide susceptibility maps in the area of the event. We also include a description of the available susceptibility maps in the data section and summarize our observations in the discussion section.

In the following I give my comments following the structure of the manuscript:

Line 23: It is important to mention that the term landslides translate to "skred" in Norway. The term "skred" contains different to all other languages snow avalanches. Thus do the total numbers in data bases refer to landslides and snow avalanches. This is discussed in the paper:

Herrera, G., Mateos, R. M., García-Davalillo, J. C., Grandjean, G., Poyiadji, E., Maftei, R., Filipciuc, T.-C., AufliÄ, M. J., JeÅ¾, J., and Podolszki, L., 2017, Landslide databases in the Geological Surveys of Europe: Landslides, p. 1-21.

and also shown in:

Kalsnes, B., Nadim, F., Hermanns, R., Hygen, H., Petkovic, G., Dolva, B., Berg, H., and Høgvold, D., 2017, Landslide risk management in Norway, Slope safety preparedness for impact of climate change, CRC Press, p. 215-251.

We have clarified this under the description of the Norwegian landslide database.

The most updated numbers published on life loss in Norway are published in:

Hermanns, R. L., Hansen, L., Sletten, K., Böhme, M., Bunkholt, H. S. S., Dehls, J. F., Eilertsen, R. S., Fischer, L., L'Heureux, J. S., Høgaas, F., Nordahl, B., Oppikofer, T., Rubensdotter, L., Solberg, I. L., Stalsberg, K., and Yugsi Molina, F. X., Systematic geological mapping for landslide understanding in the Norwegian context, in Proceedings Landslides and Engineered Slopes. Protecting Society through Improved Understanding: Proceedings of the 11th International & 2nd North American Symposium on Landslides, Banff, Canada, 3-8 June 2012, CRC Press, p. 265-271.

The text has been updated with the numbers from Hermanns et al. (2012).

Lineas 70 – 90: It might be interesting to reference in addition to the paper by Blikra et al. 2006 also more recent papers that use landslides from the geological record to reconstruct paleoseismic events in Norway:

Bellwald, B., Hjelstuen, B., Sejrup, H., Stokowy, T., and Kuvås, J., 2019, Holocene mass movements in west and mid-Norwegian fjords and lakes: Marine Geology, v. 407, p. 192-212.

Mangerud, J., Birks, H. H., Halvorsen, L. S., Hughes, A. L., Nashoug, O., Nystuen, J. P., Paus, A., Sørensen, R., and Svendsen, J.-I., 2018, The timing of deglaciation and the sequence of pioneer vegetation at Ringsaker, eastern Norway–and an earthquake-triggered landslide: Norsk Geologisk Tidsskrift, v. 98, no. 3, p. 301-318.

In this paper it was postulated that the Tjellefonna 1756 rock avalanche was seismically triggered:

Redfield, T. F., and Osmundsen, P. T., 2009, The Tjellefonna fault system of western Norway; linking late-Caledonian extension, post-Caledonian normal faulting, and Tertiary rock column uplift with the landslide-generated tsunami event of 1756, in Osmundsen, P. T., ed., Volume 474: Netherlands, Elsevier : Amsterdam, Netherlands, p. 106-123.

Which could not be proven as certain by rock stability calculations in:

Sandøy, G., Oppikofer, T., and Nilsen, B., 2017, Why did the 1756 Tjellefonna rockslide occur? A back-analysis of the largest historic rockslide in Norway: Geomorphology, v. 289, p. 78-95.

We have expanded the discussion of paleoseismic events to include also the more recent references.

Line 104:
This overview was given by:

Furseth, A., 2006, Skredulykker i Norge, Oslo, Norway, Tun Forlag, v. Book, Whole, 207 p.

and

Hermanns, R. L., Hansen, L., Sletten, K., Böhme, M., Bunkholt, H. S. S., Dehls, J. F., Eilertsen, R. S., Fischer, L., L'Heureux, J. S., Høgaas, F., Nordahl, B., Oppikofer, T., Rubensdotter, L., Solberg, I. L., Stalsberg, K., and Yugsi Molina, F. X., Systematic geological mapping for landslide understanding in the Norwegian context, in Proceedings Landslides and Engineered Slopes. Protecting Society through Improved Understanding: Proceedings of the 11th International & 2nd North American Symposium on Landslides, Banff, Canada, 3-8 June 20122012 2012, CRC Press, p. 265-271.

The paper by Harbitz referenced here is not a summary of events but the modelling of a displacement wave following potential failure of Åkneset mountain.

Good point – we use a reference to Hermanns et al. (2012) in the revised paper.

Line 110 reference missing.

We will include a reference to Hermanns et al. (2012) for this statement.

Line 116: The "Berill fault" is not classified anymore as a neotectonic fault but a Caledonian fault that was gravitationally reactivated:

Schleier, M., Hermanns, R. L., Krieger, I., Oppikofer, T., Eiken, T., Rønning, J. S., and Rohn, J., 2016, Gravitational reactivation of a pre-existing post-Caledonian fault system: the deep-seated gravitational slope deformation at Middagstinden, western Norway: Norwegian Journal of Geology, v. 96, p. 1-24.

See also newest neotectonic map of Norway, this does not include the Berill fault any longer:

Keiding, M., Dehls, J., and Olesen, O., 2018, Neotectonic map of Norway and adjacent areas scale 1: 3 000 000.

We have removed the sentence mentioning the Berill fault, and rather rely on the descriptions of paleo-events in the section "Seismicity of Norway".

Line 156: Out of the 80.000 events in the inventory a large amount are snow avalanches not considered as landslides in other languages than Norwegian, see discussion of the data in:

Herrera, G., Mateos, R. M., García-Davalillo, J. C., Grandjean, G., Poyiadji, E., Maftei, R., Filipciuc, T.-C., AufliÄ□, M. J., JeÅ¾, J., and Podolszki, L., 2017, Landslide databases in the Geological Surveys of Europe: Landslides, p. 1-21.

We have included a comment on this at the end of the paragraph describing the Norwegian Landslide Database.

Line 360: there are landslide susceptible slopes south of the epicentre see susceptibility maps on: NVE Atlas

It is true that there are landslide susceptible slopes south of the epicenter. What we actually mean is that most of the area within a plausible distance limit and south of the epicenter is covered by water, which makes landslide observations less likely. We have rephrased the text.

Figure 8: I have the impression that the colour coding of the map and figure caption does not match. The yellow star is difficult to see.

We have corrected the caption and made the star better visible.

Line 378: Landslide susceptibility of slopes are mentioned and discussed again and again without making use of Norwegians open-source landslide susceptibility map covering the entire country. This should be rewritten, or the free data should be used.

Comparisons to the available susceptibility maps have been included in the revised manuscript.

Line394: What is a "deep seated rock avalanche"? Please look for the classification of rock avalanche in Hermanns et al. 2021 and older definitions mentioned therein. There are "deep seated gravitational slope deformations" and "rock avalanches" but this new term should be defined when proposed:

Hermanns, R. L., Penna, I. M., Oppikofer, T., Noël, F., and Velardi, G., 2022, 5.06 - Rock Avalanche, in Shroder, J. F., ed., Treatise on Geomorphology (Second Edition): Oxford, Academic Press, p. 85-105.

This is indeed not correct terminology. What we mean is "rock avalanche", this has been corrected in the manuscript.

Line 396. In Norway most of the landslide-prone areas are not remote. 2.8 Mio inhabitants live on ground that falls in the susceptibility zones of quick clay slides, this is half of the population. Landslides can occur in nearly all urban environments.

This is a good point, and we agree the sentence is not well formulated. What we actually mean is that many landslides occur in remote areas where small rockfalls and landslides go unnoticed. The text has been corrected accordingly.

Line 416: I think there is a bit of dispute about that earthquakes might trigger rock avalanches in Norway and on latest conferences opposing opinions were presented. There is no > M6 earthquake in the seismic and historic records. Keefer et al. 1984 and Rodriguez et al.1999 indicate M 6 as a minimum magnitude to trigger rock avalanches. This conclusion would require a discussion if M6 earthquakes are not possible or if there is a deficiency of such events in the historic and instrumental records. Newest research on paleoseismicity using landslide distribution suggest different: see Bellwald et al., 2019; Mangerud et al., 2018.

This is a very interesting discussion. In our view, there is no reason why we should not experience M>6 earthquakes in Norway, and there are indeed an increasing number of indications that such events have occurred in the past. We have included a discussion of this issue in the revised manuscript and revised the related sentence in the conclusion.

Reviewer nr. 3: Anonymous

We thank the reviewer for the thoughtful comments and suggestions to our manuscript. We agree with the main points made by the reviewer and have revised the manuscript according to the suggestions. Some of the revisions proposed are difficult to implement due to the lack of a ground motion prediction model derived from Norwegian data, as we describe below. We include our responses to the individual comments in the following (reviewer comments in *grey italics*, our response in black).

I have read with great interest the manuscript prepared by Sorensen et al. about EQIL in Norway. I find the paper well written and easy to read. Figures and Tables are all relevant and needed for a complete understanding of the data and results presented.

From a scientific point of view, this paper is relevant because it clearly demonstrates the differences found in data (maximum distances, area affected) coming from stable, intraplate areas with respect to those more commonly available of (seismotectonic) active areas. In this sense, although uncertainties in some data presented are important (in most cases, authors cannot give a precise location of landslides), they *are still relevant for demonstrating the effect of low attenuation patterns in these areas. To this respect, maximum distances found are high, sometimes extremely high, when compared with data published by other authors, but not so different from data of similar geological contexts.*

I think that this manuscript may be enriched if authors could provide more data about characteristics of ground motion attenuation in their study zone (Norway and surrounding areas). I do not ask for a study of ground motion attenuation but for a comparison of already available attenuation laws (ground motion prediction equations, GMPE) for Norway with respect to that found for other areas (for instance, Mediterranean areas). This may help understanding how severe may be ground motion when triggering the rock falls mentioned in the manuscript.

It is very difficult to estimate the ground shaking level associated with the EQIL as there is no reliable GMPE based on Norwegian data. We have included a bit more information on Norwegian ground motion attenuation in the revised manuscript (see details below), but a new GMPE for Norway would be required to fully address this comment.

In relation with this last comment, I find through the paper that authors make no attempt to estimate how severe ground motion was in any example. Given the GMPE currently in use in Norway, what is the PGA or PGV expected for such events at the range of distances found for EQIL? Values may be surprising when compared with those reported in recent studies. For recent events, probably, instrumental data are available.

The main reasons we do not discuss ground motions for the EQIL are that 1. (as stated above) there is no reliable GMPE based on Norwegian data and 2. the EQIL all occurred either before instrumental monitoring was established in Norway or when the seismic network consisted of only few stations. Estimating ground motions would thus require significant extrapolation. We now specifically mention the low level of attenuation in Norway and also include a section in the discussion about why we do not estimate the ground shaking levels. However, we find that any attempt to estimate ground motions at specific locations for specific events will be too speculative to be of practical use.

Something similar occur when describing the size of landslides reported. Given that instabilities reported were triggered by low magnitude events (M < 6.0) and occurred at very large distances, it is expected that size is small but how small? < 1 m3? < 100 m3?

As our landslide observations are mostly based on eyewitness accounts, information on their sizes is qualitative and often imprecise. In most cases, the only statements we have are of the type "a large rockfall", "a large block" or "a landslide". It is clear from the information that the sizes vary, and we include details provided from the observers in the event descriptions when available. The uncertainty related to landslide size is also discussed in the discussion section.

Finally, given the uncertainties that affect the whole EQIL dataset, I suggest removing all no really confident data.

We assume that the reviewer refers to the 1958-event here. After long consideration (and contrary to our original response), we have decided to keep that event in our dataset. Whereas the triggering relation is uncertain, we still feel the spatial and temporal proximity of earthquake and landslide provide strong enough indications that the events are connected to include them in the list. We have revised the text somewhat, such that this is no longer presented as a special, uncertain case.

Other minor comments:

Line 164 (Abstract): Limiting rain period search to 24 hr (only) may underestimate the potential state of slopes. Please consider longer time periods.

We consider 24-hour-averaged precipitation data but do consider longer time periods than 24 hours in Figure 6 (Figure 5 in the initial submission). This has been clarified in the manuscript. Furthermore, we have revised the figure such that precipitation during the 30-day period before each earthquake is shown.

Appendix A: It has no interest and I suggest removing it. Any interested researcher may find these data in the EQ catalogue web page (line 428).

It is true that data can be accessed through the online catalog, but since this database is a dynamic product that may be updated with time (even for historical events) we prefer to give the list to the reader for reproducibility. This also allows us to state what magnitude type is considered for each event (ref. comment from reviewer 1). We could consider providing it as an electronic supplement instead if the editors find that more appropriate.

Reviewer nr. 4: Marta-Cristina Jurchescu

We thank the reviewer for her thoughtful and constructive review. We agree with the points made by the reviewer and have revised our manuscript according to the suggestions. We include our responses to the individual comments in the following (reviewer comments in *grey italics*, our response in black).

"General comments"

The submitted paper presents the systematic work conducted and the criteria followed with the aim of producing a new dataset of earthquake-induced landslides occurred over the last two centuries in Norway (Norwegian EQIL), while contributing to the understanding of some characteristics of earthquake-induced landslides in intraplate tectonic settings/conditions.

Large databases concerning earthquakes and landslides (e.g. NNSN, UiB, SEA, NLD) are cross-checked to this end, and the criteria for attributing a trigger-response connection between earthquake and landslides are well detailed (e.g. locations in time and space allowing to establish temporal and spatial coincidences between an earthquake and associated landslides).

The paper presents a clear in-depth analysis of the listed events, discriminating between earthquake-induced landslides with a lower degree of uncertainty, and those associated to a higher degree of uncertainty caused by the inability to locate the landslide or the earthquake or by an insufficient documentation of the failures' link to a seismic trigger. Hence, it is appreciated that the inclusion of records into the final dataset is presented in a clear and transparent manner.

Although the output slope failure dataset is reduced in number (containing merely 22 events), which is recognized by the authors as a shortcoming of their study, its value, and hence that of the study, *resides in the pioneering effort put into designing and following a systematic approach for producing a first dataset of seismically induced landslides for an intraplate region. Such an initial database could form the base for a future much developed one, which could be updated through remote sensing, as the authors mention. The study also contributes to supporting the idea of potentially much larger maximum landslide distance limits and landslide-affected areas than previously estimated by global studies, but in accordance with findings from other intraplate regions.*

The manuscript is well structured and written and illustrations and tables are all necessary. The conclusions are concise and comprise the most important findings related to the significance of the constructed EQIL catalogue. Overall, it was a pleasure reading this submission, and, for the reasons listed above, the paper is valuable and worth publishing with only some minor revisions which are suggested below.

"Specific comments"

1. On the Figure displaying earthquake of M≥2 in the region (Section 1.2 Seismicity of Norway, Fig. 1), I would recommend the inclusion of some tectonic features which would enable a better understanding of the general seismic and tectonic settings of Norway, defining the region as an intraplate one.

We have added the mid-Atlantic spreading ridge and major faults to Figure 1.

2. For Section 1.3 Landslides in Norway and their trigger mechanisms, a figure with photos of representative landslide types in Norway would be very helpful. In such a figure, of interest would be to also find at least one photo of a known seismically triggered landslide. If not here, then at least later on in the paper, (a) photo(s) of recent recorded EQIL would help the reader understand the types of movement triggered by earthquakes.

Unfortunately (to our knowledge) there are no photos of Norwegian EQIL. However, we have included photos of a rockfall and a landslide in clayey soil (new Fig. 2) to exemplify the events as suggested by the reviewer.

3. From what I understand, Table 1 (section 3 Results) lists the EQIL dataset constructed in this study. For more clarity, maybe you could add "Norwegian EQIL" in brackets in the table caption. Also, for more clarity, a column listing the "No" would help seeing that this table refers to the 22 EQIL. Further, in this context, I find the explanation " indicates an uncertain event" a bit confusing. As far as I understand, this table doesn't contain the uncertain events, which were eliminated from the dataset, as was explained in Section 2 Methods (page 5, lines 129-134). Then, what is indicated with "*"? Does the uncertainty refer to the existence of the landslide? Or does it refer to the movement type attributed to it? This is not very clear and should be explained, in a table footnote or/and in the text.*

We have clarified in the table caption that these are the EQIL identified in this study. However, we prefer not to include event numbers as those would not be referred to later in the paper. The marking of the "uncertain event" has been removed.

4. Section 2 Methods, page 5, line 122: The search for seismically induced landslides is restricted to earthquakes of magnitudes M≥5. An explanation would be needed at this point as to why this magnitude threshold was selected when constructing the EQIL catalogue.

The magnitude limit considered is 4.5. We explain the choice of value in the following section "Data": "*Whereas smaller events may trigger landslides as well, we select the lower magnitude threshold of M=4.5 in order to have a systematic overview of the landslide-triggering-potential of earthquakes nationally."*

5. In Section 4 Discussion, when discussing the landslide distance limits and landslide-affected areas, I would suggest the following:

- page 17, line 348: please specify "limit curve" in: "the empirically derived limit curve of maximum landslide area…";

We do not use the term "limit curve" for landslide area, but rather write "the empirically derived curve of maximum landslide area". We have clarified in the text that this is "the empirically derived curve of maximum landslide area vs. distance".

- I would recommend using a softer wording for formulating some conclusions, like at page 18, line 349: since the number of observations is small, indeed, I would suggest rewording with the vaguer "seem to confirm the systematically larger distance…" instead of just "confirm the systematically larger distance….", this would be more truthful to the degree of uncertainty inherent in the data;

We agree but have rather chosen to rephrase to "are in agreement with the systematically larger distance…".

- page 18, lines 352-353, caption of Fig. 6: for clarity, I would find it necessary to list the areas' names and corresponding citations for the grey dots in the caption as well (not only in the text); I also would write the extended explanation for the black curve: "maximum landslide distance limit for disrupted slides and falls from Keefer (1984)";

The caption has been revised.

- page 18, lines 355-356, caption of Fig. 7: I would suggest adding the reference for the maximum landslide distance corresponding to the 2011 Virginia earthquake; I also would write the extended explanation for the black curve: "maximum landslide area limit from Rodriguez et al (1999)";

The caption has been revised.

- page 20, line 375: I would suggest putting more emphasis by replacing with: "…lead to differences in the identified/estimated landslide distance limits", since the differences do not concern the limit itself but rather its identification or estimation based on the available data;

The text has been revised.

6. The discussion of the relation between EQIL and ground motion intensity for the 1904 earthquake is very important; at this point it would be interesting to also include in the discussion a map displaying EQIL distributed in relation to the Peak Ground Acceleration, if available.

There are no nearby instrumental recordings available for the 1904 earthquake. The issue of ground motion values (e.g., PGA) is also raised by another reviewer. Two major challenges in discussing the levels of ground motion at the locations of the EQIL are that all events occurred before dense monitoring networks were established and that there is no reliable ground motion prediction equation (GMPE) available that is based on Norwegian data. We have decided not to estimate ground motions for the events because the derived values would be highly uncertain. We have, however, explained this lack of GMPE in the manuscript and also describe the ground motion attenuation in a bit more detail.

7. With regard to the role of precipitation (presented in Sections 3.3-3.8., pages 13-16, in Section 4 Discussion, page 20, lines 378-385, in Section 5 Conclusions, page 21, lines 412-414, and in the Abstract, lines 15-16), in my opinion, the triggering and the preparatory roles of precipitation are presented a bit confusingly. While in the Results section, precipitation is being analyzed in order to rule out a possible precipitation trigger for the events included in the EQIL dataset (i.e. from a trigger perspective), in the Discussion and Conclusions sections, precipitation is discussed more in the context of its possible contribution to increasing terrain proneness to landsliding (i.e. from a preparatory perspective, of antecedent precipitation leading to soil moisture conditions). While from a trigger perspective, it is common to analyze precipitation amounts up to 5 days before an event, for drawing conclusions regarding the antecedent precipitation conditions, it would be recommended that the period prior to the earthquake and, thus, to the earthquake-induced landslides be a little extended, e.g. commonly at least up to 30 days (e.g. Rosi et al, 2019). Therefore, I would suggest either extending the period prior to the events in order to be able to draw conclusions related to the antecedent role of precipitation potentially increasing terrain susceptibility to landslides, or being more precise in the Discussion and Conclusions sections about what could be found so far, namely that antecedent moisture conditions may have played a role in preparing the slopes to respond to seismic shaking but that the preparatory role of precipitation and its combination with the earthquake trigger was not investigated in this study. E.g. line 412-414: instead of writing "and for three of the earthquakes triggering EQIL, precipitation is expected to have increased the susceptibility of the affected slopes before the earthquake", you could write only what has been found/is suspected until now: "and for at least three of the earthquakes triggering EQIL, precipitation is expected to have increased the susceptibility of the affected slopes before the earthquake'.

We agree that the descriptions of the role of precipitation were not so clear. In the revised manuscript, we consistently display the precipitation for the 30-day period prior to each earthquake. We have also clarified the discussion in terms of the difference between antecedent precipitation and the direct triggering of landslides by precipitation.

8. When discussing that all landslide-triggering earthquakes in the constructed dataset are contained in the period April-October (in Section 4 Discussion, page 20, lines 379-381), for more clarity, it should be put into the context of the larger earthquake database which also includes earthquakes occurring in winter but for which no corresponding records of induced landslides were found (Appendix A); this would make the reasoning much clearer.

The text has been revised to describe that earthquakes are equally likely to occur at any time of the year.

"Technical corrections"
- Section 3, page 9, caption of Table 1: all the abbreviations in the table (ML, MS, MW) should be explained (either in the caption or as a table footnote);

Explanations of the abbreviations have been included in the caption.

- Section 3.1, page 10 line 183: please replace “from” with “of”;

Corrected.

- Section 3.1, page 10 line 193: please replace “from” with “of”;

Corrected.

- Section 3.1, page 10 line 196: please insert a comma after “In this study”;

Corrected.

- Section 3.1, page 10 line 200: please move the word “almost” after the word “being”;

Corrected.

- Section 3.1, page 11 line 201: please replace “identified for this earthquake” with “identified in connection to this earthquake”;

Corrected.

- Section 3.1, page 12, Table 2: although it is clear in the text, NLD should also be explained for the table (either in the caption or in a table footnote – depending on the journal’s guidelines). Please also replace the comma with a point in: “Referred to as Storstrand in NLD. NLD...”;

Corrected.

- Section 3.3., page 13, line 240: Please replace the singular with the plural form in: “The precipitation data (Fig. 5) show ...” (since „data” is a plural noun);

Corrected.

- Section 3.3., page 13, line 248: I think you mean “300-400 m” and not “3-400 m”, right?;

Corrected.

- Section 3.3., page 13, line 249: You mean “70 m2 of forest” and not “70 m”, correct?;

The report actually states that 70 m of forest is destroyed, but we agree that this is unclear. We have rephrased to “An approximately 70 m-long stretch of forest along the slope is reported to have been destroyed.”

- Please pay attention when writing the dates. If you choose the British style for dates, I think there shouldn’t be any “.” sign after the date (see lines 247, 254, etc.): e.g. “7 October”- not “7. October”;

It is correct that according to the journal guidelines, dates should be written without the ".". The manuscript has been revised to use a consistent format for dates.

- Section 3.4, page 14, Figure 5: please export this illustration with a better resolution, as the graphs appear a little blurry; please standardize the notation on the y-axis: either 24-hour or 24-h"; also, the word "precipitation" on the vertical axes of the graphs appears underlined/marked - for esthetic reasons this should be removed;

The figure has been revised according to the recommendations from the reviewer.

- Section 3.5, page 15, line 262-263: please change the sentence to: "had a magnitude ML=4.6 and a maximum intensity of V";

Corrected.

- Section 3.5, page 15, line 281: please replace "the" with "a": "We expect that this rockfall was triggered by a combination of …";

Corrected.

- Section 3.7, page 15, lines 284-285: Please change the phrase as follows: "….with a magnitude of MW=4.9. The event was felt throughout the Nordland region with a maximum intensity of V";

Corrected.

- Section 3.7, page 16, lines 292-293: the sentence needs reworded as follows: "This supports the interpretation/hypothesis/conclusion of the earthquake being the main trigger…";

The sentence has been rephrased: "This supports the hypothesis of the earthquake being the main trigger of the landslides."

- Section 3.8, page 16, lines 295: please delete "it": "…and was strongly felt in…"

Corrected.

- Section 3.8, page 16, line 296: please change to: "with a maximum intensity of V";

Corrected.

- Section 3.8, page 16, line 298: I think "the" would need to be changed to "a", as follows: "where a respondent describes ….", right?;

Each questionnaire in the archive has been filled out by one respondent, and we therefore think it is correct to refer to "the respondent" in this case.

- Section 4, page 19, line 359: please replace with the plural form: "….at similar or higher latitudes";

Corrected.

- Section 4, page 19, line 360: I would recommend replacing "may" with "would", as follows: "the landslide area would have been larger if…";

Following a comment from another reviewer, we have rephrased the sentence and rather focus on the fact that most of the area south of the epicenter and within a plausible distance limit is covered by water and thus landslide observations are less likely.

- Section 4, page 19, line 367 (caption of Fig. 8): I think you mean "Blue squares", not "Grey squares";

Corrected.

- Section 4, page 20, line 374: please insert "the" in: "…also suggest that differences in the levels of investigation…";

Corrected.

- Section 4, page 20, line 377: please remove "it", as follows: "…and is thus directly comparable to the global studies";

Corrected.

- Section 4, page 20, line 378: I am not sure future is the correct tense to be used after "It is expected", maybe it should be: "It is expected that slope susceptibility is important for the extent…"; please check the tense;

Corrected.

- Section 4, page 20, line 389: Please check the English regarding the beginning of the phrase "This is as expected for earthquakes…", it's not very clear; maybe it could be replaced with something like: "This is in agreement with the effects of earthquakes of moderate magnitudes….";

The sentence has been rephrased: "This is in agreement with the expected effects of moderate-magnitude earthquakes that cause…"

- Section 4, page 20, line 390: please introduce a comma after "From a hazard perspective";

Corrected.

- Section 4, page 20, line 396: I would recommend replacing "Most of the most landslide-prone areas…" with "Most of the high landslide-prone areas…";

This sentence has been rephrased following a comment from another reviewer, and landslide-prone areas are no longer mentioned.

- Section 5, page 21, line 410: please use the full word instead of "1/2", as follows: " …and half to one order of magnitude larger than….";

Corrected.

- Section Appendices, page 21, lines 422-424: In the caption of Table A1, also the abbreviations ML and Mw should be explained as are the others;

Corrected.

- Please be consistent with the use of tenses throughout the paper: e.g. in section 3 Results you use, for similar statements, both present tense (line 242: "and the event is not included in our list") and perfect (line 289: " the debris slide has not been included as a separate event").

We have checked the use of tenses and made some revisions.