We thank the reviewer for the thoughtful comments and suggestions to our manuscript. We agree with the main points made by the reviewer and would like to revise our manuscript according to the suggestions. Some of the revisions proposed will be 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.

We agree that it would be helpful for readers unfamiliar with the Norwegian setting to include more information about the ground motion attenuation. A challenge here is that there is no reliable GMPE based on Norwegian data. We will, however, include more information on Norwegian ground motion attenuation in a revised manuscript.

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 can include a general discussion of the levels of ground motion expected at the locations of the EQIL, but we would find any attempt to estimate ground motions at specific locations for specific events 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 have tried to include details provided from the observers in the event descriptions when available. We will try to clarify this in a revised manuscript. 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. That event is certainly on the limit, and we will move the description to section 3.9 and thus exclude the event from the final EQIL dataset.

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 5. This will be clarified in the manuscript. Furthermore, we plan to revise Figure 5 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.