Reply to Review #2

We thank the reviewer for taking the time to thoroughly evaluate our work. Our detailed responses and the resulting changes to the manuscript are noted below. We have numbered the various comments to allow cross referencing. Our responses are in italics and we show text from the revised manuscript in quotation marks.

R2.01: The manuscript is well written and accessible in terms of its logical structure and objectives. The use of methods is somewhat standard, and offers little in terms of new insights. The results could be useful for those studying landslide inventories, if the statistical treatment would have been outlined in more detail. ESurf is a young journal so it is difficult to assess whether this contribution fits the general interdisciplinary scope. In any case, this work may need a number of substantial amendments mainly for reasons of a potential over-interpretation of a brushed-over statistical analysis that lacks any explicit treatment of errors or uncertainties. The central message of this study seems to be that a universally valid size-frequency model for landslides dictates interpretations on which landslide sizes are prone to censoring ("landscape annealing") or postglacial conditioning. However, this universality has not been demonstrated, and I have the feeling that the data here are twitched used to explain the model instead of the other way around.

The statistical analysis, including the treatment of uncertainties, has been completely revised in order to provide our results and interpretations with greater rigour. We were excited and surprised by the nature of the size distribution of this national inventory of landslides and sought to explain its features through comparison to previously documented landslide inventories. We do not claim to demonstrate the truth of our interpretation, because we realize that the dataset is yet incomplete. Nonetheless, we stand by our interpretations and offer them as interesting ideas to the community at large in order that other datasets and other investigators might a) be aware of them, and b) offer supporting or alternative interpretations.

R2.02: Abstract: This succinctly summarises the achievements of this study. Merely the quantitative detail and the mechanistic explanation for the observed differences of the inventory data with regard to event-driven inventories may want to see some better exposition.

The abstract has been rewritten to acknowledge the various comments by both reviewers. We have appended the new abstract to the bottom of this response.

R2.03: Introduction: This section gives a good overview on previous research, although brushes over (or even misses out on) some pertinent literature. For instance, van den Eeckhaut et al. (2007, EPSL) provided a thorough summary of the sort of analyses that are central to this manuscript. The role of substrate on landslide inventory statistics has also been discussed since the 1990s (Sugai and colleagues), and the same applies for "secular" landslide inventories, where most entries have no absolute ages attached.

We now include reference to the van den Eeckhaut et al. (2007) paper and thank the reviewer for highlighting this gap in our coverage. Unfortunately we have not been able to access the Sugai et al (1995) paper which we assume is being referred to.

R2.04: Data and Methods: Much more detail is needed on how the landslides were mapped originally in order to judge the quality of the dataset. It could be instructive to feature a figure that depicts the procedure of linking landslide point to polygon data, including the potential error sources involved. Using centroid points of landslide polygons for inferring underlying substrate may be compromised where landslide deposits cover substrate boundaries. This point should be duly addressed. The subsection on statistical analysis briefly describes two distribution functions, but falls short of explaining the fitting method and its underlying assumptions.

We have significantly expanded the data analysis sections to provide detail of the MLE approach to estimating parameters for the three distribution functions considered in this study in accordance with the recommendations made by the reviewers. We have added to the methods section that the use of a centroid to sample lithology is a shortcoming, but provides a simple approach to incorporating lithology into our study. We had previously used a point at the top of the landslide deposit but were concerned that common landslide occurrence where a more resistant cap rock (e.g. sandstone) overlies a weaker material such as mudrock or clay might lead to us not sampling in the material most likely to contain the failure. In the end, we decided to retain the centroid method.

R2.05: Results: This is where a number of useful results mingle with interpretations regarding similarities and differences between the UK dataset and other event-based inventories. I have a few suggestions here:

R2.05a: First, the authors may wish to keep separate the results from interpretations.

We disagree that results and interpretations were mixed together (and we agree that this would be inappropriate). The revised manuscript continues to separate results and interpretations.

R2.05b: Second, the authors may want to avoid comparing apples with oranges, given that data sources, mapping method and resolution and model fitting method usually differ between individual studies. It is not sufficiently clear whether the protocol of inferring the size distribution of UK landslides is adequately similar to that used by e.g. Malamud and colleagues. This is an important point that may distort the validity of the comparison, and should be dealt with in detail.

The point about "apples and oranges" is interesting, as landslides (in contrast to earthquakes, for example) do indeed come in many different flavours and data are acquired with different methods. Nonetheless, the observations from many analyses of landslides inventories suggest, using Occam's razor, that there may be a common underlying scaling law and that therefore there may be a common underlying process. Malamud et al suggest their data may be useful as a general model for the size distribution of event driven landslides for substantially complete inventories, and as such this model offers a useful comparison to a historical dataset known to be incomplete. We note, too, that our secular database is an amalgam of event-driven events, and therefore, any systematic differences between the two are intrinsically worthwhile investigating. We address some of the issues of completeness by comparison to an area of recently mapped landslides in the North Yorkshire Moors. We stress that there may be methodological differences in the documenting of landslides within our dataset itself, let alone in comparison to other studies and this is an inherent shortcoming of the dataset which we have tried to state explicitly in our methodology. Nevertheless the study of Malamud et al. (2004) found some consistency between different sites for event-triggered inventories compiled by different scientists, and therefore provides a useful benchmark for comparison of other landslide inventories.

R2.05c: Third, the authors may wish to elucidate whether they are dealing mostly with soil and debris landslides, given that "the majority of landslides occur in superficial material" (p. 123/l. 19). This could be an important issue to resolve, as landslides in surficial deposits may be prone to soil rather rock mechanic controls.

We note that superficial deposits refers to deposits of colluvium, till and alluvium, rather than soil. These deposits, as well as other areas of bedrock are all likely covered in soil. We are not able to distinguish landslides that occur in soil from those that incorporate superficial deposits and bedrock.

R2.05d: Fourth, using landslide abundance as a proxy of lithological resistance to erosion needs some justification, and may further need some reconciliation with the notion of "landscape annealing". The fitting of power-laws (why not double Pareto or Inverse Gamma models?) to lithologically stratified sub-samples yields different exponents, which seem to scale with sample size (i.e. steeper slopes with higher sample numbers). Clearly some more rigorous analysis is called for

On reflection we feel that using landslide abundance as a proxy for resistance is not possible when there are concerns about the spatial coverage of the data. We fit power laws by lithology because we were specifically interested in how the scaling exponent might vary with lithology.The solutions of Clauset et al. (2009) calculate standard errors as a function of sample size. The statistical analysis has been made far more rigorous.

R2.06: Discussion: This section needs some thorough attention. It revolves around the notion of an "expected" landslide size distribution, which happens to be that proposed by Malamud and colleagues (whereas the double Pareto fits seem to have been lost in the discussion). At the same time, the authors highlight "deviations" from this expectation by looking at subsets stratified by lithology and dominant type of landslide motion. For me it remains unclear what the authors wish to say or whether their intention is to prove the assertion of a universally valid size distribution for landslides right or wrong. They seem to be doing both at the same time. Similarly, I do not buy in to the notion that not having found some 150 postglacial large landslides that would otherwise have produced a better fit for the Inverse Gamma model is an indication of post- or paraglacial process control. This observation simply underlines a key problem in heavy-tailed statistics, i.e. that rare events may distort the fit, and hence model selection. What this study lacks is a rigorous statistical basis for quantifying significant differences between empirically estimated probability density functions regardless of sample size and mapping method. What is more, the whole discussion about post-LGM landslide abundance (for either larger or smaller landslides) hinges on the tacit assumption that the data have to fit the model, and not vice versa! Finally, the subsection about landslide hazard implications seems to confuse frequency with likelihood, and adds very little to points already discussed.

We do not wish to suggest that the model proposed as a general distribution by Malamud et al. 2004 is "expected" in the UK, but rather it provides a useful benchmark against which to draw comparisons between event triggered landslide inventories and secular datasets. Therefore, we interpret the size distribution of landslides in the UK beyond saying that we can fit a given model by making qualitative observations about how it compares to other landslide inventories and postulate about some possible reasons for differences. It was not our intention to test the universality of any model of landslide size distributions. We have expanded the discussion significantly to further address other possible causes of a large landslide deficit in line with the reviewers' recommendations and following feedback at a recent conference. Our treatment of the statistical analysis has been improved and expanded. We have also stressed in the section about hazard implications that if our interpretation is correct (that the statistical properties of the post-LGM landslides reflect a transient or non-stationary set of conditions and that a process of landscape annealing is occurring) then the model cannot be used to provide the probability of future landslides. This important statement is also in the new abstract (see below).

R2.07: Conclusions: These nicely synthesise the authors' interpretations, which focus more on the physical controls on slope stability in the UK than they do with regard to checking whether the initial inferences are correct. I would like to see a clear statement of whether the landslide size distributions in the UK are statistically different from models proposed earlier. If one of these models is indeed universal, lithology and other controls should not matter and remain undetectable in the plots shown. Before jumping to such conclusions, the authors should demonstrate that the methods of data acquisition and model building are comparable. Then they should explain how much variance a given universal model allows before trying to explain apparent outliers or lacking data via somewhat vague physical controls on slope stability.

The methods of data acquisition are not comparable even within our own dataset let alone in comparison to others, yet an interesting distribution emerges that we felt was worthy of discussion and worthy of being offered to the community at large

R2.08: 114/4: Consider deleting "usually". Many authorities have compiled landslide inventories regardless of specific triggering events.

OK, we now say "The statistical behaviour of the magnitude-frequency of landslide inventories is often characterized by a power-law relationship with a small landslide roll-over."

R2.09: 114/6: Reword "typically" to "often".

OK, we now say "The statistical behaviour of the magnitude-frequency of landslide inventories is often characterized by a power-law relationship with a small landslide roll-over."

R2.10: 114/12: "this secular inventory exhibits an inflected power law relationship, well approximated by an inverse Gamma or double Pareto model " – You are talking about three different distributions here. Which one is the most appropriate then? The scaling exponent should be reported with some sort of error margin.

The two distribution functions are very similar and either could be an appropriate description of the empirical dataset. The scaling exponent is reported in the following sentence, and error bands have now been included based on a bootstrap MLE approach described in the methods section.

R2.11: 114/16: "at these relatively short length-scales" – No reference has been made to these length scales yet. Is "landscape annealing" not simply censoring?

We remove the reference to short length scales and include the difference in area with peak frequency. We also expand description of landscape annealing in the new text to confirm that yes, this is geomorphic censoring (i.e. there is no difference between the two). These corrections are seen in the new abstract, appended to the bottom of this response.

R2.12: 114/17: "corollary" should be replaced by "inference".

OK

R2.13: 114/20: "we interpret as a non-linear or transient landscape response as the UK emerged from the last glacial maximum and through relatively volatile conditions toward a generally more stable late Holocene climate" – This is a very fuzzy and vague statement. Please be clearer and specify a mechanistic reasoning for this notion.

More specific mechanistic reasoning for this inference is provided in the discussion of the paper (related to the exposure of relatively steep slopes as the ice-sheet and permafrost melts and as material properties change during the paraglacial transition), but there is insufficient room in the abstract for this reasoning.

R2.14: 115/1: "generally better known" – This needs some reference. Also, the role of lithology on landslide frequency-magnitude statistics has been investigated by Sugai and colleagues nearly twenty years back.

We have rephrased this with reference to the summary of studies presented by van den Eeckhaut et al. (2007).

"We tackle two basic questions. First, does this national scale, historical landslide inventory reflect similar or different statistical properties as generally better constrained for single-event driven inventories and local scale historical inventories (cf. Van Den Eeckhaut et al., 2007). Second, what role is played by the underlying lithology and type of landslide?"

R2.15: 115/8: Suggest inserting "at least" before those estimates.

OK.

R2.16: 115/10: "pose a risk to infrastructure and are relevant in land use planning" – This is a very general statement. It would be nice for readers to learn a bit more of what this portrayed risk entails.

We acknowledge the generality of this statement but we believe it is fairly self-evident and have chosen to retain it as is; the reader is referred to the work of Gibson et al (2012) for further considerations.

R2.17: 116/1: "established" should read "proposed".

OK.

R2.18: 116/3: Delete "heavy-tailed". Not all reported studies supported this observation.

OK.

R2.19: 116/3: "power-law scaling of large events" – You need to clarify what you mean by "large events", and whether the quoted exponents refer to the cumulative or noncumulative forms of the distributions.

We referred to power-law scaling for medium to large events, purely as a descriptive approach as we build up the description of the "power-law with roll-over" distribution and therefore refrain from placing quantitative constraints on what we mean by medium-large landslides, since it is a relative description depending on the nature of the dataset examined, as is discussed further subsequently, but we had to start somewhere. We now state at the start of the paragraph that we are referring to non-cumulative distributions.

"Several studies have proposed that the non-cumulative size-frequency distribution for landslides (i.e. the number of slides of a give size occurring over a given length of time or within a given area) follows a negative power-law relationship for medium to large landslides (sensu lato)"

R2.20: 116/6: "vary from _ï´C˙z 1.0 (Hovius et al., 1997)" – Check value of exponent.

We now give a range of values reported based on the summary by Van Den Eeckhaut et al (2007) for non-cumluative statistics only. (The reported value from Hovius et al 1997 was for cumulative frequency density).

R2.21: 116/11: Avoid over-use of "typically". You are biasing your inference this way. Define "larger events". The observed rollover locations differ between studies, hence the definition of large also varies.

Agreed, the definition of large is relative to the observed dataset. We clarify:

"A negative power-law model only holds for landslides larger than a particular size, and this minimum size will vary between different inventories."

R2.22: 116/15: "a minimum critical size" – Unclear. Why are there landslides recorded with sizes below this critical size, thus creating the rollover?

We have rephrased this better explain how forces may interact to create the distribution:

"For complete landslide inventories the rollover has been interpreted as resulting from the interplay of cohesion and friction, whereby these forces offer resistance to landsliding for small and large landslides respectively (Guzzetti et al., 2002; Malamud et al., 2004; Pelletier et al., 1997; Stark and Guzzetti, 2009)."

R2.23: 116/20: "landslides being rapidly healed" – Expression. Landslides do not heal.

OK we have rephrased this:

"Under-sampling might occur due to evidence of small landslides being rapidly removed through recolonization by vegetation (Brardinoni and Church, 2004)"

R2.24: 116/23: "Two statistical distributions have been proposed to model the rollover" – Well, those concerned with submarine landslides have also proposed a log-normal distribution (see work by ten Brink and colleagues). Others have used Weibull distributions.

We emphasise that we are referring only to terrestrial landslides:

"Two statistical distributions have been proposed to model the rollover in size-frequency distributions of terrestrial landslides."

R2.25: 117/7: "then the probability distribution should also satisfy the sum" – Though it may have a different shape if it is to represent a mixture model.

We have rewritten this section:

"The universality of such a general model for landslide distributions has not been verified. Malamud et al. (2004) suggest it has applicability to historic, multi-trigger-event inventories since the model can be fitted to the large landslide tail of a historical inventory which is more likely to be a substantially complete record, since evidence of larger landslides will persist for longer time periods in a landscape. As a result, by comparison to the proposed general distribution, the total number of landslides associated with a particular trigger can be predicted even for an incomplete landslide inventory (Malamud et al., 2004)."

R2.26: 117/11: "show similar power-law scaling" – Revisit the argument by Larsen et al. (2010) to see how deceptive such similarity may be.

We add this to the introduction:

"Larsen et al. (2010) caution that estimates of volume of material transported by landslides may be very sensitive to this scaling exponent, resulting in prediction errors of over an order of magnitude."

R2.27: 117/13: "difficulty in documenting smaller landslides from aerial photos and their tendency to amalgamate" – This contradicts the claim of substantially complete inventories made earlier on.

I can't see where we made this claim, but have added this qualification:

"Guzzetti et al. (2008) interpret that the offset as due to difficulty in documenting smaller landslides from aerial photos and their tendency to amalgamate (i.e. incompleteness of the record)"

R2.28: 117/15: "due to landscape annealing by reworking of deposits and recolonization by vegetation" – The notion of "landscape annealing" (and its many synonyms) needs some better exposition here or in the discussion.

"Under-sampling might occur due to evidence of small landslides being rapidly removed through erosion, the reworking of deposits and recolonization by vegetation (Brardinoni and Church, 2004)"

R2.29: 117/16: "Such an analysis has not until now been performed on a secular inventory spanning a large spatial and temporal range." – Debatable. Whitehouse and Griffiths started with this sort of analyses in the early 1980s on Holocene rock avalanches in New Zealand. Van den Eeckhaut et al. (2007) and Larsen et al. (2010) review a number of "secular" inventories.

We have deleted this claim, but note that most historical inventories are of shorter temporal range and over more localised spatial scale.

R2.30: 117/28: "shifted toward larger landslides" – This is not surprising and a common characteristic of power-law tails. Fewer (= rarer) larger events will more easily distort the fit statistics.

We have crossed wires here, as we were referring to the position of the frequency peak (i.e. the roll over, rather than the rare large events. We have tried to clarify this:

"the position of the rollover in frequency was also shifted toward larger landslides in the Southern Alps compared to Fiordland"

R2.31: 118/6: "lack of studies relating the size-frequency distribution of landslides to the type of material failing" – See Sugai et al. (1995, I think), and Larsen et al. (2010). Both articles feature the issue of material type in their title.

We have added to this section to acknowledge the work of Larsen et al. (2010) and also Frattini and Crosta (2013) in order to set out our motivation to explore the influence of material type on landslide size distributions:

"Frattini and Crosta (2013) constructed synthetic size-frequency distributions using slope stability analysis to suggest that less resistant materials tend to promote more shallow landslides whilst more resistant lithologies tend toward deeper landslides with limited numbers of smaller landslides, consistent with our suggestion that more resistant lithologies are relatively less prone to small landslides in the SLI. This has important implications for the volume of materials transported by landslides in different materials too. Larsen et al. (2010) compiled a global dataset of landslide geometries and observed that scaling of volume with area for shallow, soil landslides has a lower exponent than for deep-seated bedrock landslides. A general model for the distribution of landslides (Malamud et al., 2004) may not take into account lithologic variability and differences in the type of mass movement processes (which are likely linked themselves)."

R2.32: 118/8-14: This section seems a bit out of logical sequence and would do great if moved a few paragraphs up.

Agreed, we have put this much earlier.

R2.33: 119/6: "compiled from secondary sources" – This is a bit hazy. Could you please be more specific.

"The database is managed by the British Geological Survey (BGS), having inherited and expanded a database initially compiled from a desk study carried out by Geomorphological Services Limited in the late 1980s to document the occurrence of landslides in the UK on behalf of the UK Government's Department of the Environment (Jones and Lee, 1994). The database consisted of records compiled from journal articles and reports, and maps and reports held by the BGS (Foster et al., 2012) The NLD has expanded from this origin and is maintained and managed by the BGS."

R2.34: 119/18: "1 : 10000 and 1 : 50000 scales" – For which of these scales was the size information about the landslides extracted?

We used size information from landslides mapped at both scales. The majority come from the 1:50000 maps and the 1:10000 provide additional data, mainly for smaller landslides. We have stated this in the Sampling Methods section.

R2.35: 121/6: "superficial deposits" – Please provide some examples. Does this include soils? If so, can you tell soil from debris and rock landslides?

Superficial deposits do not include soil but rather refers to young (Quaternary age) geological deposits (glacial and alluvial) which rest on bedrock. We have clarified this definition in the manuscript. We currently cannot distinguish soil and bedrock landslides in the database.

R2.36: 121/11: Replace "defined" by "estimated".

OK.

R2.37: 121/20: "b is a coefficient" – Needs units specified.

We have changed how we present a power law so that the coefficient is a function of α and the minimum landslide size above which the power law is fitted A_{min} (following Clauset et al., 2009).

R2.38: 122/18: "diminishing in a power-law fashion" – How can you tell? Have you tested for a power law?

We now write "appearing to diminish in a power-law fashion". Details of our model fitting results appear further on.

R2.39: 125/7: "expected, general distribution for event-triggered landslides" – Why expected? Or should it be "proposed"?

Changed to "proposed".

R2.40: 125/10: "relative incompleteness of the SLI" –And what about differences in the mapping methods?

We now make reference to this possibility here as well as going on to test the likely cause of the offset by reference to a substantially complete historical inventory from the North Yorkshire Moors in the subsequent paragraph (as before).

R2.41: 125/22: "considered to be a complete historic inventory" – On which grounds of evidence?

In the sense that it has been recently mapped in detail using combination of remotely sensed data and field recognition.

"To test the extent to which small landslides are under-represented in the SLI, we analyzed separately a subset of the landslides data recently mapped in the North Yorkshire Moors, which we considered to be a substantially complete historic inventory (a comprehensive record of landslide deposits mapped through analysis of high resolution topography, aerial photography and field mapping)"

R2.42: 126/12: "important implications for landslide size and associated hazard" – This statement is frequently used in the manuscript, though I do not see anything more specific. On the one hand, you argue for an "expected" trend in landslide size distributions, on the other hand you stress the diversity if lithology or dominant movement type comes into play.

We go on to explain why the variation in α with lithology may be important, with more resistant lithologies potentially being relatively more susceptible to larger landslides. As you point out this suggests an expected trend may not account for variation in material properties or the style of landslides, which was an argument we were keen to make but perhaps did not articulate well. The amended text is:

"Landslides in superficial deposits and soft lithologies dominate the SLI, whilst harder lithologic groups exhibit distinct magnitude frequency scaling characterized by lower values of α setting lower scaling gradient in log-log space (Figure 4a; Table 1). This result has important implications for landslide size and associated hazard. Whilst there is significantly lower probability of small landslides in more resistant lithologies, the difference is minimal for larger landslides (~10^6 m^2). Perhaps unsurprisingly the largest proportion of landslides and in particular smaller landslides (< 10^3 m^2) occurs in poorly consolidated superficial deposits and hence characterization of superficial materials will be important to site-based investigation of landslide susceptibility. Frattini and Crosta (2013) predict that size distributions should differ with material properties, such that weaker materials should result in more small, shallow landslides whilst stronger materials may promote relatively more large, deep seated landslides. Our findings also suggest that geology will play an important role in setting the size distribution of landslides suggesting that the influence of lithology should be further explored."

R2.43: 127/5: "377 k landslides" – Spell out.

Done.

R2.44: 127/10: "landslides expected by inverting Eq. (1) for N for the fitted inverse gamma function" – What happened to the double Pareto fits?

This was done to get an estimate of roughly how many large landslides we'd need to make up the large landslide deficit. This section has been revised to expand discussion of the potential causes of the large landslide deficit:

"A national landslide inventory for Italy comprising ~377 thousand landslides (Trigila et al., 2010) exhibits power-law scaling above 10^{-2} km^2 similar to the SLI (Figure 3a). Interestingly both datasets show deviation from fitted scaling relationships for the largest landslides (> 10^0 km^2 for the UK; >10^1 km^2 in Italy) suggesting that either we are under-sampling with respect to the largest landslides or large events are less frequent than power-law scaling would predict. The difference in cutoff areas between the two datasets may be the result of only reporting the areas of mapped deposits in the UK whilst in Italy area refers to the combined source and sink outline.

There are a number of possible explanations for this apparent deficit of large landslides. Firstly, the dataset is expected to be incomplete and there may be some large landslides that have not been recorded. It seems unlikely, however, that the deficit represents observational bias, since large landslides should be the most prominent in the landscape. The deficit may, in fact, be larger, as inspection shows that some of the largest mapped deposit areas consist of amalgamated deposits of numerous smaller events. Therefore, we suggest the deficit is real. Possible explanations for the deficit include spatial bias in the coverage of landslide mapping (i.e. the database is incomplete), the spatial limitation of large landslides due to a similar spatial limitation of suitable topography, and temporal transience in the occurrence of landslides since the last glacial maximum (LGM).

For a large landslide to occur requires a large slope. The availability of the highest relief is spatially limited in the UK to Central and North Wales, the Lake District and the Scottish Highlands. As previously observed, data coverage in these regions is not as extensive as in other parts of the British Isles. The relative paucity of large slopes (and associated large landslides) elsewhere in the country may result in the large-landslide deficit. We refer to this as an incomplete landscape.

Figure 6 shows the spatial density of mapped landslides comprising the SLI. Whilst we would anticipate that areas with the greatest relief and steepest hillslope gradients might *contain the highest density of landslides, this seems not to be the case in the SLI. Coverage of landslide mapping in the Scottish Highlands and parts of north Wales are sparser than other low relief areas of the UK. This is of particular relevance since these areas will have large slopes which may yield large landslides, and these areas tend to be underlain by more resistant lithologies.*

It is likely that the bulk of landslides range in age from the LGM (~27 ka) to the present-day. During this time, climate will have varied as the British Ice Sheet receded (e.g., Clark et al., 2012), and mass movement processes are likely to have been initially more active as soils and regolith both warmed and lost structural support from ice-cover and permafrost. We speculate, therefore, that many landslides and certainly most of the larger landslides would occur early in this LGM-to-present time-span, during the paraglacial transition (Ballantyne, 2002). Unlike active mountain belts, steep slopes will not be regenerated by continued rock uplift and erosion, and therefore the drivers for those landslides are gradually reduced over time as the emerging landscape passes through a period of readjustment to new and more stable conditions. Instability likely continued through the variable climate immediately prior to the Holocene, and returned again during the latter part of the Holocene (Neolithic times, in particular) as extensive anthropogenic forest clearance and land-use changes occurred. These latter processes, all else being equal, would lead to an increase in the rate of landslide activity, consistent with rapid Neolithic valley sedimentation observed in many parts of the UK (Brown, 2009). We suggest, therefore, that the population of landslides in the SLI may be dominated by the relatively rapid denudation of early post-LGM and early anthropogenic times, with the result that relatively large landslides show a deficit with respect to a model-fit that is derived principally from the relatively greater number of smaller to moderate sized landslides."

R2.45: 127/13: "It seems unlikely that this many relatively large landslides have been missed" - For an area as large as the UK? I don't see the point here.

See R2.44.

R2.46: 127/17: "possible explanation for the apparent deficit of relatively large landslides" – A simpler one is that these events have not yet been recorded.

Possibly, and we have included a discussion of this, along with alternative explanations. See R2.44.

R2.47: 127/28: What is a "volatile climate"?

We meant "variable", which we now use.

"We also suggest that the discrepancy between model and observations for relatively large landslides is a function of a transient landslide response as the UK emerged from glacial conditions and into an initially variable (e.g. Allerød warming and Younger Dryas events) then relatively stable Holocene climate."

R2.48: 128/26: "probability" – You are confusing frequency with probability. In this case, you are referring to a likelihood that is conditioned on your assumption that the (which?) model is correct.

Agreed, we have removed all usage of the term probability from this section since we do not wish to suggest that the probability of future events can be directly inferred from the frequency distribution of past events.

Ballantyne, C.K., 2002. Paraglacial geomorphology. Quaternary Science Reviews, 21(18), 1935-2017.

Brardinoni, F., Church, M., 2004. Representing the landslide magnitude–frequency relation: Capilano River basin, British Columbia. Earth Surface Processes and Landforms, 29(1), 115-124.

Brown, A.G., 2009. Colluvial and alluvial response to land use change in Midland England: An integrated geoarchaeological approach. Geomorphology, 108(1), 92-106.

Clark, C.D., Hughes, A.L.C., Greenwood, S.L., Jordan, C., Sejrup, H.P., 2012. Pattern and timing of retreat of the last British-Irish Ice Sheet. Quaternary Science Reviews, 44(0), 112-146.

Clauset, A., Shalizi, C.R., Newman, M.E., 2009. Power-law distributions in empirical data. SIAM review, 51(4), 661-703.

Foster, C., Pennington, C.V.L., Culshaw, M., Lawrie, K., 2012. The national landslide database of Great Britain: development, evolution and applications. Environmental Earth Sciences, 1-13.

Frattini, P., Crosta, G.B., 2013. The role of material properties and landscape morphology on landslide size distributions. Earth and Planetary Science Letters, 361(0), 310-319.

Guzzetti, F., Ardizzone, F., Cardinali, M., Galli, M., Reichenbach, P., Rossi, M., 2008. Distribution of landslides in the Upper Tiber River basin, central Italy. Geomorphology, 96(1), 105-122.

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New abstract:

Linking landslide size and frequency is important at both human and geological time-scales for quantifying both landslide hazards and the effectiveness of landslides in the removal of sediment from evolving landscapes. The statistical behaviour of the magnitude-frequency of landslide inventories is are usually compiled following a particular triggering event such as an earthquake or storm, and their statistical behavior is often characterized by a power-law relationship with a small landslide roll-over . The occurrence of landslides is expected to be influenced by the material properties of rock and/or regolith in which failure occurs. Here we explore the statistical behavior and the controls of a secular landslide inventory (SLI) (i.e. events occurring over an indefinite geological time period) consisting of mapped landslide deposits and their underlying lithology (bedrock or superficial) across the United Kingdom. The magnitude-frequency distribution of this secular inventory exhibits an inflected power law relationship, well approximated by either an inverse Gamma or double Pareto model. The scaling exponent for the power-law scaling of medium to large landslides is $\alpha = -1.71 \pm 0.02$. The small-event rollover occurs at a significantly higher magnitude ($1.0\text{-}7.0 \times 10^{-3}$ km^2) than observed in single-event landslide records ($\sim 4 \times 10^{-3}$ km^2). We interpret this as evidence of landscape annealing, from which we infer that the SLI underestimates the frequency of small landslides. This is supported by a subset of data where a complete landslide inventory was recently mapped. Large landslides also appear to be under-represented relative to model predictions. There are several possible reasons for this, including an incomplete dataset, an incomplete landscape (i.e. relatively steep slopes are under-represented), and/or a reflection of a transient landscape response as the UK emerged from the last glacial maximum through a highly variable climate and toward a generally more stable late Holocene state. The proposed process of landscape annealing and a transient response of the landscape has the consequence that it is not possible to use the statistical properties of the current SLI database to rigorously constrain probabilities of future landslides in the UK.