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# *Interactive comment on* "Controls on the magnitude-frequency scaling of an inventory of secular landslides" *by* M. D. Hurst et al.

**Anonymous Referee #2**

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Review of Earth Surf. Dynam. Discuss., 1, 113–139, 2013: "Controls on the magnitude-frequency scaling of an inventory of secular landslides" by M.D. Hurst and others

This study presents new statistical analyses on a national landslide inventory for the United Kingdom. Hurst et al. follow the procedures of numerous previous studies by fitting several distributions to log-transformed data on the frequency and magnitude of slope failures that were mapped mostly without any individual age constraints. The main findings of this study derive from the assumption that an inverse power law is sufficiently characterising both dataset and the controls of slope stability in the UK: The authors thus report that (a) the purported power-law tail of the size frequency data commences at landslides that are much larger compared to other event-based inven-



tories, and (b) larger landslides tend to be under-represented. These observations are then attributed to "landscape annealing" and some major climatic perturbations since the Last Glacial Maximum, respectively.

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.

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.

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.

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.

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: First, the authors may wish to keep separate the results from interpretations. 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. 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. 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. Are those exponents statistically different at the same sample size?

Discussion: This section needs some thorough attention. It revolves around the no-



tion 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.

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 be 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.

Some suggestions (page/line)

114/4: Consider deleting “usually”. Many authorities have compiled landslide inventories regardless of specific triggering events.

114/6: Reword “typically” to “often”.

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.

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

114/17: “corollary” should be replaced by “inference”.

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.

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.

115/8: Suggest inserting “at least” before those estimates.

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.

116/1: “established” should read “proposed”.

116/3: Delete “heavy-tailed”. Not all reported studies supported this observation.



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 non-cumulative forms of the distributions.

116/6: “vary from $\alpha$ ïĆż 1.0 (Hovius et al., 1997)” – Check value of exponent.

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.

116/15: “a minimum critical size” – Unclear. Why are then landslides recorded with sizes below this critical size, thus creating the rollover?

116/20: “landslides being rapidly healed” – Expression. Landslides do not heal.

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.

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.

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

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.

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.

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.

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.

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.

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

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

119/18: “1 : 10000 and 1 : 50000 scales” – For which of these scales was the size information about the landslides extracted?

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

121/11: Replace “defined” by “estimated”.

121/20: “b is a coefficient” – Needs units specified.

122/18: “diminishing in a power-law fashion” – How can you tell? Have you tested for a power law?

125/7: “expected, general distribution for event-triggered landslides” – Why expected? Or should it be “proposed”?

125/10: “relative incompleteness of the SLI” –And what about differences in the map-



ping methods?

125/22: “considered to be a complete historic inventory” – On which grounds of evidence?

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 in landslide size distributions, on the other hand you stress the diversity if lithology or dominant movement type comes into play.

127/5: “377 k landslides” – Spell out.

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

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.

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

127/28: What is a “volatile climate”?

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.

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