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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 #1

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The authors have determined and analysed the frequency/probability density of landslide area for a national inventory of landslides in the UK. With the exception of the work of Trigila and co-workers (2010) [cited by the authors], this is the only paper that attempts a critical analysis of the frequency-size distribution of landslide size for a national inventory. Other similar works (e.g., Guzzetti et al. 2008 [cited], although they treat a larger dataset, they cover a significantly reduced study area. This is a merit of this work.

I have a few comments and some questions that I list below.

I am not convinced that the word "secular" used in the title and in other parts of the text is good to represent the type and temporal span covered by the national inventory. Is this the same type of inventory that Malamud and co-workers (2004) [cited] have called “historical”, and Guzzetti et al (2012) [cited] “geomorphological”? Please clarify.

The second paragraph of the Introduction (page 115, lines 5-16) is out of context, really. Consider deleting the paragraph. The section 4.4 Implications for hazard assessment does not justify the paragraph.

In the review of the literature on the frequency/probability statistics of landslide size (pages 116-118) there are a few points that need clarification, or attention.

(A) Some confusion exists because in the literature authors have used “cumulative” (e.g., Dussauge et al. 2003 [cited]) and “non-cumulative” (e.g., Malamud et al. (2004) [cited]) statistics. Comparison of the results of the different studies is therefore problematic. The authors should make this clear, and specify which statistics were obtained from cumulative distributions, and which from non-cumulative distributions.

(B) In the text the author mix (confuse?) statistics of landslide area (e.g., Hovius et al. 1997, Pelletier et al. 1997, Stark & Hovius 2001, Guzzetti et al. 2002 [all cited]) and landslide volume (Dussauge et al. 2003 [cited]). Dussauge et al. (2003) studied rock falls, and all the other authors studied landslides of the slide or flow types. The difference is relevant, and the statistics may not be comparable. For landslides of the “slide” type authors have found that the relationship linking landslide area and volume is non-linear (e.g., Parker et al. 2011 [cited], Guzzetti et al. 2009, doi:10.1016/j.epsl.2009.01.005, Larsen et al. 2010, doi:10.1038/NGEO776). I am not aware of studies linking the area and volume of rock falls.

(C) Brunetti et al. 2009 [doi:10.5194/npg-16-179-2009] have re-examined the dataset of rock fall volumes compiled by Dussauge et al. (2003), and have determined a different (and larger) scaling exponent for the power law distribution that describes the empirical data. These authors have also found a difference on the scaling of the power law describing the volume of “falls” and “slides”.

(D) The “double Pareto” (Stark & Hovius, 2001 [cited]) and the “inverse Gamma”

(Malamud et al., 2004 [cited]) are the most common distributions used to model the probability/frequency-area distribution of landslides. With a few exemptions (e.g., Guzzetti et al. 2008 [cited]), they are not used to describe the probability/frequency-volume of landslides (of the "slide" or the "flow" types), although this is certainly possible. For the investigation of the probability distribution of rock fall volumes see e.g., Brunetti et al. (2009) [doi:10.5194/npg-16-179-2009]. Recently, Chen et al. (2011) [doi:10.1209/0295-5075/95/49001], based on the application of non-extensive statistics, have proposed an analytical distribution function to describe the frequency-area distribution of landslides.

I have some concern on the method used to assemble the landslide database that was used for the analysis. The authors are clear in explaining the steps they have taken. However, inspection of Fig. 2 makes me think that landslides were not mapped very accurately, at least for part of the database. This has influence on the frequency-area statistics, and on the conclusions drawn. As an example, the shape of the landslide polygon centred on coordinates E412000,N384000 in the map shown in Fig. 2 is indicative of possibly multiple coalescing landslides, and not of a single (and larger) landslide. This may be due to the scale of the mapping, which is relatively small (1:50K). The authors should comment a bit more on the quality (and diversity in quality) of their inventory. They should also consider adding a few more examples showing areas mapped at different scales, obtaining inventories of different qualities.

It is not entirely clear to me how the frequency/probability density was obtained, and showed in the Figures 3, 4 and 5. The authors first state that they have (page 121) "calculated FD and PD for the NLD dataset by sorting the data into logarithmically-spaced bins in A". It is unclear how this was done. Where bins all of the same size, in log coordinates, or not? Logarithmic binning has is problems when the number of points is reduced. This may be the case for the very large landslides. Then the authors state (page 123) that the double Pareto and the inverse Gamma distributions were estimated using "maximum likelihood estimates to find the best fit parameters".

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I presume MLE was applied to the raw data (and not to the log-binned data), but this is not clear. More information is required on the technique(s) used to determine the frequency/probability densities.

Some of the discussion is based on the qualitative (visual) or quantitative (using the parameters controlling the equation) comparison of the different probability density models obtained for the different subdivisions of the landslide dataset. However, it is not clear what is the uncertainty associated to the different probability densities. Depending on the uncertainty, in Fig. 4a the densities for the surficial, mudstones, interbedded and coarse clastic deposits may be indistinguishable, or may be statistically different. The same is for the density for carbonates, metamorphic and igneous rocks. In Fig. 4b, the four frequency densities for rotational, planar, flow and fall type landslides my also be statistically indistinguishable, or not, depending on the associated uncertainties. This is a crucial point that needs to be resolved. The density models were determined using MLE. It should therefore be possible to determine the confidence levels for the individual density models (e.g., using a bootstrap method), and to compare them. My doubt is that for some of the datasets the number of samples may be too limited to constrain sufficiently the density models.

The authors observe a paucity of very large landslides in their distributions. The explanations given for this finding are plausible. I have two suggestions on this topic. First, clearly the size of the very large landslides is somewhat controlled by the size of the slopes where the large landslides occur. Is it possible that the lack of large landslides is related to the lack of very large slopes? Second, is it possible to segment the landslide database for the UK on time, and use the (relatively) recent landslides (e.g., those occurred in the last 50 or 100 years) to investigate the extent to which the power law scaling for large landslides holds?

In the text and the Table, do not use e.g., 15.3 x 10ˆ3 kmˆ2, but instead 1.53 x 10ˆ4 kmˆ2.

Fig. 1. Add geographical reticule to the map.

Fig. 2. Add scale bar, or clarify in the map that coordinates are in metres (m).

Fig. 3. The journal accepts colour figures at no extra cost. The authors should exploit this opportunity and make the Figure in colours. This will improve the readability of the Figure. Indicate the number of samples in the landslide dataset. A suggestion: do not use dashed lines in the box plot, and provide a legend for the box plot (different criteria can be used to prepared the box plots, and without a legend it is impossible to tell what the different elements of the box plot (rectangle, central line, range) represent.

Fig. 4. Suggestion: Indicate the number of samples in the different landslide datasets. Do not use dashed lines in the box plot, and provide a legend for the box plot (different criteria can be used to prepared the box plots, and without a legend it is impossible to tell what the different elements of the box plot (rectangle, central line, range) represent.

Fig. 5. The journal accepts colour figures at no extra cost. The authors should exploit this opportunity and make the Figure in colours. This will improve the readability of the Figure. Indicate the number of samples in the landslide dataset. Do not use dashed lines.

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