Dear Reviewers, dear Editor,

thank you for your constructive comments! The points addressed in the two reports are discussed below, where changes to the manuscript are highlighted in bold letters. Line numbers refer to the version with highlighted changes.

Best regards,

Stefan Hergarten

Reviewer 1 (Maria Teresa Brunetti)

Overall, the paper is well written, but I would recommend providing more details to facilitate understanding of the mathematical steps that, although formally correct, are not so obvious, at least to a reader of NHESS. Steps need to be clearly explained even at the cost of some verbosity.
In addition, given the diverse expertise of potential readers, a more extensive explanation of the physical meanings of the variables used in individual equations would be appreciated.

I added some more explanations about the mathematical framework, mainly guided by your comments in the manuscpript and by the comments of the second reviewer.

Regarding the theoretical framework, an event size-dependent depletion in analogy with the case of wildfires (Drossel-Schwabl forest-fire model) is innovative and interesting, but would require a stronger phenomenological/physical hypothesis. It is worth explaining why should larger landslides behave like burned areas, the extent of which depends on the interaction of individual adjacent spatial units.

I would argue that a potential dependence on size is the default assumption, while a probability independent of the size is specific and would need to be justified. In this context, I think that a power-law dependence with an adjustable exponent as proposed in Sect. 3 is a rather general approach since a size-independent probability would also be captured (exponent = 0). **Anyway, I added a second part to the motivation section (Sect. 2.2) based on my old rockslide model from 2012.** Although this model is not necessarily realistic, it may help to illustrate the idea and make clear why the assumptions makes sense.

Minor revisions are in the attached file.

Line 32: I would suggest using always the same units, i.e. m^3 or km^3 for a better comparison between the landslides sizes.

I have thought a lot about this idea at several occasions. However, I can easily imagine what 1000 m^3 are, but not what 10^{-6} km^3 are. In turn, 1 km^3 is easier to imagine that 10^9 m^3.

Lines 54–55: Do you mean tectonics and fluvial incision? SOC is observed also in lunar landslides, where fluvial incision isn't (and wasn't) present.

Not tectonics and fluvial incision, but landsliding vs. any slow process that produces relief. **I rephrased it (line 54)**.

Line 97: (t)

I am afraid that I did not get the point here.

Line 111: In the radioactive decay process, the number of remaining undecayed isotopes $N(t)$ follows the negative exponential law. Is it correct to assume the same law for a frequency density function?

Yes, of course. The frequency density is in principle just the number of objects of size s (per event size). **I added a little explanation, although I am not sure whether it helps (lines 148–149).**

Line 120: Isn't n, accordingly, the initial number of objects with size greater than or equal to s?

Yes, of course. But this is just n. **I added the relation $\Phi(1, 0) = n$ and hope that it is clearer now (line 159).**

Line 130: Please, use a different letter to substitute α, since e is the Euler number. For sake of clarity you can add (as above) that you substituted $-\alpha$ and μst in Eq. 9.

Good point, **I replaced it with q (line 169).** However, replacing the variables $-\alpha$ and μst is too obvious for explaining it explicitly.

Line 133: per unit time (year) and at time t

I added it, but slightly above (line 170).

Line 136: Why you selected these values. What is the physical meaning? Is it an observational choice?

The exponent of the DS-FFM is $\alpha \approx 1.2$, but here it was just chosen in order to obtain a nice plot. The parameter μ only affects the absolute time scale. **I added this information (lines 175–176),** although I am not in favor of adding too much unnecessary information.

Line 141: Please, explain it better.

I rephrased it in order to clarify that it is an immediate consequence of the text prior to the equation (lines 180–181).

Line 152: Is this a consequence or an assumption? Please, be more clear.

It immediately arises from the definition of V_0. **I tried to clarify it (lines 191–192).**

Lines 154–156: Could you, please, explain better this part, which is not straightforward?

I expanded the text a bit (lines 193–199). Hopefully, it is clearer now in combination with the new Sect. 2.2.

Caption Fig. 4: The results for exhaustion start times shown in the graphs deserve a more in-depth explanation in the main text.

I added some explanation (lines 245–246).

Lines 193–194: Could you provide the number?

Looks as if you imagine likelihood as a number with a value of 1 indicating "certain" or so. Since it is a probability density, this is not the case. Therefore, it was already stated in lines 192–193 that absolute likelihood values have no immediate meaning. Just as a clarification: The maximum likelihood value occurring in Fig. 4(a) is 6.26×10^{-22} m^{-12} where the unit m^{-12} arises from nondimensional properties (numbers) in criteria 1–3 and volumes in criteria 4–7. I cannot imagine that any reader would find this value useful.

Line 198–199: This concept is very important in my opinion. Could you explain it better?

Maybe important, but so far only addressed in the literature for shallow landslides in soil-mantled slopes. **I added a reference to Fig. 3 and some explanation (lines 253–255).**

Lines 201–202: The description of the two alternative scenarios requires a clearer explanation because it is not immediately understandable. Why you need to define these two additional scenarios?

I added some more explanation how these two scenarios are related to the potential incompleteness of the data concerning large events (lines 258–271).

Line 216: It could be useful to say why you are using this type of distribution.
Line 217: Please, explain again the meaning of each variable.

I added a short derivation of the equation (lines 287–296). By the way, it should have been $\Phi(V, t)$ instead of $F(V, t)$.

Line 220: Why? It is not trivial to me.
Lines 221–222: Please, explain it better.
Line 223: Could you please provide the uncertainty in the estimation/measurement of the volume? The question arises because the reported values, which are approximated to several decimal places.

Initially, I thought that this aspect was important, but probably nobody would ever think about it. **So I focused the discussion on a slightly different aspect (lines 303–330),** which hopefully helps to get a better feeling for the uncertainties.

Line 234: I would add "according to this framework".

Ok (line 335).

Caption Fig. 9: It's quite difficult to me to distinguish the different lines

Right – I just want the readers to become clear that the two alternative scenarios are very similar. So I do not worry about the similar lines.

Lines 249–251: This could be likely more clear, explaining better lines 154 to 156.

Hopefully **(lines 193–199 and 355–367)**.

Line 268: For a clearer understanding, I would add: "..., which shows the relationship between the cumulative frequency and the time t_0..."

However, it is not only t_0, but the range from t_0 up to 8000 BP. So I am not sure whether this would provide a clearer understanding.

Line 268: Could you, please, explain why referring to colors and lines in Fig. 9?

Just because I want to consider the three different scenarios as well as the five different values of α_V. However, I have no idea how to explain why I want to consider both components.

Line 270: Where is his case in Fig. 9?

In the left, upper part. **I added some text (lines 381–383).**

Line 271: Actually, we don't know if the observed rockslide datasets are affected by a cutoff due to exhaustion or not. This is why when calculating the slope of the distribution we exclude both the initial rollover and the far end part of the distribution.

In principle, we know that they are affected by a cutoff since the number of huge rockslides predicted by the power law would be very high. Of course, we do not know whether this cutoff arises from exhaustion or from limited relief. Anyway, this discussion would not fit into the line of arguments about the initial distribution here.

Line 274–275: Why? Please, clarify

I added some explanation (lines 386–394).

Line 277: I would add: "... for all scenarios in Fig. 9".

Ok (line 396).

Line 284: Why? Please, refer to a Fig.

I described it in more detail and referred to Fig. 8 (lines 403–404).

Line 316: At which time?

Hm, it is written in the previous sentence that it is the size interval from s_1 to s_2 and the time interval from t_1 to t_2, and the following equation contains t_1 and t_2. Would it really make sense to repeat it once more?

Line 333: Why? It is not trivial to me.

Not trivial, but basic statsistics. **I tried to explain it a bit better (lines 454–459),** but I think it is not useful to introduce all fundamentals of statistics here.

Line 338: You mean 4-7?

Right – thanks! **Anyway, the text has changed,** owing to the more detailled description of the maximum likelihood approach.

Reviewer 2

The authors develop a theoretical framework to explain event size dependent exhaustion. They propose a mathematical formulation for the decline of big events capturing statistical behavior of self-organized criticality with a simple cellular automaton forest fire model (DS FFM) as an example. Next, the authors apply this framework to rockslides.

This is an interesting concept and I believe the paper can be of added value to the rockslide/landslide community but only after considering the following points:

Since I am not a member of the rockslide/landslide community, I have to believe that the paper would not be of added value without taking into account these points.

The mathematical derivations at the heart of this manuscript are, at several points, difficult to follow. I provide some line comments below, but in a more general sense, the text would highly benefit from clear and structured explanations for every assumption made, equations proposed, and parameter values chosen.

Unfortunately, I am not sure what clear and structured explanations look like in your field. Therefore, I can only follow your detailled comments below.

The model is tested/validated using a very limited dataset with observations. This is almost certainly an incomplete dataset restricted to a relatively small area (on a global scale). The implications of the limited dataset used here should be discussed thoroughly and implications for model accuracy and robustness in general should be made.

I expanded the discussion about the uncertainties (lines 258–271 and 303–330). However, I do not understand the argument about the small area.

Also, a reflection should be made on the validity of this framework and SOC models in general. Are rockfalls following self-organized criticality and why is that? Is the mechanism that explains rockfall size similar to the mechanisms that explain forest growth and decay. Some reflection on spatial correlation of rock properties is needed here. Is there potential to scale this exercise up to larger (global) scales?

Please allow a statement from my personal point of view. I wrote my first paper on (potential) SOC in landslides 25 years ago. The idea of SOC motivated me to look at earthquakes, wildfires and some other phenomena. Without looking at the DS-FFM in detail, I would never have had the idea behind this manuscript. Therefore, looking at landslides with SOC in mind is great. In turn, SOC never became the unifying theoretical framework it promised to be. And to be honest, all these models that were tuned to reproduce the power-law distriubtion of landslides did not bring much. Of course, a critical paper about the benefit (if there is any) of SOC in geomorphology would be useful. However, I just do not write about this topic here. I also do not see any immediate use in discussing spatial correlations of rock properties here. Upscaling to the global scale would probably only be possible by calibrating a model such as my old model for rockslide disposition (or any similar) model. However, it is really difficult to understand why there is no added value to the landslide/rockslide community without taking these aspects into account.

The mathematical framework is proposed as a useful test for larger scale models such as a model published by the same author. Explicit comparison of modelled rockfall (Hergarten, 2012) with this framework in a dedicated section would illustrate this concept more clearly.

I introduced a new section (2.2) about this old model, but really just for illustrating that the concept makes sense. Analyzing this model in detail with regard to the concept of exhaustion would go beyond the scope of this paper.

Line 27: can be excluded for these two rockfalls

I added something similar (line 27).

Line 54: Do you mean that a system which exhibits SOC develops an equilibrium between slowly but continuously evolving versus rapid discrete event-based processes? Please clarify

Correct, **I rephrased the sentence (line 54).**

Line 77: nearest-neighbor connections: does this imply only cardinal cells on a square lattice?

I am not familiar with the definition of cardinal cells, but the term nearest neighbor is widely used in this context.

Line 96: The overall behavior of the DS FFM simulations is compelling, but I am wondering (i) why the excess occurs at 1e5 and (ii) why the rapid decline and deviation from the power law scaling relationship occurs after. I do not agree it is not relevant here. The fact that this figure will be published warrants explanation for this deviation, regardless of whether it is relevant for the subsequent rockfall analysis. Has this to do with the dimensions of the grid or boundary conditions?

Neither of both. The rapid decline is due to the finite growth rate, while the grid is large enough to have no effect. The bump in the distribution is related to the shape of large clusters. Imagine that clusters grow by adding indivudual trees at the boundary, but also by connecting two clusters. If the latter process becomes dominant, the shape of the clusters changes. However, this is specific to the DS-FFM. **I added a short remark (lines 98–100),** but readers who want to understand this effect need to read my old 2011 paper.

Line 110: describe λ (add symbol in sentence above)	**Ok (line 142).**
Line 113: here and at several points: it's always better to guide the reader a little through the math. Makes the manuscript way more accessible. Here e.g. mention integration and boundary conditions to solve eq. 3	I agree to some extent in general. However, readers without any basic knowledge in calculus and statistics will have no chance anyway. In this example, I think that it would not be useful to derive the exponential solution in detail from the decay equation.
Line 118: Not clear how this is a cumulative formulation. In the given formulation, ψ declines for increasing values for s. For a cumulative pareto function, I would expect something of the form $1-(s_{\max}/s)^{\alpha}$. Explain this better. Also, for these and following variables mention realistic parameter value ranges (in the context of rockfall)	Right, but in the context of hazard etc., the complementary cumulative distribution is widely used. **I added a short note (lines 155–158).**
Line 120: definition n is not clear	Would it make sense to define explicitly that n is a factor and explain its meaning two lines after?
Line 151: "μ (Eq. 1) is the decay constant" This is not clear.	**I explained it more explicitly (lines 191–192).**
Line 154: What should that be expected? *Line 155-156: This should be derived and explained much clearer. It is not clear at all to me why this results in respectively 2/3 and 1/3.*	**I expanded the text a bit (lines 193–199).** Hopefully, it is clearer now in combination with the new Sect. 2.2.
Line 162: This is almost certainly an incomplete dataset. How is this influencing the calibration of the model?	**I expanded the discussion about the uncertainties (lines 258–271 and 303–330).**
Line 178: The derivation of the maximum likelihood is essential to understand figure 4. I recommend including the appendix in the main manuscript.	No, a qualitative general understanding of the maximum likelihood method is sufficient for understanding Fig. 4. I agree that this is challenging for readers without any knowledge on the maximum likelihood method. **So I added a short explanation how to set up the maximum likelihood approach in the main text (lines 222–227)**. The expressions for the likelihood developed in the appendix are only important for readers who want to apply the method to another data set, but would distract the reader here.
Also on Figure 4: α_v is a function γ (eq. 15). It is unclear how α_v and gamma are plotted as (independent?) variables. How is γ varied for the same value of $alpha_v$? By changing α?	Yes, 2 out of 3 parameters can be considered independent, and the third can be computed from the two others. **I added a note (lines 241–242).**
Line 319: why is the likelihood given by the Poisson distribution?	Just because practically all results of counting random events follow a Poisson distribution. **Hopefully, this becomes clearer with the additional text (lines 222–227 and 440).**

Line 192: not clear how the t_0 lines are obtained. More detail is highly needed here.	**I added some explanation (lines 243–246).**
Line 214: again, does gamma depend on α_v (eq 15?)	See comment above.
Line 216: yet another distribution. Explain why this distribution is used. Also explain the symbols... *Lines 216–222: I do not understand what the author does here. Please break this down into clear and understandable pieces, referring to the symbols used in the distribution equation.*	Indeed one more distribution, but still less different distributions than figures. **I added a short derivation of the equation (lines 287–296)** for those readers who are not familiar with extreme value statistics. By the way, it should have been $\Phi(V,t)$ instead of $F(V,t)$.
Line 234-235: that is, if this model that is calibrated with relatively few datapoints is true.	**I added "according to this framework" (line 335).**
Line 249: Despite	**Fixed (line 351)** – thanks!
Line 250: See earlier comments on line 155	Hopefully clearer with the additional explanation **(lines 193–199 and 349–354)**.
Line 254: This is interesting. I recommend giving an explicit example where the author's earlier modelling work is evaluated using this theoretical framework e.g. for one specific site.	**I introduced a new section (2.2) about this old model and some more discussion here (lines 355–367).** However, analyzing this model in detail with regard to the concept of exhaustion would go beyond the scope of this paper.
Line 266: True: which is why a clear explanation on how the t_0 lines were derived is needed.	**I added some explanation (lines 243–246).**
Line 275: is this because the topographic configuration does not allow for large rockfalls to materialize?	Right – **I added some explanation (lines 386–394).**
Line 298: I highly recommend illustrating this with a specific example, see also comment above.	But rather not here in the conclusion.