Reviewer comments in black

Author response in blue

“text changes”

Anonymous Referee #1

General comments:

Understanding the mechanism of long run-out of a landslide/avalanche is still a great challenge, but plays an important role in correctly describing the landslide dynamics and its deposition morphology. Models and hypotheses have been presented to explain the exceptional run-out of landslide, including fragmentation. The authors mention, that fragmentation may consume energy, potentially at a cost of the runout length. So, they are concerned to specify the integrated effect of fragmentation on the runout dynamics of rock avalanches. By analysing analogue models of dynamically fragmenting rock slides, the authors isolate the effects of friction and fragmentation on rock avalanche. For this, they assume that there exists some mechanism that causes a low, but constant effective basal friction. So, the variation in run-out or mobility (called Heim’s ratio) is assumed to depend only on fragmentation. This is summarized by presenting a scaling law which shows that the change in the degree of fragmentation can explain the large variation in runout of rock avalanches seen in nature. The authors also compare their experimental results to a set of data (that also utilizes date from some internal report) and discuss their relevance to natural systems.

The topic is very important and interesting. The presented mathematical model is one of the main contributions in this paper manuscript that may play a crucial role in describing runout of a landslide. There are some appreciable, clever and novel ideas, and important observations.

However, there are also several critical issues on the presented model and other conceptual aspects that must be addressed properly. This mainly concerns the presented mathematical model and explaining the observed results with underlying mechanics. Parameters should be well defined. The paper could have been better organized and discussed.

We thank the reviewer for this positive view. We revised the math section for clarity and discussed the critical points raised now more specifically.

Specific comments:

Some confusions are already seen in the Abstract: Usually, exceptionally long runout is associated with the large volume that results, e.g., by some fluidization/lubrication effects. This has been discussed by presenting a mechanical analytical model in https://doi.org/10.1016/j.enggeo.2013.01.012. These relevant aspects should have been discussed.

We agree that weakening processes involving fluids are of prime importance. However, the experiments aimed at isolating the effect of fragmentation and having all other parameters as constant as possible. Amongst those constant parameters in the experiment is the friction coefficient which Pudasaini and Miller (2013) argue is scale-dependent and controlled by fluidization, i.e. pore fluid pressure. Accordingly, this is especially significant in submarine environments while in dry subaerial, terrestrial, non-volcanic environments that we focus on the friction coefficient seems less affected by fluidization. For example, looking at the data shown in Pudasaini and Miller (2013) for “non-volcanic events” it seems this subset (which is the one relevant for us) shows the smallest range in friction coefficient and least sensitivity to volume compared to others. The data and model have a plateau in the volume midrange suggesting insensitivity for at least some part of the spectrum. At the same time, it is this part that shows the greatest variability for a given volume in the whole data set (suggesting additional, likely similar important controlling factors). So, while we appreciate the validity of their model over a wide range of sizes and environments, it seems that at least in those environments that we focus on, additional controlling factors are at least equally important.

We discuss this limitation/focus of our study now more specifically in the introduction:

“One of the best established, but perhaps least understood observations of rock avalanches, is the dependence of the Heim's ratio on volume: rockslides below a size of approximately 10^6\,m^3 all have a relatively constant Heim's ratio of $\sim$0.4-0.7, but for larger rockslides it decreases with volume, reaching values $<$0.1 for volumes larger than 10^9 m^3 \citep{Pudasaini2013,Lucas2014}. This suggests a scale-dependent mechanism of decreasing apparent friction with volume that becomes dominant at large volumes \citep{Davies1999runout}. Analytical modelling and numerical simulation involving lubrication mechanisms by \cite{Pudasaini2013} and \citet{Lucas2014} provided mechanical explanations for this observation. Importantly, however, even within a narrow range of volumes, runouts are seen to span orders of magnitude suggesting additional controlling factors on runout that are insensitive to size.”

L12: 150 km/h is not that high for rapid avalanche with exceptional run-out.

We found it difficult to constrain since direct observations have been rare. We include now also the 100m/s used as an upper bound for the modelling in Legros (2002):

“With volumes larger than 10^6\,m^3, and speeds reported at over 150\,km/h \citep{Campbell1989} and possibly up to 100m/s \citep{Legros2002}, the destructive power of rock avalanches is unprecedented.”

L17,22: Although friction is assumed to be low and constant in this paper, the above mentioned reference resented the first-ever explicit and unified theoretical model for exceptional mobility of landslide and avalanche: with the consideration of volumetric, physical, and topographical parameters, the authors presented a new model to quantify the scale-dependent friction coefficient of large debris avalanche events. It might be relevant to discuss.

We mention this model now more specifically (see reply above). However, we here focus on the scale-independent effects seen (for example in the data set used by Pudasaini and Miller (2013):

“Importantly, however, even within a narrow range of volumes, runouts are seen to span orders of magnitude suggesting additional controlling factors on runout that are insensitive to size.”

we further specify this limited scope:

“To isolate the scale-independent effect of fragmentation we keep both the volume and friction within a narrow range in our models compared to nature. Note, this approach explicitly excludes dynamic weakening mechanisms that are suspected in natural prototypes. Specifically, our models do not include fluids and frictional heating is insignificant such that lubrication mechanisms \citep[e.g.][]{Pudasaini2013,Lucas2014} do not play a role. Granular pressurization \citep[e.g.][]{Imre2010} is also not considered significant in our experiments because of the low energy involved. Other potentially important mechanisms like bedrock erosion \citep[e.g.][]{Hungr2004,Pudasaini2020} are excluded here for simplicity. The experimental design, therefore, means that the observed variation in Heim's ratio is due to fragmentation and dry friction.“

“...our results suggest that the variation seen in Heim's ratio for these rock avalanches are not (only) caused by scale-dependent basal friction, but by differing degrees of fragmentation.”

L27: “additional controlling factors”: One such very important, dominant factor is erosion/entrainment that explains the mechanical causes of exceptional long travel distance. This is worth mentioning with reference.

We agree that erosion is an important mechanism lowering apparent basal friction. We now included it with reference to https://doi.org/10.1130/B25362.1 and https://doi.org/10.1016/j.ijmultiphaseflow.2020.103416 in the first paragraph.

“This suggests low effective basal frictions μ_{eff}, which is usually attributed to various dynamic weakening processes \citep[e.g.][]{Kent1966,Shreve1968,HSU1975,Melosh1979,Campbell1989,Pudasaini2013,Legros2002,Lucas2014,Wang2017} or additional basal erosion processes \citep[e.g.][]{Hungr2004,Pudasaini2020}.”

However, our models do not include basdal erosion so we added it to the list of limitations/simplifications of our models:

“... Specifically, our models do not include fluids and frictional heating is insignificant such that lubrication mechanisms \citep[e.g.][]{Pudasaini2013,Lucas2014} do not play a role. Granular pressurization \citep[e.g.][]{Imre2010} is also considered not significant in our experiments because of the low energy involved. Other potentially important mechanisms like bedrock erosion \citep[e.g.][]{Hungr2004,Pudasaini2020} are excluded here for simplicity.“

L41-42: “We assume that there exists some mechanism that causes a low, but constant effective coefficient of basal friction and keep it constant in our model.”: This is a clever idea, but is this realistic and observable in nature? Please elaborate with reference.

A constant friction coefficient at natural scale is likely not realistic while in the lab it is (we tested the rate and state dependency of sands used here and found no significant weakening). Since we reduce natural complexity in our analogue model this assumption is part of our strategy to isolate the fragmentation effect. In summary of the above comments and replies, we would like to keep the list of potential weakening mechanisms short as our models do not include most and are intentionally simplified and necessarily limited. We specify our approach:

“To isolate the scale-independent effect of fragmentation we keep both the volume and friction within a narrow range in our models compared to nature. Note, this approach explicitly excludes dynamic weakening mechanisms that are suspected in natural prototypes. Specifically, our models do not include fluids and frictional heating is insignificant such that lubrication mechanisms \citep[e.g.][]{Pudasaini2013,Lucas2014} do not play a role. Granular pressurization \citep[e.g.][]{Imre2010} is also not considered significant in our experiments because of the low energy involved. Other potentially important mechanisms like bedrock erosion \citep[e.g.][]{Hungr2004,Pudasaini2020} are excluded here for simplicity. The experimental design, therefore, means that the observed variation in Heim's ratio is due to fragmentation and dry friction.”

L58-59: The normalization is a bit strange and not justified! E.g., why the length L_spread is normalized by the vertical fall height H and not by other more relevant length scale such as l_0?

We agree that L_spread/l_0 is an intuitive ratio and describes the extension of the slide. On the other hand, L_spread is similarly correlated to H as L is and so both are at first order describing the conversion of potential energy into translation and deformation, respectively. Moreover, this ratio drops out of our scaling law (eq. 5) suggesting it has a physical meaning beyond the purely geometric meaning of L_spread/l_0 (extension).

We specified this choice now:

“We normalize L_{spread} by fall height H to have a parameter describing the conversion of potential energy into spreading equivalent to Heim's ratio.”

Also, the definition of degree of fragmentation m_c is strange and not discussed why done this way: there can be very few fragmented big boulders and almost all small particles. Then, defining m_c in terms of m_max may not be the best representative of the fragmentation. This should be discussed.

It is true that in models we could derive statistically more quantitative parameters describing the amount of fragmentation, e.g. based on the full fragment size distribution. However, we here explicitly aim at a proxy for the fragmented volume which is accessible and easy to derive in nature, too. It is therefore a trade-off between capturing the process accurately in models and accessibility of the information in nature (and from literature). Concerning energy consumption due to fragmentation we feel it is intuitive that the largest fragment, which remains intact and controls the complementary fragmented volume, is a valid first-order proxy for the energy consumed by fragmentation. In Haug et al. (2016), we verified the usefulness of m_c by benchmarking it against the breakage parameter used in previous studies (e.g., Bowman et al., 2012; Langlois et al., 2015).

We clarify this by saying:

“We choose this rather simple parameter, which has been validated and benchmarked against breakage parameters used by previous studies in \citet{Haug2016}, as a tradeoff between capturing the process accurately in models and accessibility of the information in nature.”

L63-71: The readers might ask why these parameter values are chosen.

These were experimental constraints. In analogue modelling, we are limited in the parameter space and try to extend it as much as possible with the materials we have which may result in odd numbers. For clarification we added:

“Combining these sets of data from various experiments allows for covering a wide enough parameter space for the analysis in this study.”

L74-78: not easy to follow. Not clear which initial conditions are used.

We rephrase:

“To quantitatively analyse the experiments we focus on the correlation between runout and fragmentation and neglect all other parameters. This is justified by the collapse of experimental and natural data in Figure~\ref{fig:f03}a: When plotting the Heim's ratio against fragmentation (m_c), all data collapse to the same trend and therefore no distinction is made between the experimental data in this figure. Qualitatively, Heim's ratio decreases rapidly for low to intermediate degrees of fragmentation, reaching a minimum at $m_c \approx 5$ of about 0.2 and increases again slightly for higher degrees of fragmentation. A similar relation is observed between the length of the deposits (Figure~\ref{fig:f03}b), which increases with fragmentation until $m_c \approx 5$ and slightly decreases beyond.”

Fig. 2: Figures could be better organized, e.g., by first putting Fig. 3 then Fig. 2; first present model then Fig. 2, etc.

OK, we followed this suggestion and switched figure 2 and 3.

The strange behaviors of increasing H/L and L_spread/H with large m_c must be clearly discussed.

The trends seen in these two plots of now Fig 3 are clearly correlated and suggest an intrinsic relation between spreading and runout: Below m_c=5 the increase in L_spread causes H/L to decrease while above that threshold the spreading decreases and H/L increases consistently suggesting energy consumption.

We specified in the figure caption:

“Note the opposite trends of the two curves suggesting an intrinsic relationship between spreading and runout.”

We hope the shape and relation of these two curves become clearer with the revised structure and wording in section 3.1.

Is this so great to mention about the plotting script in the caption?

OK, we omitted this.

L79-82, 85-86: Very interesting/important, novel observation, but the writing should be improved. E.g., does it mean fragmentation results in decreased runout?

For larger degrees of fragmentation this is correct (m_c>5) because fragmentation consumes energy (Haug et al. 2016 and references therein). Below m_c=5 fragmentation seems to increase mobility and runout.

We clarify now and also include deposition:

“The stronger sample (Figure~\ref{fig:f02}A) is observed to fragment less than the weaker one (Figure~\ref{fig:f02}B). Thereafter, fragments of the stronger sample spread with limited interaction while the fragments from the weaker sample collide and/or slide next to each other and deposition starts relatively early. We infer, at first order, that while mobility generally increases with fragmentation, a higher amount of internal deformation is experienced along with increased fragmentation and increased deposition.”

L90-92: Appreciable novel observations! However, not quite clear what you really want to say. You have not yet clearly quantified the internal friction and interactions between the fragmented particles.

The fragmentation effect on runout for larger fragmentation degrees (energy sink) has been described in more detail in Haug et al. 2016 and we here take up their findings. A more quantitative analysis follows in section 3.2.

We have re-phrased that part “preparing” the quantitative section for clarity also including the effect of deposition and loss of momentum:

“Considering the increased internal deformation observed with the degree of fragmentation (Figure~\ref{fig:f02}), the reduction of runout with $m_c>5$ appears to be the result of the increased energy dissipation through internal friction within the rock mass as well as an increase in basal friction as the sliding surface becomes rougher due to syn-sliding deposition \citep[e.g.][]{Pudasaini2020}. A loss of mass and therefore momentum due to deposition may additionally result in deceleration and reduced runout as a function of m_c \citep[e.g.][]{Pudasaini2020}. Consequently, the minimum of the Heim's ratio observed in Figure~\ref{fig:f03}a appears as the result of a competition between spreading enhancing mobility and the energy-consuming fragmentation process.”

The energy dissipation is also due to loss of momentum because of the early depositions of (many small) fragments. Such a reduction in mobility due to deposition has recently been explained with the mechanical erosion model for mass flows.

That’s a good point. Thank you for emphasizing this. We included this now specifically:

“Considering the increased internal deformation observed with the degree of fragmentation (Figure~\ref{fig:f02}), the reduction of runout with $m_c>5$ appears to be the result of the increased energy dissipation through internal friction within the rock mass as well as an increase in basal friction as the sliding surface becomes rougher due to syn-sliding deposition \citep[e.g.][]{Pudasaini2020}. A loss of mass and therefore momentum due to deposition may additionally result in deceleration and reduced runout as a function of m_c \citep[e.g.][]{Pudasaini2020}.”

L97-118: The following are critical issues that must be properly addressed. There are two main essences of this paper. (i) fragmentation experiments and the analysis of the data, and

(ii) developing a mathematical model explaining the runout in terms of fragmentation intensity. I hope the models and the associated figures are right. However, the authors must fix the following:

Readers can’t follow, please derive equation (3) e.g., in and Appendix.

We added a new figure A01 in the appendix where all the distances used to derive eq. 3 are defined.

Equation (4) might not be right as it appears now; you need 1/Mg in W, or?

That is true, thanks for pointing us to this error.

L_spread and W are assumed to be implicit functions of frictions and fragmentation, that might be reasonable, but are not quantified.

True, these are reasonable assumptions that we do not quantify. Likely a dedicated study would be needed but this would be beyond the scope of this “Short communication”.

Equations (5), (7) and can’t be obtained in a usual way, please check and prove.

We checked the math and corrected it where necessary. Eq. (5) follows in a step of rearrangement from eq. (4) while the derivation of eq. (7) (now eq. (8)!) is now presented in one step more as resulting from inserting (6) and (7) into (5). We hope the revised math section is clearer now.

Further, why do you have mu inside mu?

We think there is a misunderstanding: The bracket indicates a factor term, not a function variable. Mu appears at two positions in that product.

Also, the logarithmic dependency of the work W on fragmentation m_c is not clear, must be discussed.

This is an inference based on the trend shown in Fig. 8b in Haug et al. (2016). We now specified and justified it better:

“...the experimental work by \citet{Haug2016} suggests that dissipative energy loss through fragmentation increases less for higher degrees of fragmentation and therefore can be described with a logarithmic function of m_c:”

Please check carefully and derive the model equations explicitly, may be in and Appendix. Equations (6), (7): m_c > 1, by definition, and also alpha > 0, then – alpha ln(m_c) < 0, means L_spreading <0? This is not realistic.

We realize that the order of terms on both sides of the formula was switched which is mathematically not relevant but from which confusion may have arisen. We now treat and explain the two terms separately and correct the signs.

So, please derive all the equations such that the readers can easily follow and understand the mechanisms behind them.

We revised this section for clarity, corrected minor errors and decluttered the math. We hope it is clearer now.

Technical comments:

The English should be improved (e.g., L2, L14, L41, . . .).

OK, the revised version finally has been checked by Jon Bedford (native speaker).

Notations should be clearly defined (e.g., L20, what ap stands for, . . .)

True, that was not clear. “ap” stands for “apparent”, we clarified this now:

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“The resulting ratio
\begin{linenomath*}
\begin{equation}
   \mu_{apparent} = \frac{H}{L}
\label{eq:Heim}
\end{equation}
\end{linenomath*}
is known as the Heim's ratio \citep[as cited in \citealp{HSU1975}]{heim1882bergsturz} and serves as a proxy for
$\mu_{eff}$ when called the "apparent" coefficient of friction \citep{Manzella2012}.”
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L23: Heim’s ratio can be much smaller than 0.1.

True, but for those terrestrial, non-volcanic cases we focus on 0.1 appears as a lower bound (e.g. https://doi.org/10.1038/ncomms4417, https://doi.org/10.1016/j.enggeo.2013.01.012)

Fig. 1: Caption: would be better to replace “measurements” by “scales”?

OK, we modified:

“sketch of the slope geometry of experiments, relevant parameters and length scales...”

Fig. 3: Why not the same times for panels on both columns? It is difficult to compare.

The time indicates the time since the first impact while we have chosen increments of equal travel distance. We explain this more specifically now:

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“\caption{\textbf{Snapshots from the experiments}: (a) intermediate strength sample ($C = 40$\,kPa) and (b) low
strength sample ($C = 4$\,kPa).The red lines in the upper images indicate the geometry of the basal plates.
Images are chosen to represent similar travel distances in (a) and (b). The time given above each image reflects
the time since the first impact. ...“
```

Also, put scales in x and y axes, and c = ** on top of the columns.

We added a scale bar and indicated the cohesion as suggested.

L97, 100: Parameters are not well defined. E.g., what is L_s, which length? Please

clearly define all parameters and show in the figure.

L_s is the length of the slope. We specified it in the text and in a new figure A01 which illustrates how the equation is derived.