Reply to Reviewer #2

We would like to thank Christoph Mitterer for his helpful and detailed review of our manuscript.

Summary

The authors present to my knowledge one of the few mechanical-based model approaches to better understand the release behavior of glide-snow avalanches. The basic concept of the model approach relies on a self-organized criticality (SOC) approach, which has been applied to various other, similar rapid gravitational mass movements. Since our knowledge on release of glide-snow avalanches is notoriously limited, the authors are forced to make various assumptions and parameterizations. All of them are valid and plausible. The modelling approach is two-fold. First, the authors apply their model assumptions and governing equations to a simplified, uniform slope and perform a sensitivity analysis. Then, they apply the model to a complex topography and compare the results to observed data from the test site Dorfberg above Davos, Switzerland. The model results reveal interesting insights into the components that are relevant to the release of glide-snow avalanches, but more importantly sets the stage for further more detailed investigations using the presented approach.

Evaluation

The presented manuscript applies transparently a sound set of methods to obtain innovative results on the mechanical processes relevant to glide-snow avalanche release. Approach and results are scientifically relevant and represent a major impact on that specific topic for the community.

The manuscript is concise, well-structured and nicely written. The reader can easily follow the thoughts and approaches of the authors. Figures and tables are clearly structured and adequately described. I am convinced that this excellent work should be published on NHESS after addresses some additional points.

General remarks

In general, I miss a little more discussion and context to previous studies, especially on those that already pointed to some drivers that may be very important for the release of glide-snow avalanches. I think by adding some more details and discussion, the manuscript would highly gain impact especially on narrowing down some of the processes relevant for glide-snow avalanche release.

Here are some of my thoughts:

- You compare the model results to observed glide-snow avalanche data without stratifying according to surface vs. interface events (cp. Fees et al., 2023). Are there differences in the observed x_{min} and alpha when accounting for the different events?

We investigated the power law exponent and x_{min} separated into surface and interface events (Figure 1) and observed that interface events showed a larger power law exponent than surface events. However, as the other reviewer also pointed out, the available data for large avalanche events is currently very limited. We will point out the difference in the power law exponent between interface and surface events in the discussion as an indication for further research. We will also put this 'preliminary' finding in context of the currently limited data availability.

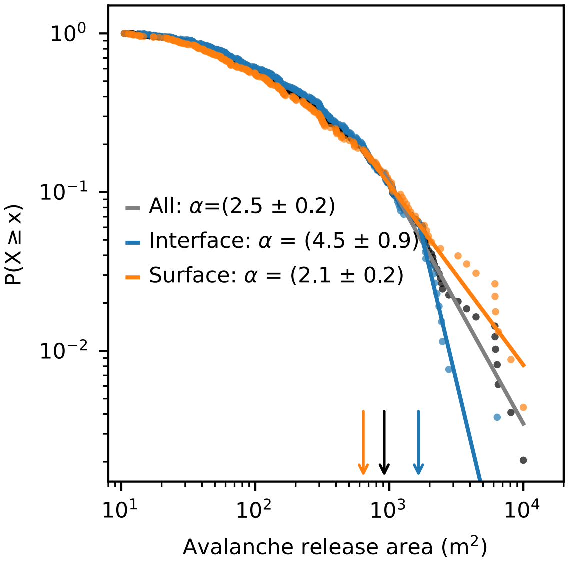

Figure 1: Dorfberg avalanche release area distribution separated in surface and interface events (points) and the corresponding power law fit (line). The arrows indicate x_{min}.

- While you nicely show that snow density and snow height do not have large impact on especially alpha, I was wondering if changes in snow height, e.g. amount of new snow would lead to more sensitive reactions of the model. This in turn would be interesting since especially interface events (formally also called cold-temperature events) seem to highly react on added mass of snow (Dreier et al., 2016).

At the moment the model is based on the assumption that a spatially uniform reduction in basal friction drives the instability of the slope. If we assume a model setup where the basal friction distribution does not decrease (but is in a potentially critical state) and uniform snow loading drives the model we would expect that the snow loading has a similar influence on the release area distribution as the 'basal friction reduction step size' (Manuscript Figure 6c). Both the reduction in basal friction and snow loading would be a spatially uniform contribution towards instability. To investigate the simultaneous basal friction decrease and snow loading the snow loading would have to be implemented in the model. This would be an interesting step in further model development.

- On the other side you show that variance and correlation length of the basal friction have large impact on the power law fits. Both results (not sensitive to snow height, sensitive to the correlation length of the basal friction) were already mentioned in Bartelt et al. (2012). I think the community would highly benefit, if you could discuss in more detail where your and the results by Bartelt et al. (2012) show similar and/or different behaviour and why and where assumptions of both models may have contributed to the agreement or differences.

Thanks for this insightful comment. We will refer to the stauchwall model in the discussion of the results to highlight similarities and differences. In the 'model potential' section we will point out that our model can help to narrow down the length scales of the gliding zone which is one of the major assumptions in the stauchwall model.

Specific remarks

- Lines 124-125: The term "stable state" might be a bit misleading here.

  We will clarify the definition of "stable state".

- Table 1: Why did you use 30 simulation runs and are the number of runs relevant for the results?

  We chose 30 simulation runs because, on average, this resulted in a number of simulated avalanches in the order of magnitude comparable to the Dorfberg field observations. The aim was to keep the modeled and observed distribution comparable. We did an exemplary study on the baseline simulation to determine if the number of simulations influences the power law exponent. We found that more simulations did not influence α substantially. We will address this in a boundary conditions section in the Appendix (Figure 1a, more details are provided in reply to the first reviewer).

- Line 231-232: Can you underpin a little more your statement: "For avalanches in early winter ...".

  We will extend the discussion by including the following statements: "For avalanches in early winter, this assumption may be less appropriate. We suspect that the interfacial water in early winter originates from geothermal heat melting the snow at the bottom of the snow cover or from capillary suction of water from the soil into the snowpack. Both processes would allow for local increases in interfacial water due to, for example, locally higher soil temperature or soil saturation."

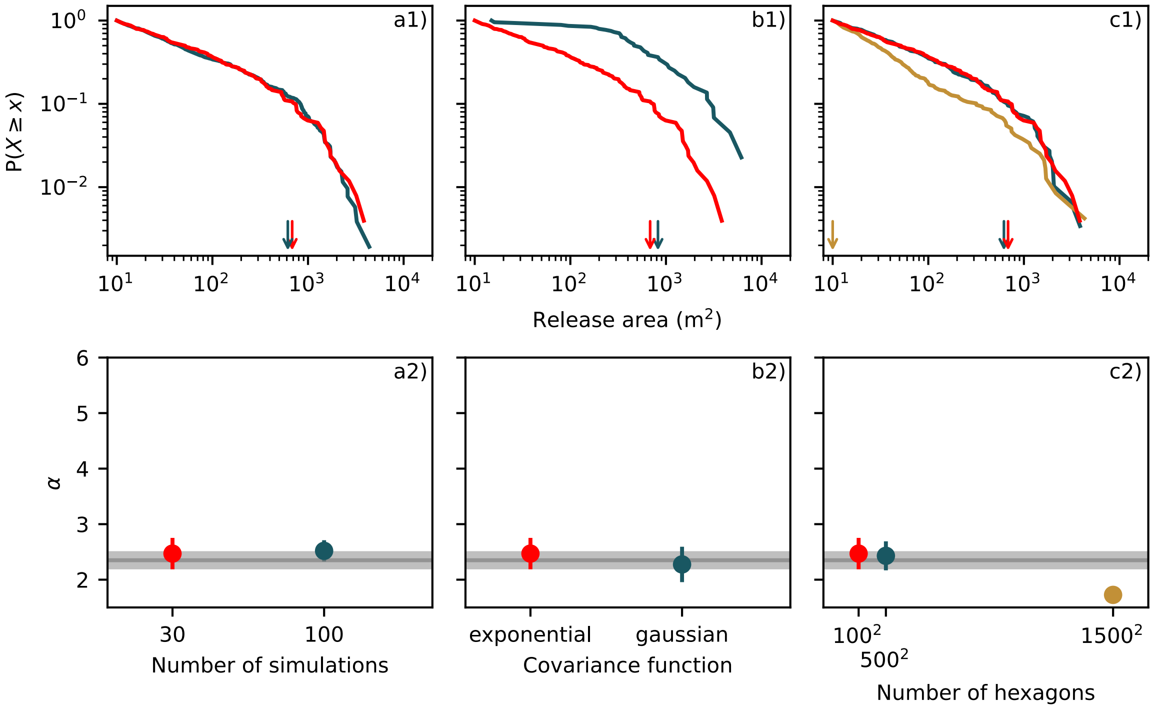

Figure 1: _1) Release area distribution for different boundary conditions of the model – a) the number of simulations, b) the covariance function used in the GRF, and c) the number of hexagons in the simulation domain compared to the baseline model (red). _2) The power law exponent α in comparison to the Dorfberg exponent and fit uncertainty (gray). The error bars indicate the fit uncertainty.

Literature

Bartelt, P., Feistl, T., Bühler, Y., and Buser, O.: Overcoming the stauchwall: Viscoelastic stress redistribution and the start of full-depth gliding snow avalanches, Geophys. Res. Lett., 39, 2012GL052479, https://doi.org/10.1029/2012GL052479, 2012.

Dreier, L., Harvey, S., van Herwijnen, A., and Mitterer, C.: Relating meteorological parameters to glide-snow avalanche activity, Cold Reg. Sci. Technol., 128, 57–68, https://doi.org/10.1016/j.coldregions.2016.05.003, 2016.

Fees, A., Van Herwijnen, A., Altenbach, M., Lombardo, M., and Schweizer, J.: Glide-snow avalanche characteristics at different timescales extracted from time-lapse photography, Ann. Glaciol., 1–12, https://doi.org/10.1017/aog.2023.37, 2023.