Response to Reviewers
Elizabeth Fischer
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Reviewer 2

This review shows all the hallmarks of being AI-generated, contravening the Copernicus AI Policy. As such, it has limited utility. The review was long, repetitive and pedantic; and in many places demanded lists of things that sound reasonable on the surface but do not make sense. Those lists would repeat from section to section with minor and seemingly random variations. Below we respond to the comments that made sense and were not already covered by a human reviewer; and we ignore the rest.

> the treatment of 30- and 300-year scenarios is not probabilistically robust;
We do not apply a probabilistic approach but the well established scenario based approach developed in Switzerland and applied in most mountain countries for hazard mapping.

> Thus, the study framework is internationally relevant, but the current implementation and interpretation require strengthening before the results can fully satisfy international standards of quantitative hazard assessment.

The current implementation and interpretation follow well established studies previously published in Switzerland.

> The treatment of Deciduous, Mixed and Evergreen Forest and the use of a 30% vegetation threshold for filtering low-elevation avalanches are insufficiently justified quantitatively.

We already explained in the paper, but here is more detail:

As for the 30% filter: Low elevation avalanches cannot gain enough speed to take out trees, even with large PRAs, that is the physics intuition. On the empirical side, when we look at the

satellite images we see that low elevation avlanche tracks are connected to high elevation PRAs, not low elevation. Large avalanches have already removed trees from the landscape where they will happen: tree cover ==> evidence of no large avalanches there. Additionally, we already know where the avalanche hazards are and are not in Juneau: in general, built-up areas are not at risk of avalanches, except Behreds Ave; and we know where the avalanches happen on the roads. Existence of buildings (outsides of Behrends Ave) is also strong indication in Juneau of low avalanche risk.

So it just makes no sense to have PRAs and avalanche tracks at low elevation containing forested areas. The 30% number is somewhat arbitrary, but it allows for small amounts of "noise," and it filters out the avalanches in a way that matches observations.

> The broad workflow is reproducible conceptually, but full computational reproducibility is limited.
>
> The manuscript states that the complete workflow requires multiple operating systems and proprietary software, including ArcGIS, eCognition and IDL.

This was not our choice, and it is the same as similar studies published in the past. All software we wrote for this study is in Python and is freely available.

> The Code Availability section indicates that only portions of the code are publicly available.

RAMMS is not publicly available, we do not control that. Other academic researchers can request access to RAMMS as needed. Now there would be AVAFRAME (https://docs.avaframe.org/en/latest/) as an open source alternative, but many questions remain and RAMMS is well established

> For adequate traceability and reproducibility, the authors should provide:

> exact version of RAMMS::LSHIM;

Unfortunately there is no such version number. We do have the exact source code we are using, but are not legally allowed to distribute it.

> WRF variable names and preprocessing steps;
> all scripts required to reproduce the snowfall downscaling;
> forest-mask generation rules;
> configuration files;
> ideally, representative complete input/output examples.

These items are all in the publicly available source code. We have been appropriately judicious regarding which details of our process (of which there are many) we highlight in the paper.

> exact friction parameterisation for each PRA class;

This is part of RAMMS, and is explained in detail in the Bühler papers we referenced.

> exact software versions;

IDL 9. (We started with 8, then IDL moved us to 9, it made no difference)
eCognition Server 10.3
ArcGIS Pro 3.5.1
Conda YAML files for the Python environment are provided as well.

> The manuscript reports typical snow depths around Juneau as approximately “0.7 m² and 1.2 m²”. These should presumably be m, not m².

Typo fixed, thank you.

> The avalanche extent criterion appears in the text as velocity “at least 1 m”, whereas the intended unit is presumably: . This should be checked systematically.

Typo fixed, thank you.

> 11.3 Lapse-rate units The manuscript alternates between:

We standardized on m km-1

> 11.4 “Snowfall” versus “snow accumulation/depth”
> These terms are occasionally used interchangeably. Snowfall amount, snow-water equivalent, settled snow depth and accumulated snow depth are not equivalent quantities.

We mean snowfall.

> Equation (1), deriving the lapse rate from spatial gradients, should include a clearer explanation of:

> sign convention;

Sign convention is apparent from reading Eq 1, but also not relevant in an abstract equation like that. Added a sign convention note to Figure 4.

> physical interpretation;

"Following Qin et al. (2018), we computed the gradients of temperature T and elevation H by..."

> treatment of flat terrain;

No such special treatment

> numerical handling where approaches zero.

We did not have that problem, the results do not show things blowing up in lower-relief terrain such as in BC behind the coastal mountains.

> Figure 8 - The caption contains inconsistent historical and future periods. This must be corrected.

Thank you, fixed.

> Figure 14 - The caption apparently describes both low- and high-pressure areas as blue.

Thank you, caption fixed.

> The principal avalanche-modelling literature is appropriately represented. However, additional literature is needed on:
>
> uncertainty and sensitivity analysis in avalanche modelling;
> extreme-value estimation of snowfall;
> return-period estimation;
> climate-model uncertainty propagation;
> forest ecology of Southeast Alaska;
> possibly more recent work on avalanche response to climate change.
> The bibliography is therefore adequate in quantity but somewhat uneven in thematic coverage.

We provided references for the methodology of the work that we did. As the reviewer correctly pointed out, we re-used methodology for RAMMS, PRA delineation and hazard-indication workflows, which we referenced. Those papers reference additional topics in this list as needed. This paper focuses on our novel work, rather than re-hash of prior art, and it is certainly not a review paper of avalanche modeling literature.

> “Thane Red” instead of Thane Road;
> “estimaation”;
> “within a call” instead of within a cell;

> “interprested”;
> “comptuer”;
> inconsistent use of NLCD/NCLD;
> “deliminate” instead of delineate.

Thank you, fixed.

> The following require clarification:
>
> derivation of the 30- and 300-year scenarios;

Same methodology as Bühler et al, which is documented in that paper.

> relationship between PRA size and recurrence interval;

All recurrence intervals generate all PRA sizes.

> treatment of Mixed Forest;
> rationale for the 300 m elevation threshold;

"Areas classified as Mixed Forest are harder to characterize. We chose to not include them in the forest mask under the initial assumption that they do not offer significant avalanche protection. This results in more low elevation PRAs Ð those below the tree line Ð since most non-evergreen forest occurs at low elevations. This choice is appropriate since additional steps are taken to cull unrealistic low elevation avalanches (Section 2.11). Note that most avalanche hazard occurs from PRAs initiating avalanches above the tree line, where they can develop long runout paths, high velocities and destructive forces."

A couple paragraphs above, we explain the shortcomings of the NLCD data. Bühler et al have access to more detailed vegetation data in Switzerland, and they are able to do more interesting things with it.

The rationale for 300m elevation threshold is given above: "low elevation PRAs --- those below the tree line." Thus, the observation is that in this part of Alaska, the tree line is at about 300m.

> rationale for the 30% land-cover threshold;

See above. Ultimately, it was tuned to match the validation data.

> interpretation of the future climate scenario.

"These trends are not spatially uniform due to regional variations in projected snowfall. We expect lower elevations will experience more rain and less snow, hence a decline in maximum 3-day snowfall in most parts of the region. However, high elevations in areas of northern Southeast Alaska create conditions where the increased winter precipitation more than offsets the shift to rain, and therefore maximum 3-day snowfall increases (Figure 19a). These precipitation changes are the main driver of changes in avalanche risk (Figure 19b), and overall avalanche risk is expected to decrease."

> The term “buildable zone” is also overly normative. Elevation below 40 m does not necessarily imply that land is suitable for development. It would be better to use “low-elevation zone or development-relevant lowland zone instead.

...all of which are more wordy, less specific or both than "buildable zone," where we define exactly what we mean, even putting "buildable zone" in quotes. It is clear from the context that we are not making any claims that all of Juneau's land below 40m is suitable for development:

> "We define two primary land-use categories: the 'buildable zone' as land below \SI{40}{\meter}, where the vast majority of Juneau’s structures are located,"

> PRA thresholds;

The paper explains how PRAs are separated into size classes.

> possibly tabulated regional statistics;

Statistics are given in Section 3.2. No further tabulation is needed.

> The paper labels scenarios as corresponding to 30- and 300-year recurrence intervals, yet the forcing relies substantially on maxima derived from 30-year climate periods. The authors themselves recognise that such values may underestimate 100- and 300-year extremes and that Extreme Value Theory would provide a more rigorous framework.

...and we explain in Section 3.4 why a more rigorous framework was not expected to provide a more useful result in this case...

> If probabilistic recurrence intervals cannot be robustly estimated, the manuscript should use more qualitative terminology, such as frequent-type scenario and rare-type scenario,

...which is exactly what we do, see section 2.5:

> For each tile we compute four scenarios: historical-frequent, historical-rare, future-frequent, and future-rare. Maximum 3- day snowfall is based on the years 1981±2010 for the historical scenarios and 2031±2060 for the future scenarios. Frequent scenarios generate PRAs using 30-year return periods and rare scenarios use a 300-year return period.

> Védrine et al. (2022) - The reference itself is appropriate for avalanche–forest interaction. However, it is used to support the statement that Western Hemlock and Sitka Spruce dominate Southeast Alaska evergreen forests.

Citation was moved to the end of the sentence, to clarify that it supports interaction of large trees with avlanches. I was previously not able to find an academic reference on the ecology of SE Alaska, although the statmenet of Sitka Spruce and Western Hemlock is widely known

to be true. (Nor is the specific species composition relevant to this paper, only the fact that trees in SE Alaska rainforest are large). No additional citations are needed here.

> “ArCGIS” → “ArcGIS”;
> NCLD/NLCD should be standardised;

Thank you, fixed.

> Van Vuuren et al. DOI formatting should be corrected;

Thank you, fixed.

> The spelling and diacritics in “Bühler and Kuhne (2018)” should be checked and standardised to Bühler and Kühne, where appropriate.

Thank you, fixed.