Response to Reviewers
Elizabeth Fischer
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Reviewer 3

> Are the years selected for a historical baseline (1981–2010) more relevant to use than the current 30-year climate normal period (1991–2020) or another period? Some justification/sensitivity testing would be appropriate here, especially given trends in recent decades in temperature and potentially precipitation phase and snowpack.

These are the years we have available. The dynamically downscaled climate model runs end in 2010.

> Suggest to include more quantitative information in the abstract (L10–13), e.g., spatially, how large are the changes in hazard extents, how much less are runout distances, how much more snowfall?

We can include this information drawn from the Conclusions section.

> How sensitive is the hazard indication mapping methodology to different snowfall rates across snow and avalanche climate regions? Were different rates tested (e.g., 1 day, 2 day, 4 day, etc)?

Not sure what reviewer means by “different rates”? 3-day max snowfall is well established for avalanche modeling.

> How well do the CFSR snow depth climatologies verify with actual observations?
> How were these climatologies used (or not) in the WRF downscaling process?
> Is the maximum (L120) from CFSR?

> Overall, I felt the WRF modeling approach could use more explanation, if this gets overwhelming to include in the main text, an appendix would be appropriate.

See Lader et al 2020 and Lader et al 2022.

> The statement on orographic precipitation (L136) requires a citation.

The statement in question is: “Orographic precipitation is a major driver of snowfall in Southeast Alaska.” We dispute the need for a reference on this basic fact. Encyclopedia Brittanica states: “Orographic precipitation happens when moist air from the ocean is forced upward by coastal mountains, cooling and dropping heavy rain or snow on the windward side. We note that the mountains in question are, in fact, a coastal mountain range, and the coastal rainforest is a well known fact of Southeast Alaska.

> Was any verification performed on the lapse rate calculation, if even against another model due to limited observational data?

The verification was against the data itself, in which different nearby WRF gridcells have different snowfall amounts. See Section 2.3.1 in the paper.

> Could the downscaling output on other fields (rain versus snow, wind, temperature on precipitation days) be leveraged to provide additional insight to the potential changes in avalanche characteristics in the future?

We used the same methodology as Bühler et al 2022. Additional fields could be leveraged to the extent the underlying methodology evolves.

> What boundary conditions were used for WRF historical run? Presumably this is in Lader et al. 2022 but needs to be noted here.

CFSR runs. See Lader et al 2020.

> Additional detail on the limitations of single deterministic projection (one run from one model) is necessary (this should form another bullet with details in the discussion)

See Lader et al 2020 Section 2 first paragraph (Data and methods).

This study of *hazard indication maps* is based on the *maximum* possible 3-day snowfall over a 30 year run. In this case, the difference between single vs ensemble runs is reduced.

> is the CESM model biased warm/wet/cold/dry compared to other CMIP models in this area? How much ensemble spread is there in CESM itself?

We used CCSM not CESM. See Lader et al 2020 for discussion of biases.

> How sensitive could presented results be to scenario uncertainties, internal variability uncertainties, and model physics-related uncertainties (especially microphysics in WRF, which given limited verification data, may be difficult to chose)?

See the Monaghan 2018 paper (referenced in Lader et al 2020) where they did the sensitivity of the parameterization schemes. Then Lader et al 2020 used their parameterization selections.

Monaghan, A. J., M. P. Clark, M. P. Barlage, A. J. Newman, and L. Xue, 2018: High-resolution historical climate simulations over Alaska. J. Appl. Meteor. Climatol., 57, 709–731, https://doi.org/10.1175/JAMC-D-17-0161.1.

> Hazard and risk is qualified inconsistently throughout, I suggest to clean up the language and ideally quantify hazard ratings. A few examples: L265: “high-risk”, L283: “clear and present danger”, L284: “serious hazards”, L300: “real risk” L308 “significant avalanche hazard”, L326 “at-risk”. I disagree that a ‘blunter more qualitative approach’ aligns with available data to classify natural hazards; we should strive for qualitative estimates in risk assessments at all times to ensure communication is clear and efforts are repeatable and not susceptible to human biases.

We will update the manuscript with the following conventions:

- *hazard mapping:* Traditional hazard maps made by experts
- *hazard indication mapping:* Automated hazard mapping as defined by Bühler et al. and used in this study.
- *return period:* Inverse of probability an event happens in any given year.
- *susceptibility:* Area of land covered by avalanche extents under a given scenario (Section 3.2)

> Figure 19a does not support the claim made in L333 as it does not show any change in rain versus snow. I would expect a set of figures showing frozen/liquid accumulation ratio changes and total precipitation changes for each phase. Are there more high elevation rain-on-snow events? This statement requires further evaluation, which given the impact of projected precipitation phase and magnitude changes and the availability of model output to assess this completely, is well warranted.

Lader et al 2022 Figure 8 shows the snow/rain ratio decreasing everywhere. Lader et al 2020 Figure 20 shows total precipitation increasing everywhere in SE Alaska. Together, these statements in referenced papers support our claim.

> (also the units are strange in Fig 19a, snowfall is not measured in kg m^-2).

The units provided from Lader are in “mm SWE,” which are not an SI unit. The SI unit as per Copernicus standards, is kg m-2, which is equivalent to mm SWE. Snowfall very much can

be measured in kg m-2, and consistent use of SI units minimizes ambiguity across space and time.

> How was baseline and future risk calculated in Figures 18, 19b, 20? I couldn't find details on the calculations used nor the input data to estimate exposure and vulnerability of values-at-risk.

Under the terminology standardized above, Figure 18-20 plot *susceptibility*. The baseline and future *susceptibility* were calculated based on the total area of extent polygons for the avalanches we simulated.

> Specific comments:
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> L27-34: Complex and long sentence, suggest breaking up.

Thank you, broken up.

> L36: Should this be 'the state's avalanche terrain' as much of the state is not susceptible to avalanche hazard?

Changed to "avalanche hazards for the vast majority of the state remain unmapped"

> L39: How many [weather] stations are in the area and in the initiation zones?

There are 16 weather stations in Southeast Alaska, which includes one in Cordova.

https://www.arcgis.com/apps/mapviewer/index.html?layerId=0&layers=cb1886ff0a9d4156ba4d2fadd7e8a139

See here for more info on NWS weather stations:

https://www.weather.gov/arh/ak_obs

Weather stations are in fact far more sparse in Alaska than the Lower 48.

> L94: Suggest to include genus/species names for tree species.

See Harris and Farr, 1974. I am not a botanist and I cannot speak with scientific authority on these issues. The specific species involved is not relevant in this case. The key fact here, which is cited from Harris and Farr 1974, is: “with thin flexible stems that do little to stop the initiation or spread of avalanches. Alders are a successional species, most common in areas disturbed for example by large avalanches, logging or fire.” Harris and Farr 1974 also does not use genus/species names for trees, and my knowledge on the subject goes no further than that paper.

> L140: Better word than “porous” to describe the coast? Fractal? Porous pertains to how liquid flows through a solid so seems awkward. Landform literature might have a better term.

Thank you, yes fractal is better.

> Figure 8: A third panel showing a difference map of future - historical snowfall would help see areas of largest/smallest changes.

Thank you, caption for Fig 8 updated to reference Fig 19.

> Figure 10: what do the dark vs light green pixels indicate?

See above, appropriate caption info added to refer to the LANDSAT colors.

> and many others, does not have key cartographic elements and will need a scale, legend, and inset map.

We made reasoned choices to present the relevant data in as clear a manner as possible. Specifics on which features the reviewer feel are missing in which plots would have been helpful.