Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2019-433-RC1, 2020

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Sensitivity of modeled snow stability data to meteorological input uncertainty" *by* Bettina Richter et al.

Simon Horton (Referee)

shorton@avalanche.ca

Received and published: 4 April 2020

General comments

This paper investigates how stability indices predicted by snowpack models are impacted by uncertainties in the weather inputs. Spatial snowpack simulations could be valuable for avalanche forecasting, however there are numerous challenges in producing accurate spatially distributed weather inputs for these models. This paper provides a strong quantitative analysis of what the implication of these uncertainties are when assessing snowpack stability. The sensitivity analysis uses weather data for a situation where a prominent weak layer formed in the snowpack and subsequently resulted in avalanche activity throughout the season. Although only a single scenario is in-

vestigated, the implications of various biases added into the data provides a robust analysis of how uncertainties in different weather inputs impacts weak layer formation, slab properties, and snowpack stability.

The paper provides a significant contribution by improving the interpreting stability indices and illustrating the need for improved prediction of snowfall patterns. The methods are valid and rigorous, and the manuscript is well structured, organized, and easy to follow. My comments are relatively minor and could improve the manuscript by clarifying a few details and expanding on some interesting results.

Specific comments

- An interesting result that could use more discussion is explaining why the uncertainties resulted in unequal proportions of properties relative to the reference run. For example, Fig. 3 shows the majority cases had weak layers with lower densities and larger grain sizes than the reference run, and Fig. 9 shows the majority of cases had smaller critical crack lengths than the reference run. While such trends are reported throughout the results, they are not explained in the Discussion. Do these results mean (a) the distribution of input uncertainties were biases towards these results, (b) there were interaction effects between different combinations of biases that favoured these results, (c) some type of non-linearities in the model, (d) something else? If related to the biases, which biases resulted in these trends and why?
- There could be a bit more clarity on how the biases were applied to the weather data, since the distribution of weather inputs has a substantial effect on the results. I interpreted the method as follows: for a given time series, a bias b was randomly chosen for each variable and then that single value applied to the variable for the entire season. This could be stated more explicitly. If random biases

were selected for each variable you would expect a roughly equal proportions of different bias combinations (e.g. samples with P+/TA+, P+/TA-, P-/TA+, P-/TA-). Would such combinations reflect the distribution of conditions you would actually expect to find in nature? Is this method consistent with other sensitivity studies using weather data? I suspect the method of applying these biases resulted in the skewed proportions discussed in the previous comment.

- A limitation of the study is that it considers a single type of weak layer and snowpack structure combinations (i.e. early season facets above a crust). The type of weak layer considered in this study is important and should be stated in more places (e.g. abstract and conclusions). While briefly discussed in lines 317-323, many of the results likely still generalize to more types of snowpack conditions (especially the slab properties). For surface hoar, a major sensitivity is the exposure time of the layer on the surface in between precipitation events. A light amount of snow could stop surface hoar growth in a much more dramatic way than facets. This again strengthens the argument that precipitation patterns (spatial, quantity, and timing!) are critical. While the details of surface hoar formation are outside the scope of this study, acknowledgement of this limitation and more discussion of what results likely transfer to other weak layers would be valuable.

- While the paper touches on most of the interesting results, there are a few minor results listed in the Technical comments that could also be discussed (e.g. why does wind speed impact shear strength?, why does weak layer grain size on 2 Jan not show sensitivity to temperature or radiation as might be expected for facets?)

- The discussion section could be reduced as there is substantial repetition from previous sections (e.g. lines 281-282 repeat the methods, lines 301-306 repeat introduction/motivation of study, lines 307-309 repeat methods, etc.). While this section is well written and examines interesting results, the repetition of why and

how the study was done is unnecessary.

- The conclusions could have greater a emphasis on the contributions of the study. Although well written, they primarily focus is summarizing the results.

- Overall the figures are clear, legible, and are effective at communicating the key results of the study.

Technical comments

- p1 l8: It would be helpful for the abstract to briefly explain the snowpack conditions for the case study (especially the fact the type of weak layer was early season facets above a crust).

- p1 l 17: add "(more stable)" following crack length sentence for consistent structure.

- p1 l15 "sensitive **to** precipitation"

- p2 l49-52: It would be helpful to explicitly explain how to interpret SK38 and rc in relation to initiation and propagation (e.g. "low values of SK38 indicate initiation more likely, low values of rc indicate propagation more likely")

- p2-3 l 53-74: I appreciate how this paragraph concludes by identifying the clear gap in literature that this study addresses, however most of the paragraph reads like a long list of studies and the link to your research question isn't always apparent. I think by rewording some sentences it could be clearer how these studies relate to your research question. Also, Andrew Slaughter's PhD thesis (Slaughter, 2010) performs a SOBOL sensitivity analysis for formation of several types of weak layers and is relevant to this study.

- p4 l1: Just a comment: the weekly snow profiles aren't directly used in your study, although I assume they were important for understanding the avalanche conditions that you describe.
- p4 l115: Thickness-weighted averaging may smooth out the properties of the most unstable layer(s) that may contain the critical properties for avalanche release. Could this averaging method somehow impact the biases favouring the formation of more unstable layers?
- p4 l116: "shear strength **of the weak layer**. . ."
- p4 l116: I understand you present the SK38 and rc derivations in general form, but would it make sense to use the bar notation for the variables that you substitute with thickness-weighted averages (such as slab and weak layer densities)?
- p5 l131-134: Please provide a written explanation of what this correction factor accounts for.
- p5 l 136: In the abstract you specify the uncertainty values are typical for extents of 2 km and elevation changes of 200 m. It would be worth including that somewhere in the text.
- p6 l150: Please specify here whether Case ALL has a unique set of biases or simply concatenates the two other cases.
- Sect. 2.4: This section could use some additional explanation. First, it would be helpful to move the written description of what S_{Ti} means (line 160-161) before the mathematical definition in Eq. 5. On line 162 you describe a 'perfect additive model', but do not explain whether this is important or how that idea applies to this study. It's not clear what information is contained in the A and B matrices as you simply describe their dimensions rather than their content, and thus the

importance of A_B is unclear. Without explanation I'm wondering if B is a matrix full of biases b you introduce in Table 1 (i.e. the same letter).

- Sect. 3.1: This section provides a very clear and helpful practical explanation of the case study.
- p8 l195: A more intuitive wording would be something like "We present weak layer and slab properties on 2 January with results from the reference run and case WL because..."
- p8 l197: In the methods your weak layer group consists of more than just facets (e.g depth hoar and surface hoar), does the "percent facets" variable actually mean percent of weak layers or literally percent facets and there was no depth hoar or surface hoar?
- Fig. 3 and 4: Would a more logical progression be showing Fig. 4 first to show which input uncertainties had the greatest effect then show Fig. 3 to show the direction of the effect? Seeing which weather input had the greatest impact on a given property would help explain why a specific scatter plot is being shown. Same logic applies to Fig. 5 and 6 and 9 and 10. Just a thought.
- p9 l204: Is weak layer thickness also calculated as an average of each individual layer, or was it the sum of all identified weak layers? The sum seems more meaningful.
- p9 l204-205: This result about the impact of precipitation is somewhat unique to how this weak layer is being identified (as all layers forming over a date range), and it is not necessarily intuitive to think about how precipitation during a formation period impacts weak layer formation. It would be helpful to reiterate what precipitation means for this specific case. Also, wouldn't you expect grain size to be more sensitive to air temperature (and perhaps the radiation variables) given the weak layers were faceted crystals?

- p9 l208: It would be interesting to discuss why weak layer shear strength was most sensitive to wind speed as well as the direction of the relationship (i.e. did increasing wind typically result in higher or lower shear strength?). This result is not necessarily the most intuitive and could be discussed more.
- p11 l211: It would be helpful to introduce this date the same way as 2 Jan by introducing the fact you now consider all three cases before you start reporting results.
- Fig. 5: The load-P subplots present obvious results and it's not clear there's added value in graphing these relationships.
- p12 l223: High slab load than what? The reference case?
- p12 l227: Does it make sense that S_T would change between 2 Jan and 9 mar if Case WL uses the reference data from 2 Jan onwards?
- p12 l233-243: This paragraph is very well written and easy to follow!
- Fig. 8: Is it correct to follow the points as a time series starting from the bottom left? If so, including a line connecting the points (and perhaps even an arrow) could make it clearer this shows evolving stability properties rather than an independent scatter of data points.
- p13 l 254: Could you provide a similar summary for rc as done for SK38 in line 247 ("This suggests, that different slabs influenced SK38 more than different weak layers"). It appears from Fig. 9 rc was equally impacted by weak layer and slab properties.
- p14 l259-268: I found this paragraph slightly confusing to read. Perhaps some parts could be reworded or even some of the interpretation moved to the Discussion.

- p18 l320-324: This result agrees with Horton et al. (2015) who examine how variability in meteorological fields from NWP models across elevations resulted in reasonable predictions of surface hoar formation. Slaughter (2010) also analyzes sensitives of surface hoar and other weak layers to weather inputs.
- p18 l340-342: This is a very practical take away from this study that supports practical forecasting experience, and could be a valuable application of snowpack models.
- p18 l343: "than in **weak layer** shear strength"
- p18 l344-345: These results could be supported by citing field studies that describe the lag in weak layer shear strength increases after loading, such as Jamieson et al. (2007) who also give interesting implications on spatial variability of stability indexes due to variable precipitation.
- 18 lp351: How do you explain this counter intuitive result where SK38 remains low into spring? It would seem that since the load continues to increase that the weak layer strength must have remained low. Was this the case?
- p19 l360-361: How does this sentence about precipitation tie back to the theme of climate change?

References

- Horton, S., Schirmer, M., and Jamieson, B.: Meteorological, elevation, and slope effects on surface hoar formation, The Cryosphere, 9, 1523–1533, https://doi.org/10.5194/tc-9-1523-2015, 2015.

- Jamieson, B., Zeidler, A., Brown, C.: Explanation and limitations of study plot stability indices for forecasting dry snow slab avalanches in surrounding terrain, Cold Regions Science and Technology, 50(1-3), 23-34, https://doi.org/10.1016/j.coldregions.2007.02.010, 2007.
- Slaughter, A. E.: Numerical analysis of conditions necessary for near-surface snow metamorphism, PhD thesis, Montana State University, Bozeman, Montana, USA, 2010. https://search.proquest.com/openview/4e7f8f2f70589efc81d6d9198d67ee62/1?pq-origsite=gscholarcbl=18750diss=y

---

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2019-433, 2020.