Review: Ruttner et al., for *TC*, Nov. 2026

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The authors present an interesting investigation of event-scale wind-loading behaviors relevant for avalanche prediction along mountain slopes in Iceland. In my view, the primary contribution is the high-resolution TLS dataset and analysis of detailed event-scale dynamics across different meteorological conditions. The secondary contribution is a relationship between snow loading and topographic shelter (maximum upwind angle, Winstral Sx), which has been previously demonstrated in numerous other environments and only partially explains the observed variability at these sites. The graphics are quite nice, and the text is generally easy to understand, though I do have some minor recommendations on wording. Overall, I think this is a worthwhile case study and suitable for TC, though I think it could be considerably strengthened by (1) incorporating topographic metrics related to fetch or upwind contributing area and (2) condensing some of the text to make the key points more explicit.

Main Comments

#1: Metrics related to upwind "snowshed" / contributing area / fetch / etc.

As the authors note in line 39, with flat-topped mountains (and even just in general), the upwind contributing area (from which snow is scoured) may be more important than the small-scale wind shelter (Sx). Conceptually, if there are several small parallel gullies on a mountain front (like the authors' study sites), whichever gullies are farthest upwind should trap the most snow, and farther-downwind gullies should receive less snow even if they have equal (or greater) shelter metrics. This has been explored extensively in the drift trapping and snow fence literature dating to the 1950s-1980s; see Tabler's many works for a starting point. This is the principle that snow fences use to trap snow upwind of critical infrastructure: if the blowing snow source is depleted by the first few obstacles, downwind shelter no longer matters. In an environment with many parallel gullies, this would be particularly important when winds are oriented parallel to the mountain front (perpendicular to the gully orientation). In that case, I would not expect the Sx metric to adequately capture topographic effects on drifting (even discounting wind speed, precipitation, and other factors).

However, there are already some existing topographic metrics addressing this problem that the authors might consider. The earliest that I am aware of is "fetch," which is defined as the distance along a specified azimuth to the nearest topographic obstacle (Lapen and Martz 1993). This must be defined at a suitable grid scale so that "topographic obstacle" is not trivially represented by individual rocks.

> Lapen, D. R., & Martz, L. W. (1993). The measurement of two simple topographic indices of wind sheltering-exposure from raster digital elevation models. *Computers & Geosciences*, *19*(6), 769–779. https://doi.org/10.1016/0098-3004(93)90049-B

I have personally investigated this effect quite a bit in the Rocky Mountain region, where I introduced a new "upwind area" topographic metric that explicitly accounts for the effects of plateaus and other blowing snow source areas. The idea is to integrate the horizontal area within some range of upwind azimuths for which a particular location is the nearest downwind sheltered

region. Appendix C has the formal algorithm definition, but the R code is in the repository for that paper, and I am also happy to correspond with the authors directly if they are interested in applying or adapting the upwind area metric to their study sites. Especially see Figure 4 (conceptual version of the metric) and Figure 8 (comparison of the upwind angle / Sx with the new upwind area). Figure 9 further shows that upwind area, not upwind angle, is the dominant terrain factor for the deepest snow regions in a flat-topped mountain region.

> Boardman, E. N., Fountain, A. G., Boardman, J. W., Painter, T. H., Burgess, E. W., Wilson, L., & Harpold, A. A. (2025). Wind and topography underlie correlation between seasonal snowpack, mountain glaciers, and late-summer streamflow. *The Cryosphere*, *19*(8), 3193–3225. https://doi.org/10.5194/tc-19-3193-2025

For the present Iceland study, a new metric could also potentially be introduced as the cumulative upwind Sx within some maximum distance, which directly addresses how many upwind gullies could trap blowing snow before it gets to a particular point. At minimum, I would like to see the authors more explicitly delineate between wind shelter and wind accumulation potential, though I think that the opportunity to explore accumulation potential through additional topographic metrics would be worth considering to expand the paper's scope and possibly address some of the unexplained spatial variability.

#2: Condensing text to clarify key points

This is somewhat of a generic comment that is applicable to many papers, but I think it would be helpful to restructure the discussion (and perhaps also the results) to have a clearer organization that leads the reader to the key conclusions. Concretely, I think the discussion should be considerably shortened, and much of the sensitivity analysis and alternative approaches (WindNinja, Figure 15, wind speed and precipitation scaling in Section 5.3, etc.) should be moved to the methods and results. Discussion paragraphs would be easier to follow with clear topic sentences that set up a well-defined argument or interpretation in each paragraph. I encourage the authors to identify 2-4 simple key points that can be stated as a single sentence, then use these points as the topic sentences for the discussion paragraphs, and only build the minimum of additional text necessary to support the points. In my opinion, that would make the reader more likely to retain the study's takeaway-messages, which will lead to better discovery and citation in the future.

Line Comments

Line 1: Suggest generalizing the wording by removing "in Iceland," as the wind/avalanche relationship is applicable in many geographic regions.

Line 8: "snow thickness" reads strangely to me; "snow depth" seems more common.

Introduction: The anecdotal start is a bit unusual, but I like it, because it immediately and concretely frames the problem in this particular region.

Line 23: "is known to build up quickly" is passive voice; suggest rewording to active voice: who knows this? (Perhaps "local reports indicate that avalanche risk builds up quickly…" or something like that.)

Line 37: What is a “direct action avalanche regime?” Citation for that framing?

Line 65: “applied in the study” which study?

Lines 71-73: Sure, these models aren’t necessarily designed to reproduce individual events at the scale of release areas, but then neither is the upwind angle index used here. A better argument might be the lack of high spatiotemporal resolution forcing data and/or computational complexity or other missing-data problems with process-based models.

Line 106: The idea of an upwind “snow catchment area” mentioned here was mapped quantitatively for the first time and named the “snowshed” in my recent work:

> Boardman, E. N. (2025). Constraining Kilometer-Scale Mountain Snow Transport and Snowshed Areas. *Geophysical Research Letters*, *52*(10), e2024GL113599. https://doi.org/10.1029/2024GL113599

Especially see Figure 4 (upwind snowshed concept) and Figure 1 (flat-topped mountains).

Line 148: Ah, I see now what the authors mean by “snow thickness.” I still think that terminology might be overly confusing in the abstract.

Section 2.3: Did the authors perform any bias-correction or coregistration between snow-free and snow-covered DSMs? There can be substantial aspect-dependent biases introduced by sub-grid biases, which is typically handled through bare-surface coregistration.

Lines 156-157: The definitions of HS and DS should be introduced earlier, when “snow thickness” is first mentioned within the main text.

Line 177: As written, “we experimented with…” leaves it unclear whether the WindNinja downscaling was actually used for the study results, or just tested. Also, what WindNinja settings were used? The model has potentially very different results for the mass-only solver vs. the mass-and-momentum solver (which calls the underlying OpenFOAM CFD library).

Line 184: Either explain what a “size 3 avalanche” means, or cite a reference for the sizing criteria.

Figure 2: This is a good figure. However, I’m a bit confused why the study events seem focused on the period (in March) when snow was declining (DS SM4 decreasing), as opposed to the period in earlier March when snow was accumulating. Presumably this accumulation period, leading up to the peak DS SM4 around mid-March, would have been the time when wind was most important for determining the final snow loading, right?

Line 238: Sentence cut off midway?

Line 247: A maximum search distance of 15 m seems extremely short. With wind speeds of 20+ m/s (Figure 3), suspended snow travels that distance in less than 1 second. Thus, it is reasonable to assume that the snow may blow many hundreds of meters over the duration of a storm, and could even just blow above/across shallow depression on the 15 m scale. At a minimum, I would

like to know what range of search distances the authors tested (up to kilometer-scale ideally) and confirm that the results are really only sensitive over such a short range.

Line 258: I understand why the authors exclude areas affected by avalanches (to avoid confounding effect on depth variability), but this is conceptually problematic if the motivation is specifically to investigate avalanche release zones. Is it not possible to compare ΔDS even in avalanche zones as long as no avalanche occurred between 2 scans? Or is that basically what the authors are doing already?

Line 283: The "wind directions associated with the greatest potential accumulation" seems to beg for the application of an upwind contributing area (snowshed) metric, without which it is hard to say whether it actually has more accumulation potential, or is simply more sheltered. The number of upwind drift-trapping obstacles in a particular direction is probably more important than the absolute shelter of any particular location. See main comment #1.

Figure 11: I wonder if part of the slope-scale mismatch here could be attributable to the short (15 m) scale of the Sx calculation. Have the authors considered a larger-scale Sx, not just with a longer search distance, but also with a coarser/smoothed DSM? For instance, a 10 m resolution DSM with 100+ m search distance could reveal slope-scale shelter patterns that are confounded by micro-scale variability (individual rocks) at 1 m.

Figure 13: This reinforces my point about the appropriate length scales to calculate Sx: it seems like much of the variability in the Sx maps is driven by micro-topography as opposed to slope-scale topography, while I expect that snow drifting is more responsive to slope-scale effects (10s to 100s of meters), especially at high wind speeds.

Figure 14: This is a great figure. One way to incorporate my suggestion of a coarser/long Sx would be a second ring around the existing wind rose, or even a 2-part version of the right panel with the directional response of Sx at 15 m using a 1 m DSM (which I think is what it is already showing) and a second version at 150 m using a 10 m DSM. This would directly show to what degree Sx changes depending on scale-dependence.

Lines 400-407: This answers my previous question about WindNinja; however, the original question persists because of the paper structure. I think the WindNinja section should be in the methods and results, not just the discussion.

Figure 15: I think this is one of the best results figures, I like it. However, I'm not sure it belongs in the discussion as opposed to the results.

Line 415: Some numbers here would be helpful: briefly remind the reader by how much the correlations improved.

Line 425: But wouldn't this already be captured by Sx?

Line 430: Even though this is preliminary and based on just a few events, why not put this into the main results with a figure? As currently written, the "strongest relationships" only get a brief mention in the discussion with no numbers or figures, which is a bit odd. Either the relationships

are deemed spurious / unphysical (and the authors should just delete that section) or the relationships are interesting enough to merit a full results paragraph.

Line 440: It does seem like the Sx maps could be especially useful for visualization and discussion purposes, as it directly highlights the terrain variability on a map. I would recommend adding a sentence specifically focused on the "visualization potential," which is currently hinted at ("looking at…") but not explicitly stated.

Conclusion: The first paragraph seems a bit too much like redundant summary. I would recommend a more incisive synthesis here, instead of just a recap.

Acknowledgements: I think it's generally good practice to state the names of which AI tools were used (for example, "Claude Opus 5.5"), though I'm not sure if it's required.