Response to Referees

Investigating wind-driven Snow Redistribution Processes over an Alpine Glacier with high-resolution Terrestrial Laser Scans and Large-eddy Simulations

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Dear editor and referees,

We would like to thank the editor for handling our manuscript and the referees for their careful evaluation of our work and the valuable suggestions, comments and questions. We believe that the manuscript substantially benefits from the referees' feedback. Below we address our detailed responses to all the comments.

In this response-to-review document we try to clarify and address each of the suggestions, comments and questions made during the review. Therefore we have copied the comments in blue boxes and have addressed them one by one. In the response we use italic fonts to quote text from the revised manuscript.

Yours sincerely, Annelies Voordendag, Brigitta Goger & co-authors

Response to referee #1

Overview

R1-1: Authors have correctly addressed most of my comments. Now the manuscript benefit of the publication of the snow drifting module and on a more quantitative analysis of the case study. I want to also recognize the effort of the authors on calculating the compaction, to better evaluate the drifting model. I think that this paper is now acceptable to be published in TC after addressing some few minor comments more that arised in this second lecture of the paper.

We are very thankful for the constructive comments of referee #1.

Piecemeal

R1-2: L69: accordingly Wagner et al. (2014)

The sentence now reads:
At this resolution, both topography and glacier ice surfaces in the Alps can be expected to be well-resolved, given that at least 10 grid points across a valley are necessary to resolve the relevant boundary-layer processes (Wagner et al., 2014).

R1-3: southerly without caps.
R1-4: L125: If the trough is over France, are you sure is moving westward? Or is it eastward?

Corrected these typos. The sentence now reads:
The southerly flow was mostly associated with a trough over France moving eastward towards the Alps, while the trough axis passed our location of interest on 7 Feb 2021 after 18:00 UTC.

R1-5: L170: there is a problem here with the sentence going beyond the margin and the end of the page.

This is a problem with *latexdiff*. In the version without track changes, this problem is not apparent.

R1-6: Figure 8. For better comparison I would suggest to represent b and c panels only for those points with data in a. In the current figure I find difficult to compare the points between a and b.

We added grid lines to all subplots in Figure 8 and the glacier outlines as used by the model in Figure 8a. Removing simulated data in Figures 8b and c would limit us in our statements on the good model simulations at the ridges, as the ridges are sparsely captured by the TLS system, but realistically simulated in the model.

R1-8: L387: line typo

This is a problem with *latexdiff*. In the version without track changes, this problem is not apparent.

R1-9: L412: I think that this sentence does not totally agree with the results showed.

- Only large-scale spatial structure is realistic (L390: model is not able to capture the small-scale snow depth structure at the slopes), and it links with the following sentence.
- Magnitude is not realistic (L372: The order of magnitude of the snow depth changes from the observations is twice as large as the simulated snow redistribution due to snow drift from the simulation).

Maybe the authors can rephrase lowering the claim.

L412 (from the manuscript with track changes) has been clarified:
The simulated snow redistribution is realistic in terms of spatial structure and magnitude. However, the processes at smaller scales are smoothed out, which is due to the horizontal resolution of 48 m and the smoothed model topography restricted by numerical stability. The model topography limits the slope angles to a maximum of 35°, and thus the model topography clearly deviates from real topography.
The magnitude of the snow redistribution is realistic and we also stated several times that the surface elevation changes are not directly comparable to the snow redistribution simulations, as the surface elevation changes also include compaction. For example in L373/374 directly after the statement in L372 and earlier in L222/223. This has been elaborated in the description of Figure 9.

Response to Matthieu Lafaysse

General comments

R2-1: I thank the authors of this paper for their detailed answers to my comments and their attempts to introduce new results in the paper to strengthen its conclusions, regarding precipitation and wind forcing, and mainly concerning the spatial variability of snow depth changes which is the main interest of using these TLS data. However, I feel when reading that this new analysis is maybe not yet completely mature (see my detailed comments).

Thank you very much for the positive feedback and for reviewing our manuscript again.

R2-2: The goal and impacts of the study have also been clarified. I think that additional information about the implications of coupled and uncoupled processes in this system and about the numerical cost would help the community to identify the pros and cons of this approach compared to more expensive or cheaper simulation systems for their application. In particular, the authors suggest in their answer that the feasibility of applying such model over a full season is not so far, but there is nothing in the paper that support and explain that if this is true. On the contrary, a large part of the discussion is dedicated to the implication of not being able to run a continuous simulation to get realistic initial snow density. Therefore, the readers need to understand why the high numerical cost dedicated to the atmosphere is justified compared to the numerical cost dedicated to snow processes for this kind of analyses.

We are - indeed - not sure whether the current set-up is applicable to entire seasons. If seasonal simulations are the goal, they could be also run with a coarser grid spacing (e.g., $\Delta x = 240\,\mathrm{m}$). However, it has to be considered then that the topography and boundary-layer processes are less well resolved than at $\Delta x = 48\,\mathrm{m}$. We treat this point in the discussion.

R2-3: The limitations of the single-case study are well described in the discussion. The code and data availability section has now been provided. I don't know the reasons to not share the TLS data (technical or political) but I believe this kind of data set could also be highly valuable for the community if available, although it is probably not mandatory for this journal.

The TLS data has so far not been published as we wanted to wait until the PhD of A. Voordendag was finalized. Also, there are several products available from the TLS data (registered/unregistered point clouds, digital elevation models) and with 3 years of (nearly) daily data, we have not yet decided on an appropriate way to publish the large data set. However, the data is available upon request from ACINN/Rainer Prinz.

R2-4: Although I think that a second round of improvements is necessary before publication, I recommend the authors to finalize this promising work as detailed evaluations of snow transport models are challenging and insufficient in the current snow modelling literature.

Thank you again for these constructive comments.

Detailed comments

R2-5: The line numbers in my report correspond to the manuscript with tracked changes.

Thank you for the clarification.

R2-6: L52 validate → evaluate

Changed.

R2-7: L53 The reason to cite SNOWPACK is unclear. It would be better to mention the large variety of available snow models in the literature (Krinner et al., 2018; Menard et al., 2021). Then, snow processes are also very commonly simulated in coupled mode. Maybe the goal of limiting this sentence to standalone simulations was to be more specific about higher resolution simulations? If yes, it should be said.

We explicitly mentioned SNOWPACK because it was recently coupled to the atmospheric model we are using in our study (WRF). We added the inter-comparison studies suggested by the referee and re-wrote:
Modelling snow processes is usually achieved by a large variety of standalone snow models, which receive input data from atmospheric models or observations (Krinner et al., 2018; Menard et al., 2021). Recent studies also couple full (previously) stand-alone snowpack models with atmospheric models.

R2-8: L59-60 «CRYOWRF can successfully simulate snow accumulation »and redistribution both over the Swiss Alps and Antarctica (Gerber et al., 2023).» I have checked this reference. It only presents simulations over Antarctica, and it does not demonstrate that «snow accumulation and redistribution are successfully simulated». (It depends what is supposed to mean «successfully», I guess it was consistently with observations). This statement should be reformulated closer to the actual conclusions of this paper (the evaluated variables are local-scale blowing snow occurrence and local-scale surface mass balance, but not the redistributed snow mass). The evaluation of snow redistribution is extremely challenging for all simulation systems and I think conclusions should always be formulated with caution and accuracy.

This was a mistake from our side - we wanted to cite Sharma et al. (2023), where the simulation over the Alps is explicitly highlighted (their Figs. 15-17), and also mention the applicability over Antarctica with Gerber et al. (2023). We re-wrote the sentence:
First results suggest that CRYOWRF is capable of simulating snow accumulation and redistribution over the Swiss Alps and Antarctica (Sharma et al., 2023; Gerber et al., 2023).

R2-9: L61 I don't think that «golden standard» is an appropriate expression. The choice of numerical models depend on applications and CryoWRF and Méso-NH-Crocus are definitely not a golden standard for instance for real-time operational snow simulations designed to monitor snow cover over large domains, or for coupled climate models designed to be able to run over the whole century. However, these models are indeed the ones resolving the most in detail all the coupled physical processes of blowing snow, and this is the best choice for process studies on dedicated case studies, at the expense of a very high numerical cost.

We re-wrote the sentence:
While fully coupled snow-atmosphere model chains likely resolve coupled processes and atmosphere-cryosphere interactions well for case studies (at a high numerical cost),

R2-10: L66 What does mean «relevant» here? Relevant for which application?

Changed to "relevant for process understanding".

R2-11: L72 «In contrast to coupled modelling systems» would suggest that the snow transport module used in this work does not have feedback to the atmospheric model whereas if I understood well (from L171 and L175), it does. Please rephrase to clarify.

We agree and removed the formulation.

R2-12: L72 I don't think that the absence of change in the compilation procedure is a strong argument to justify the interest of the approach.

We agree and wrote a different sentence:
To our current knowledge, this is the first time where an openly available, easy-to-use formulation for wind-driven snow redistribution is implemented in the WRF model code.

R2-13: L71-78 Although the proposed modifications improve the understanding of the objectives of this paper, I think that the target applications of this numerical system are still not explicit enough in this paragraph. «Study the impact of wind driven snow redistribution on a large Alpine glacier for a case study» is the goal of this study, ok. Is it the main application of this modelling system? Or does it have broader objectives?

This study is the first one to test and evaluate the snowdrift module after the initial implementation for a real-case study. Given the TLS observations and the complex environment, the glacier is an ideal test-bed to assess the model performance (for both the atmosphere and cryosphere component). We changed the sentence to:
We present a first evaluation of the newly implemented snow drift scheme with high-resolution TLS observations and examine whether the model delivers realistic results in snow depth change and spatial patterns in this highly complex environment.
The broader objective of the snow drift module in WRF is to add an easy-to-use formulation of wind-driven snow redistribution in the WRF model.

R2-14: L77-78 As the study is limited to a specific event, is it really possible to estimate the impact on the glacier mass balance? At least, it is unclear at this stage why it would be possible. And finally line 443, the authors acknowledge this is not possible.

Exactly - we check whether the set-up bears the potential to asses the impact on the mass balance and then conclude that currently it's not possible. Still, the study shows us a first estimate *whether* the model (and observations!) are able to detect wind-driven snow redistribution in a reasonable way. However, we added in the conclusions that the computational power restraint is obviously limiting the study.

R2-15: L171 Here, the «coupled» word means that there is feedback to the atmosphere, right?

Exactly - we added the feedback in brackets.

R2-16: L192 This is a constant for pure ice density, right? This should be specified to avoid confusion with glacier ice density.

Indeed - changed.

R2-17: L208 Is ρ_s the density of the upper snow layer? Please specify. Also I realize it is not clear how the 3-layer snow model simulates compaction and as I have already asked during the first review, what is the density of new falling snow? These components of the model have to be detailed when the snow model is

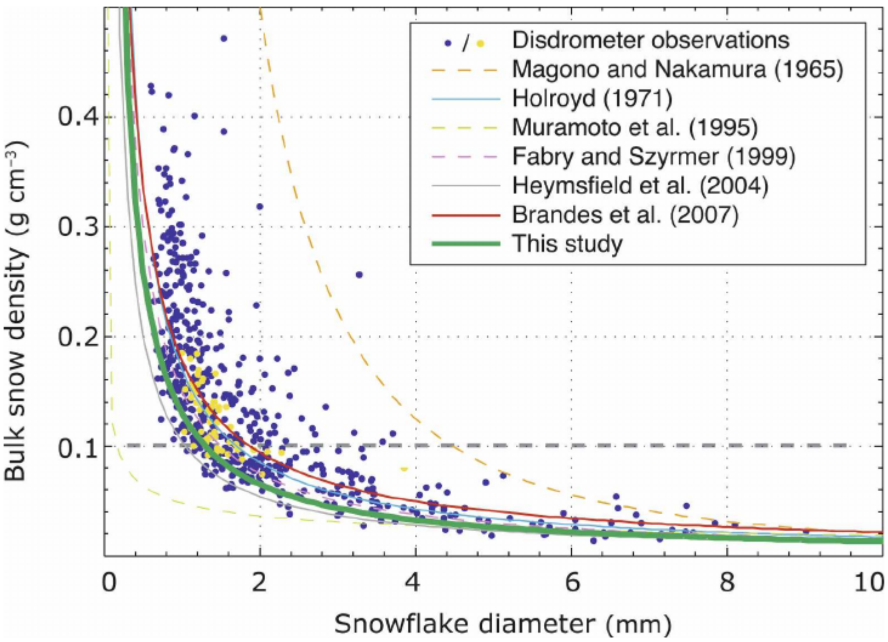

FIG. 1. Plot of bulk snow particle density vs diameter showing disdrometer observations of Brandes et al. (2007) and the modeled relationship using assumptions in the new bulk microphysics scheme. The typical value used in most models is a constant (0.1 g cm^{-3}).

Figure R1: Snow density as simulated by the Thompson scheme (green line). Taken from Thompson et al. (2008, their Figure 1).

> introduced in Sect. 2.4, so that it is possible to understand their interaction with the snowdrift module.

Yes, ρ_s is the density of the upper snow layer. The density of the falling snow is described in Thompson et al. (2008), where snow assumes a nonspherical shape with a bulk density varying with diameter (Fig. R1).

The alternation of the snow surface structure and snow density by erosion and deposition can cause feedbacks onto the drifting snow and flow field structure is currently not included in our scheme - however, it is planned to be added later (M. Saigger, personal communication).

We mentioned in the discussion of the revised manuscript (line 433) that snow compaction in NOAH-MP is included following the empirical formulations by Anderson (1976), while snow compaction by the snowpack's own weight is calculated after Sun et al. (1999). We agree that we have to introduce the compaction description earlier, therefore we added it together with the references in Section 2.4 (Numerical Model).

> R2-18: L256 Are simulated surface temperatures below freezing point over the whole glacier? If yes, this spatial extent of the statement should be mentioned.

Changed sentence to:
[...], the simulations also suggest that surface temperatures remain below freezing point over the entire glacier.

> R2-19: In Figure 3, the color contrast is low between the orange point and the map color scale in Fig b and c, while it is important to distinguish the point to understand the comments Lines 232-233.

We agree and added black circles around the station locations to make them easier to distinguish from the surroundings. We also adjusted this in Figures 8 and 9.

> R2-20: L322-323 «Taken into account that observed and modelled precipitation are two different physical quantities by the way they are obtained» I don't understand what the authors mean. Simulated and observed weather or snow variables are always obtained differently but we still have to evaluate the reliability of simulations. What is specific to precipitation here? Why would observed and simulated precipitation not

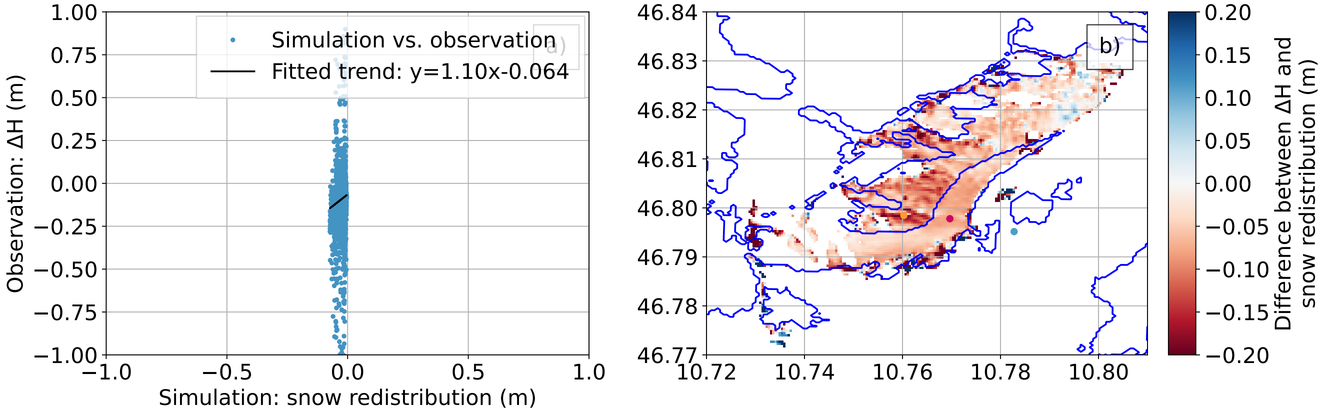

Figure R2: a) Observed snow depth changes over HEF at Δx=48 m between 8 Feb, 10:22 UTC and 9 Feb, 01:42 UTC plotted against the simulated snow redistribution for all the covered grid cells (blue) and the linear fit between these variables (black) **with harmonized x- and y-axis**. b) The difference between the observed snow depth changes and the simulated snow redistribution over the region of interest.

represent the same physical quantity? I would understand from Fig 6 that new snow accumulation is probably underestimated by the model.

This sentence was unnecessarily complicated - we re-wrote it:
We conclude that the model is able to simulate the temporal pattern on the case study day successfully, albeit with a slight underestimation.

R2-21: L324 temporal pattern?

See above.

R2-22: Figure 9a: To better identify the agreement or not between observations and simulations, can you harmonize the x and y axis boundaries so that it looks like a traditional scatter plot ? I also understand from this plot that the observed and simulated variabilities highly differ. This is not so surprising but this should be commented. Is there a significant correlation in this plot ? (you could provide the value of R2). Note that this kind of pixel-to-pixel evaluation is extremely challenging for any snow transport model, and I would not be surprised that the agreement could be moderate or low. It still worth showing this kind of result to be aware of the limitations of snow transport models.

Figure R2 shows Figure 9 from the manuscript with harmonized x- and y-axis. As pointed out by the reviewer, the variability of the observations and simulations differ strongly. The simulations only range between -0.073 and -0.0032 m, whereas observations are found between -2.82 and 3.9 m. These variations in the simulations can be attributed to processes such as avalanches and small-scale snow redistribution from the windward to the leeward side. Due to these variations, the coefficient of determination (R^2) is found to be 0.005 and as expected by the reviewer, this is low. We decided to remain with the figure as presented in the original manuscript, but note that we added grid lines and the glacier outlines as used by the model to Figure 9a.

We have rewritten L386 (manuscript with track changes):
The observed data in Fig. 9a is highly variable compared to the simulations, but this can be related to the more complex topography in reality compared to the smooth model topography. Furthermore, events such as avalanches are not represented in the model.

R2-23: L381 The link the authors attribute between the intercept in their linear regression and the compaction is very unclear, please explain.

We have rewritten this paragraph and it now reads:
The relation suggests that for every 0.01 m of simulated snow distribution 0.011 m of snow redistribution is observed, or in other words, the model underestimates the amount of snow redistribution by only 9.1%. We assume that the compaction rate over the snow pack in the period of 15 hours over the study area is constant and thus the 0.064 m

in Equation 11 is related to the compaction of the snow pack. This amount of compaction is in the range of the compaction that we found for a different winter season (between 0.018 m and 0.071 m of the total snowpack decrease of 0.079 m). Therefore, we assume that the average compaction rate of 0.064 m over 15 hours during this study period is realistic. The observed data in Fig. 9a is highly variable compared to the simulations, but this can be related to the more complex topography in reality compared to the smooth model topography. Furthermore, events such as avalanches are not represented in the model. Likewise, the amount of compaction is not absolutely constant over the study area, as this also depends on the snow depth and the weight of overburden layers, and to a minor extent to the wind speeds. However, we assume that variability in compaction is low relative to the effects of snow drift and therefore assume it to be constant.

> R2-24: L389 This results does not only inform on biases but also in on the realism of the spatial pattern of snow redistribution.

Changed.

> R2-25: L389 «we have to keep in mind that the TLS data still includes the snowpack compaction». Both observations and simulations include compaction. The compaction of new snow highly prevails after a snowfall event compared to compaction of old snow. Therefore, in this situation, the argument of the authors in their response that initial snow was initialized to a fixed density is not sufficient to consider that simulations are not able to reproduce snow depth change due to compaction. This also has to be considered in the discussion (L434-436), compaction of new snow should prevail in this case.

We agree that the model is able to reproduce snow depth change due to compaction, but this only works if the model is initialized with a realistic snow pack, i.e. fresh layers of snow on layers that are already compacted. This was not possible in our case study due to the high computational costs. Also, we can derive the amount of snow at the glacier with DEM differencing of TLS scans between October 2020 and February 2021, but we do not know any of the physical properties of the snow pack, such as surface temperature or density, which makes a realistic initialization also not viable. We added to L436:
Also, the amount of snow at the glacier can be derived with DEM differencing of TLS scans between October 2020 and February 2021, but any of the physical properties of the snow pack, such as surface temperature or density remain illusive, which makes a realistic initialization also not viable.

> R2-26: L390-391 «Adding the domain-wide average of the snow compaction rate we found in Fig. 9a leads to inconsistencies in the observational data set; therefore, we omit this step.» As compaction is already simulated by the model, why would the authors want to add again an extra-compaction. Please clarify this unclear statement.

We meant that an average of the obtained compaction of Fig. 9a cannot be subtracted from the observational data set, therefore we omit this step. We do not talk about the model results here. We re-wrote the sentence:
Adding the spatial average of the snow compaction rate from Fig. 9a to the observational data set leads to inconsistencies; therefore, we omit this step.

> R2-27: L411-412 «The simulated snow redistribution is realistic in terms of spatial structure and magnitude». Is the spatial structure of snow redistribution really realistic? This is far from obvious from Fig 9a. Can you be more accurate and/or mention from which Figure this conclusion is obtained?

We re-wrote the paragraph:
However, the processes at smaller scales are smoothed out, which is due to the horizontal resolution of 48 m and the smoothed model topography restricted by numerical stability. The model topography limits the slope angles to a maximum of 35°, and thus the model topography clearly deviates from real topography. In agreement with the TLS acquisitions, the simulations show that snow is eroded mostly at the ridges and that the snowpack at the glacier is sheltered and less affected by snow erosion.

> R2-28: L422-423 «Its simplicity compared to fully coupled atmospheric and snow models» Beyond the difference in the snow scheme complexity, could you discuss more accurately which processes are not coupled in this system while they are in CryoWRF and Meso-NH and the possible implication of this uncoupling? I guess there is no interaction between the snow transport module and the water content of the lowest levels of

the atmospheric models? Also, what is the advantage of using this system compared to completely uncoupled system as can be found in hydrological systems (e.g., Marsh et al., 2020; Quéno et al., 2023; Baron et al., 2023) It is important to explain that as the disadvantage is rather clear (L429 : «The computationally expensive LES cannot be run with a long spin-up time to initialise the snowpack correctly.»).

Our snow drift module within the WRF model is able to provide feedback to the atmosphere: After the calculation of the snow sublimation due to snowdrift, the energy and mass fluxes due to sublimation then modify the specific humidity and air temperature of the atmosphere aloft. This feedback can be switched on or off in the model namelist; in our case, the simulation was ran with full sublimation feedback. Test runs without sublimation suggested that the impact of sublimation on the atmosphere structure aloft was, however, negligibly small. We added a paragraph on the advantages and disadvantages of our approach compared to other models in the manuscript:
One of the advantages of the presented snow drift module in WRF is its simplicity compared to fully coupled atmospheric and snow models (Vionnet et al., 2013; Sharma et al., 2023), because our snow drift scheme are embedded within the established modules of the WRF modelling system. However, coupling to grain-scale snow models(Vionnet et al., 2013; Sharma et al., 2023) can, of course, provide more detailed information on snowpack evolution and full feedback (fluxes, temperature, humidity) between the atmosphere and the snowpack is possible. In our setup, the feedback of the atmosphere by the snow drift module consists of the impact of snow sublimation on the temperature and special humidity of the atmosphere aloft (Saigger et al., 2023). Furthermore, employing a full physics-based atmospheric model at high resolution provides high-resolution input data for the land surface model. This poses an advantage compared to completely uncoupled hydrological systems (e.g., Marsh et al., 2020; Quéno et al., 2023; Baron et al., 2023), which rely on input from downscaled data, which can be also challenging over complex topography.

R2-29: Finally, the discussion is clearly missing an analysis of the reasons for discrepancies between simulated and observed spatial patterns of snow depth changes, and perspectives to go beyond pixel-to-pixel evaluations in the evaluation of snow transport models.

Thank you for this remark. We added a new paragraph to the discussion:
Still, a systematic evaluation of snow transport models with observations is challenging. In our case, the pixel-to-pixel comparison between the model and the TLS observations allowed us first insight on model performance, however, we are aware that we are comparing different terrain geometries between model and observations. On the other hand, point observations of snow depth or blowing snow fluxes might be unrepresentative, because spatial variability is especially high in complex terrain. New observational approaches such as particle tracking velocimetry (Aksamit and Pomeroy, 2016) will allow for more detailed evaluation of high-resolution snow transport models. Furthermore, bringing modern, multi-scale observational methods together (e.g., TLS, particle tracking velocimetry, snow depth and SWE measurements) in dedicated measurement campaigns would provide excellent test beds for snow model validation.

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