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Interactive comment on "Snow profile alignment and similarity assessment for aggregating, clustering, and evaluating of snowpack model output for avalanche forecasting" *by* Florian Herla et al.

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Summary and major comments

Herla et al. present a matching algorithm between snowpack profiles able to classify and evaluate snow cover simulations designed for avalanche hazard forecasting. I fully agree with the need of new tools to extend the evaluations of these models and facilitate their use by forecasters. The question is very well introduced in the paper. I am also glad to see that the proposal of Hagenmuller and Pilloix, 2016 to use such

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maching algorithms for that purpose found an echo in other modelling teams and is applied in new contexts and with other similarity metrics. I also like the discussion about a possible concrete use of the method in an operational forecasting system. The results shown by the authors in a clustering application are interesting and a number of new advances in this paper are valuable for the community (new metrics, classification and synthesis methods).

However, I am a bit uncomfortable with the way the authors present their work in this context. Indeed their unique reference to Hagenmuller and Pilloix, 2016 is: "Hagenmuller and Pilloix, 2016 was the first to align, cluster, and aggregate one-dimensional snow hardness profiles from ram resistance field measurements using Dynamic Time Warping (DTW), a method from the fields of time series analysis and data mining. While their contribution demonstrates the usefullness of DTW for snow profile comparisons, their method is not general enough to allow for meaningful comparisons of snow profiles from different sources and varying levels of details." Then, they present their work as "a new approach for computationnally comparing, grouping and summarizing snow profiles" and they present the algorithm in details in Section 2.2.1 as it could be expected for the first publication of an algorithm. They also state in the discussion "we are the first to present an alignment algorithm and similarity measure for generic snow profiles". However, the method proposed in this paper can not be told as new. Indeed, the algorithm is extremely similar to the one developed by our colleague P. Hagenmuller and which is already used in several publications (with or without a detailed description of DTW depending on the references): Hagenmuller and Pilloix, 2016; Teich et al., 2019; Hagenmuller et al. 2018a; Hagenmuller et al. 2018b; Viallon et al. (accepted). Note also that DTW was also already applied on snow by Schaller et al., 2016. The innovation of Herla et al. is mainly the distance the authors introduce in the application of this algorithm, in order to use a criteria more focused on mechanical stability, but the general philosophy of the method is the same. Furthermore, the application of such algorithm for clustering also already appears in Hagenmuller et al., 2018b and also include a selection method of a representative profile. Although this reference is not

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a publication in a peer-review journal but a conference proceedings, the authors are aware of this contribution.

I think there is room for everyone to work on similar topics if the context is fairly presented. Therefore I think that the authors should consider the following modifications and questions:

- The introduction should better emphasize the own innovations of this paper and avoid general statements presenting the whole method as a new algorithm.

- Section 2.2.1 should better emphasize what is common and what differs between this algorithm and the algorithm of Hagenmuller and Pilloix, 2016 and following papers. The details of DTW already descibed in previous literature can be moved to an Appendix in order to be more focused on the novelties in the main text.

- Then, it is unclear what is unsatisfying in the algorithm of P. Hagenmuller. What do the author mean by "not general enough to allow for meaningful comparisons of snow profiles"? Hagenmuller and Pilloix, 2016, discuss the possibility to modify the distance criteria depending on applications: "The metrics D and V between profiles whose definition involve the mean square difference of logarithmic hardness can be adapted to incorporate other snow properties. [...]" with examples. In Viallon et al., accepted, we applied DTW with a more general distance combining density, liquid water content, grain shape and depth, better suited for an overall model evaluation and the algorithm behaves well in this context. The distance presented here by the authors is probably better suited to their application but can not pretend to be more "general". Do the authors refer only to the management of missing values in snow profiles? Or is there something else? I think it is important to be more specific on the issues in previous references to better justify and emphasize the innovations they want to publish here.

- A number of choices in the distance definition are model-dependent relatively to the SNOWPACK model. For instance, neither layer hardness or layer date are diagnostics

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of the Crocus model (similar to SNOWPACK in terms of complexity). Furthermore, a number of considerations in the distance definition for grain type are based on considerations about the current typical behaviour of SNOWPACK (lines 5-6 and 23-31 page 5). These considerations might not apply to another model or even to a future version of SNOWPACK with for instance new parameterizations of snow metamorphism. This option can be arguable if there is added value to make this choice but the limitation should be clearly discussed. Will it be necessary to modify the distance definition if significant changes are implemented in the model?

- Finally, it is questionable whereas there is really added value for the community in the future to provide two separate codes from two research teams based on a similar algorithm. The authors requested the code of P. Hagenmuller last winter and they received it with a number of explanations from P. Hagenmuller. This is surprisingly not mentioned in the Acknowledgements section. I think the authors should justify in the paper the need for another code by describing the reasons which have probably prevent them using directly the code from P. Hagenmuller. A better understanding of these limitations might help to avoid further work duplication and hopefully allow more shared developments in the post-processing of snow models in the future, although I am fully aware of the difficulties of such collaborative developments.

Other comments

Page 10 Line 15: Is there a limitation of snow heights differences to apply the rescaling? Is it meaningful to rescale profiles even when the relative difference of snow height reach for instance 500% or more?

Page 10 Line 21: The formulation is a bit ambiguous. Is the value of 0.5 cm was indeed chosen for grid resampling in this work?

Page 15 Lines 27-28 The variables chosen to compute the similarity measure are not prognostic variables of the model but rather indirect diagnostics (grain type, hardness) involving very uncertain parameterizations from prognostic variables. Beyond the clus-

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tering application presented in this paper, the possible use of this criteria for model evaluation raises some questions. Indeed, it can not be established if this metric would be really representative of the real skill of the physical model or if these transformation functions would not prevail in the obtained metric. I fully understand the motivation relative to the avalanche hazard application but this topic should be discussed because (1) a perfect model in terms of physical evolution laws might not provide a perfect similarity measure due to errors in the variables transformation and (2) this criteria might not be recommanded for a process-related model evaluation where I think a metric based on density and temperature profiles for instance should be preferred. In spite of this comment, I acknowledge that the detailed thinking about an appropriate weighting of layers in the final similarity metric is really interesting and useful (although a bit complex) from the mechanical point of view.

Competing interest

I mention as competing interest that I work in the same research group as Pascal Hagenmuller.

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