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Review of the paper "Decision tree-based detection of blowing snow events in the European Alps" by Z. Xie et al. submitted to HESS.

Anonymous Referee #2

Referee comment on "Decision tree-based detection of blowing snow events in the European Alps" by Zhipeng Xie et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-119-RC2, 2021

This paper presents the development a new method to detect the occurrence of blowing snow event in alpine terrain. The authors built decision tree classification models (DTMs) using data from automatic stations measuring simultaneously standard meteorological observations and blowing snow fluxes. They proposed to develop decision trees purely based on standard meteorological observations. The authors found that maximal wind speed had the largest influence in determining the occurrence of blowing snow events for different conditions. They also illustrate how the selection of the stations in the training set influence the performances of the resulting DTMs.

The subject of this paper is interesting for a large community studying snow in mountainous region for various applications: avalanche hazard forecasting, mountain hydrology, road maintenance, … The estimation of blowing snow occurrence is also a key in blowing snow models. My main comment about the study concerns the absence of quantification of the benefit of using DTMs compared to more traditional methods to predict blowing snow occurrence. Machine learning is still new in the community and it would be very valuable if the authors could quantify it benefit compare to more traditional methods. This question needs to be clarified prior to publication in HESS. It is detailed below as a general comment followed by more specific and technical comments.

General Comment

Different methods have proposed to obtain an estimation of the threshold wind speed for snow transport. When combined with information on wind speed, theses methods can be used to estimate the occurrence of blowing snow events. The simplest method consists in constant threshold value for dry and wet snow (see for example the average values reported on page 209, section 4 of Li and Pomeroy, 1997). In the same paper, Li and

Pomeroy (1997) proposed a slightly more complex formulation of the threshold wind speed that only depends on air temperature. He and Ohara (2017) proposed a more advanced formulation that depends on snow particle size, deposition time and temperature. Despite being more complex, this formulation can still be derived using standard meteorological measurements (precipitation, temperature). Finally, formulations of the threshold wind speed that depend on the physical properties of the snowpack have been developed. Liston et al. (2007) used a formulation that depends on the surface snow density whereas Schmidt (1980), Guyomarc'h and Merindol (1998) et Lehning et al (2000) proposed formulation that depends on the microstructure of the surface snow.

The decision trees developed in this study are a new method proposed to determine the occurrence of blowing snow. For the community studying snow in mountainous region, the authors should document the performance of this new approach compared to previous approaches used as benchmarks. So far, it is very difficult for the reader to estimate the benefit using decisions-trees compared to more standard methods. The significance of the paper would be certainly improved it the authors can show that their DTMs can provide improved the detection of blowing snow occurrence compared to more standard methods.

All the formulations of the threshold wind speed depending on physical properties of surface snow cannot be tested here since they require a snowpack model. However, the decision trees could still be compared with the performances obtained for three formulation the threshold wind speed of various complexity: (i) constant threshold for dry and wet snow, (ii) Li and Pomeroy (1997), (iii) He and Ohara (2017). These threshold wind speed could be computed at an hourly time step at all the ISAW stations and compared with mean wind speed and/or maximum wind speed to determine blowing snow occurrence. OA, POD, FAR and HSS could be then computed and compared with the results of the DTMs. Such evaluation would highlight the benefits and limitations of the decision trees and would guide future developments.

Specific comments

Abstract L 21-22: it would be interesting to clarify in the abstract that snow depth and measurements are used as well to determine the occurrence of blowing snow. Indeed, without snow on the ground, blowing snow cannot occur.

Abstract L 25: the authors should explain which atmospheric variables have a divergent distribution.

P2 L 48-49: I recommend the authors to also include the original references for the different sensors measuring blowing snow fluxes:

- SPC: Sato, T., Kimura, T., Ishimaru, T., & Maruyama, T. (1993). Field test of a new snow-particle counter (SPC) system. *Annals of Glaciology*, *18*, 149-154.
- FlowCapt: Chritin, V., Bolognesi, R., & Gubler, H. (1999). FlowCapt: a new acoustic sensor to measure snowdrift and wind velocity for avalanche forecasting. *Cold Regions Science and Technology*, *30*(1-3), 125-133.

P2 L 53-57: The large increase in surface snow cohesion in presence of liquid water (resulting from rainfall or melting) is not the only factor affecting the evolution of threshold wind speed contrary to what is suggested here by the authors. I recommend to explicitly mention the impact of sintering od dry snow (Schmidt, 1980, He and Ohara, 2017). In addition, mechanical fragmentation of snow grains during blowing snow events (Comola et al., 2017) can favor snow sintering and affect the evolution of the threshold velocity (Vionnet et al., 2013).

P 2L 57-58: the dependence of the threshold wind speed on air temperature and humidity is only indirect and is due to the influence of air temperature and humidity on the cohesion of surface snow. It would be interesting to explicitly mention it in the introduction so that it is clear for the reader.

P 2 L 60: Liston et al (2007) are only reporting values from previous studies for the threshold wind speed for fresh snow. I recommend the authors to refer to papers that measured the threshold wind speed for snow transport in the field: Li and Pomeroy (1997), Guyomarc'h and Merindol (1998), Doorschot et al (2004), Clifton et al. (2006).

P 3 L 90-91: I think the author should add snow depth in the list of variables because they are still using this measurement to determine if there is snow on the ground or not. If snow depth is not used at all, I would find very distributing to have an algorithm that predict blowing snow occurrence without even checking if there is presence of snow on the ground.

P 4 L 97: does the reported height (3.5 m) correspond to the height of the wind sensor about snow-free ground? If it is the case, are the authors using the snow depth measurement to adjust the wind speed at a constant height during the course of the winter? This may have a slight influence if enough snow is accumulated below the wind sensor. See for example Vionnet et al. (2013).

P 5 L 107-115: It would be interesting to describe in this paragraph how the authors used the signal of the snow depth (SD) sensor to identify the periods when the surface was covered by snow. Did they use SD> 0 and another threshold value?

P 5 L 109: which threshold check was applied to relative humidity?

P 5 L 114-115: the author should explain in this section how they define a blowing snow occurrence. So far it is done in the discussion (L 456-457). All blowing snow fluxes strictly positive are considered so far. Trouvilliez et al. (2015) used a threshold of 1 g/m2/s to remove non-significant blowing snow occurrence when processing FlowCapt measurements. Can the authors comment on the sensitivity of their results to the value of this threshold? Such data processing could potentially improve the ability of the decision trees to detect the main blowing snow events.

P5 L 121-123: Compaction due to overburden is not affecting the properties of surface snow so that it is not impacting directly the evolution of the threshold wind speed. The term "viscous" is not really appropriate here and I recommend the author to remove from the text: "and are generally drier and less viscous than the deposited snow".

P 5 L 125: was it challenging for the authors to identify blowing snow events with concurrent snowfall? Indeed, the precipitation gauges installed at the ISAW stations must certainly suffer from large wind-undercatch during strong wind events which may potentially lead to no solid precipitation accumulating in the gauge.

P 5 L 130: Can the author explain how they identify the atmospheric conditions for melting snow? Did the authors only consider one hour of positive air temperature to identify melting snow?

P 5 L 130-132: the authors identified 3 types of atmospheric conditions when developing their algorithm. For a better understanding of the importance of these conditions, it would be really interesting if the authors could produce a graph or table that shows the frequency of occurrence of the 3 types at each station and at all stations combined. This graph or table could also show the frequency of blowing snow occurrence for each of the 3 types of atmospheric conditions at each station and at all stations combined. Such graph would highlight that blowing snow events are rare events (as mentioned by the authors at L 335-336). This is important to understand the evaluation metrics derived from the contingency tables.

P 8 L 197: Can the authors explain the definition of the mean and standard deviation in the context of the evaluation of the DTM? At the moment, it is not clear for a reader that is not familiar with DTM.

P 8 L 200: It the selected value of 0.8 is a classic ratio used when training and evaluating DTM? A comparison with a few references from the literature would be certainly appropriate here.

P 10 L 230-245: the tests using only single attribute such as air temperature or relative humidity do not make a lot of sense from a physical point of view. Based on the existing and extensive literature on blowing snow, it is well established that wind speed is the primary driver and a necessary condition of blowing snow occurrence. It would be interesting for the reader to present step-by-step and logical construction and tests of the DTMs: (i) tests of the mean wind speed (ii) tests of the benefit of using maximum wind speed since gust affects wind-induced snow transport (Naaim Bouvet et al., 2011), (iii) tests of using additional variables such as air temperature and relative humidity to account for their indirect effect on surface snow cohesion. Such reduced and logical set of tests would also make Figure 1 easier to read.

P 10 L 236-239: it would be interesting to add a few references from the literature to support this description.

P 14 Figure 5: The 'MR' metric is not described in this paper. (see also Fig. 8, and the rest of the text).

P 17 L 364: Is it only WSMAX which is considered here or the distributions of other features were also considered?

P 20 L 401: it would be interesting if the authors could discuss in this section how these decision trees could be used in blowing snow models. Indeed, as explained at L 265-266, the authors used different decisions trees as a function of the atmospheric conditions. The authors should explain how such concept could be applied in a numerical model.

P 21 L 432: In alpine snowpack, snow metamorphism is mainly driven by temperature-gradient (Armstrong and Brun, 2008). This sentence should be reformulated.

L 21 L 438: In Vionnet et al. (2018), the wind direction is used to identify the windward side among the virtual slopes surrounding the stations. On the windward slope, blowing snow occurrence is then computed by combining information on wind speed and information on simulated surface snow following the method proposed by Guyomarc'h and Merindol (1998). The wind direction is not used directly as an input variable of the algorithm used to detect blowing snow occurrence. I recommend the authors to modify the text.

P 22 L 469-480: in this section of the discussion, the authors compare the performance of their decision trees with the performances of a modelling chain for avalanche hazard forecasting that simulates blowing snow occurrence (Vionnet et al., 2018). This modelling chain has been evaluated at the same stations as those used to develop the decision trees. The authors should compare the performance of their decision trees with the simulation R3 used in Vionnet et al. (2018) that used the observed wind speed at the stations to drive the modelling chain. Instead, simulations R1 and R2 from Vionnet et al. (2018) and mentioned by the authors used the wind speed from the SAFRAN analysis that adds uncertainty in the simulated blowing snow occurrence. The fact that the DTMs used in this study achieved similar results to experiment R1 in Vionnet et al. (2018) cannot be used to support the claim made by the authors at L 480-482. As explained in my general comment, the best method to support such claim would be to carry out a systematic benchmark versus standard methods available in the literature.

P 23 L 520: can the authors comment about the availability of the code of the decision trees proposed in this study?

Technical comments

Text

P2 L 64: the paper by Lehning et al (2000) coud also be mentioned in this list of references:

Lehning, M., Doorschot, J., & Bartelt, P. (2000). A snowdrift index based on SNOWPACK model calculations. *Annals of Glaciology*, *31*, 382-386.

P 3 L 80: Gilbert et al by should be replaced by Guyomarc'h et al.

P3L 91: wind direction can be removed from this list since it is not used by the authors.

P3L 91: the acronym "ISAW" should be defined here.

P 5 L 129: "wet precipitation" could be replaced by "liquid precipitation".

P 6 L 146: is the word "package" missing after "scikit-lean"?

P 21 L 428: "microphysical" should be replaced by "microstructural".

Figure

Figure 1: This figure is really interesting. Can the author make the different subfigures slightly bigger?

The upper graphs on figures 2, 3, 6, 7 are hard to read.

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