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Earth System Dynamics
Discussions

Interactive comment on "Estimation of the high-resolution variability of extreme wind speeds for a better management of wind damage risks to forest-based bioeconomy" *by* Ari K. Venäläinen et al.

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We would like to thank the reviewer for the constructive and positive comments that help us to improve the manuscript. The first comment is related to calculation of weighting factors "p. 4, l. 14: the rationale behind the weighting factor should be explained"

In our work we first interpreted the terrain cover available from CORINE database to surface roughness values using the same methods as used e.g. in the Wind Atlas (Tammelin et al. 2013). We are interested in very high resolution spatial variation of wind speed in typically highly variable terrain mosaic composed of forests, fields,

lakes, clear cut areas etc. The detailed structure of wind flow in this kind of heterogeneous terrain is very complex (e.g. Dupont&Brunet, Forestry, Vol. 81, No. 3, 2008. doi:10.1093/forestry/cpn006); one dominant feature being rapid deceleration of wind when wind encounters forest edge. The main wind damage are found typically within distance less than 50 m from the forest edge (Peltola et al. 1999b). In integration of the so called effective roughness we have applied normal distribution having variance 150 m. With these assumptions the weighting of each grid is as demonstrated in Fig. 1. The weight of the closest grid square is about 11 % and the furthest grid square located 500 m upwind has the weight of 0.04 % only. With no doubt, this formula is a simplification of a very complex issue as the exact impact of roughness elements on wind flow depend beside terrain properties also on the characteristics of prevailing air flow. However, when aiming in computationally light applications all these issues cannot be taken into account and the approach selected here gives a realistic interpretation of the complicated issue. We would be happy to add the Fig. 1. (e.g. to appendix) and explanatory text to the manuscript.

The next comment, "p. 6, l. 33-34: the authors use 12 m/s as a wind speed likely to cause damage. Given the availability of the HWind model, it would seem interesting to provide examples of stands that would be vulnerable to such wind speeds. The same applies at l. 39. I believe the paper could be strengthened if this section was somewhat expanded."

Simulations using HWind would really give additional value for the study. Unfortunately for this study and for the Pyhätunturi test-area we did not have all the needed forest data. However, in our next studies we plan to expand our studies to cover whole country is respect of wind climate and apply HWind model for another test areas where we have needed detailed forest characteristics data.

The third comment relates to accuracy of weather station data " p. 7, l. 28-29. The authors point out some potential imprecisions of weather station data. It would be interesting to know to what extent this problem is present in the data base."

FMI has a three stage quality control system, first check is done at the observation station site checking the main instrument malfunctions, and next check is done before storing the data to database. This check includes e.g. comparison with the extreme values and temporal and spatial consistency. The final step is the manual quality control for those values that did not pass earlier steps. The quality control ensures that the values stored in database are realistic and can have occurred. However, quality control does not guarantee that the measurements are exactly correct. As well, quality control does not ensure the homogeneity of observations. The changes at measuring site and changes in instrumentation as well as, the changes of the height of anemometer installation can lead to discontinuations, break points, in observational time-series. These break points are relatively common also in wind observational time series like studied by Laapas and Venäläinen (2017). We are happy to add some more text about the reliability of wind observations.

Next comments: “p. 13: please provide units with column headings”. Yes, we will add. “p. 19: the title of the y axis should be changed since it represents 10 year return levels of maximum wind speed for two different approaches”. Yes, we will change. p. 22: it is mentioned that the figure includes only values > 11.4 m/s. Do you mean that the graphs were truncated at this value or that the whole statistics did not consider lower values? The whole statistics did not consider lower values. This was done because we were interested in high wind speed values and the direction distribution of these strongest winds.

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