

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.

Anonymous Referee #1

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The paper tests a relatively simple approach to characterize high wind speeds across large regions. Even though the paper does not bring new knowledge about wind behaviour, I believe it is worth publishing because it tests in a rigorous way an innovative approach to incorporate wind climate in managing windthrow risk.

The study context is clearly stated. Managing windthrow risk at the local scale requires one to have an estimate of the recurrence of strong winds on a given site. Some wind circulation models can provide that kind of estimates but can hardly be used in an operational context. Hence, the interest for alternatives is great.

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The methods are rigorous and seem to have been applied correctly. However, some additional details may be helpful. The proposed approach has been compared to Wasp, a model that has been used in many previous windthrow studies. Even though this is not a validation, it shows that, in most situations for the region, both approaches compare quite well. Comparisons with weather station data clearly demonstrate that the method is a major improvement over using regional estimates of wind speed.

Here are some minor comments: p. 4, l. 14: the rationale behind the weighting factor should be explained 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. 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. p. 13: please provide units with column headings 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

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?

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