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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Windstorms in the Northeastern United States" *by* Frederick W. Letson et al.

Anonymous Referee #1

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This study presents an identification of severe windstorms over northeastern North America. Objective cyclone identification and wind speed exceedance methods are used to identify these phenomena and their dynamical features and impacts are put into the context of the wider climatology. The aims of the paper are good and I believe it could make an interesting publication that is appropriate for the journal and would provide a good piece of analysis to partner the numerous similar studies over Europe. However, I believe many of the methods used are inconsistent with these other studies and are not sufficiently justified or argued for in the text. All the methods used are slightly different from those established in the literature, such as tracking at 700 hPa instead of 850 hPa, analysing wind gusts at 100 metres instead of 10 metres, and using an extremely high exceedance threshold of the 99.9th percentile instead of the

more commonly used 98th percentile. I would like to see some evidence for the choice of the methods. Furthermore, the introduction and framing of the paper (especially the first half) feels very incoherent and I believe requires significant re-structuring. In addition, the presentation of the figures feels clumsy and very difficult to interpret with numerous similarly styled lines and colours overlayed on similarly shaded fields, which needs rectifying. My full points can be found below. I therefore recommend that this study requires major corrections.

Individual points

1. L43 – I feel the reference here to Fig. 1a should illustrate the two different types of cyclone tracks you are describing. As the figure is for wind gusts it in no way illustrates the differences in the genesis location

2. L47-49. How is the influence of post-tropical cyclones in the USA consistent with Europe? Are less than 1% of cyclones also post-tropical in this region? Perhaps it is best to remove the 1% statement.

3. L61-63. Can a trend line or some evidence of the trend from the data in Fig. 1b also be included/quoted? Due to the large inter-annual variability of the data it is hard to tell from the figure you have presented that there is a positive trend. Furthermore, you quote the 98th percentile in the text (perhaps this refers to the results of Bronnimann et al., 2012), yet the figure in question (I assume Fig. 1b?) uses number of grid points exceeding the 99.9th percentile of U. Please clarify these differences, or make the figure consistent with the text.

4. Section 1.3. From your introduction there is limited evidence of where this study fits within the established research and scientific literature. I think it could be framed better to give a clear narrative as to where the gaps in the literature are and how this study addresses those gaps.

5. L123-124. I feel this statement is incorrect for the last 10-15 years with the in-

troduction of more advanced and homogenous reanalysis products. Please make it clearer how ERA5 is beneficial compared to previous generation products such as ERA-Interim, etc.

6. I am a little confused as to why the 100 metre level is being used throughout this study as you are specifically interested in damaging windstorms. You have justified this in the text, however I feel it may be better to be consistent with previous studies, which generally use the 10 metre level. Does using 100 metres lead to drastically different results to using 10 metres? Furthermore, as you are using near-surface ground observations as a validation, surely it would make sense to also use the 10-metre product from ERA5 as there could be significant differences in the 100m and 10m distributions.

7. As above, doing the tracking at 700 hPa instead of 850 hPa is also confusing to me. Using the Hodges method tracking is mostly done at 850 hPa. Are there specific issues with representing the flow in the orographic regions? Are all cyclones identified at 700 hPa the same as those that would be identified at 850 hPa?

8. I feel the methodology section can be condensed significantly. You introduce the tracking on line 148 and then describe it in detail, several pages later. It feels very incoherent and should be re-structured for conciseness.

9. L210 – you use the 99.9th percentile, what is the justification for this? As you mentioned in the text your method is similar to numerous other storm severity assessments, however most use the 98th percentile. Due to the relative short extent of data from ERA5, are there not chances that the 99.9th percentile is exceptionally skewed by very large events?

10. L223 – is it possible that by using such a high threshold some spatially large events ($p98<U<p99.9$) are missed and events with a very small area of $U>99.9$ are counted instead? It may be that in some of these cases could the large, yet slightly lower intensity events have larger impacts than small scale high-wind events.

11. L225 – a 14 day restriction seems rather large. Have you tested this criteria to see if any high impact storms are excluded as a result of this threshold? Several studies have shown that more intense cyclones are more likely to cluster (e.g. Mailier et al., 2006), therefore this could be removing events from your set.

12. L235 – further references are needed here such as Vitolo et al. (2009), Mailier et al. (2006), Pinto et al. (2014).

13. L265 – do you have specific requirements for each of these cyclone classes (i.e. genesis location). If so please state this in the text.

14. Fig 2. I find the layout and organisation of this figure very messy. The legends should be moved outside of the panel boundaries and all text on the panels be made clearer as it is very hard to decipher any of the information.

15. L317 – reference should be outside of brackets.

16. L348-349 – as discussed above. If your systems traverse your region of interest in $\sim$72 hours, why the 14 day separation? Please clarify this.

17. Fig 4 and throughout – is it worth referring to each storm by its ranking in the text and figures. I find it hard to keep track of which year is which through the text, this may be a simple way to avoid this.

18. L396-406 – would the authors be able to illustrate these results in some way instead of just giving a description. Furthermore, commonly dispersion is calculated as counts per month, or counts per winter season, and not counts per calendar year. Vitolo et al. (2009) Fig. 15 demonstrates how dispersion (and surrounding uncertainty) commonly increases with aggregation period and as these storms you are interested in are only features of approx. 6 months of the year is it likely that this dispersion value is representative?

19. Figs 6 and 7 – legibility of these figures is very difficult. Often green lines are plotted above an area of green (especially figure 7a) and also with text on the figures it

makes it very difficult to distinguish and correctly identify features. I would recommend a redesign of these figures to aid legibility.

20. Fig 7b and table 3 – are the mslp units in hPa the anomaly from the background field? This needs to be made clearer as the magnitude of the values are confusing.

21. L412 – again, do you have requirements for these cyclone classes. It would be useful to define these earlier in the text instead of just approximating by eye if they are one class or the other. This is also applicable for L416 where you state a cyclone transitions to the NE class.

22. L464-465 – please display this information somewhere in either a figure or table, with it being stated as is it feels unjustified. Also you state how all storms have RP >50 years in some location, is this evident in figure 8 as the colorbar does not extend beyond 50? Perhaps to make this clearer the authors should extend the colorbar beyond 50 years and then highlight the regions which exceed 50 years.

23. Figure 8 and table 3 – what is the uncertainty on the return period calculations? As with ERA5 there is only $\sim$40 years of data the uncertainty must be very large for the 100 year event. Please clarify and quantify this in the text and figures.

24. All figures – the visualisation and colour clashes at times makes for figures that are very hard to interpret. Fig. 8 the red lines of states over the map is almost impossible to clearly distinguish. Fig 6 and 7 the track lines an intensity circles are hard to see as they overlap and also clash with the background colours. Figure 2 has legends overlapping figure space and also text on figures that cannot be read. I feel a redesign of these figures is required to accurately present the authors results.

25. The storm severity metrics (following Klawa and Ulbrich, 2003) are introduced but in no way used in the analysis and I feel could be a strong contribution to the results. Performing an SSI-like calculation to compare ERA5 ranking to actual ranking would be a useful addition to the analysis. It would be good to compare the U_999 spatial

extent and maximum wind speed to this storm loss metric.

References

Mailier, P. J., Stephenson, D. B., Ferro, C. A., & Hodges, K. I. (2006). Serial clustering of extratropical cyclones. Monthly weather review, 134(8), 2224-2240.

Pinto, J. G., Gómara, I., Masato, G., Dacre, H. F., Woollings, T., & Caballero, R. (2014). LargeâĂŘscale dynamics associated with clustering of extratropical cyclones affecting Western Europe. Journal of Geophysical Research: Atmospheres, 119(24), 13-704.

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