Review for Letson et al.

The authors have addressed the comments previously and I am satisfied with their justification of the methods used and also the additional included analysis regarding the p98 and p99.9 disparities. I still feel that the manuscript is in some places confusing and is not particularly easy to follow. Sections 3.2-3.4 I had particular difficulty following and a few specific points are detailed below. I would recommend the authors try to increase the clarity of this final part of the results as I believe it is very important in quantifying the differences in the impacts and loss potential for the different classes of cyclone. I am particularly happy with how the authors have re-phrased the introduction and methods of the paper. Below I have included some more detailed points that I would like to see addressed.

1. In the abstract it may be worth adding a statement as to the purpose of the study (similar to what is in lines 102-104)
2. Lines 21-22 and 440-443. You state that the value for D is less than in Europe, however the method used for the cyclone counts will likely provide a value of D that is different from the previous studies. Can you therefore state that the D value of 0.18 is less than what you would see over Europe using your method?
3. Lines 35-37: is it also possible to include a similar figure that is just a result of windstorms or extratropical cyclones? Or is annual data like this not obtainable?
4. Line 45: You state 'Previous research has found that these cyclones…' I assume this is referring to the Alberta Clippers, but could the authors please make this clearer in any potential cases that a reader thinks a Colorado Low and be north of Lake Superior.
5. Line 115. This last part of point 3 is a repeat of the first line of point 3.
6. Line 222-223: In the analysis do you apply any threshold for the wind speed exceedance being a set distance from the cyclone (You discuss how this is part of the XWS analysis on line 204). It is probably unlikely that due to the extreme threshold employed that there will be any exceedances that are not a result of the cyclone, but it should be mentioned whether or not any distance criteria is used.
7. As the comment above, are any thresholds used to associate precipitation to the cyclones, there may be considerable separation between the windstorm, cyclone, and maximum precipitation and is something that should be addressed.
8. Line 318-320: Would it be possible for the authors to put the figure from the first round of reviews in the appendix/supplement and reference here?
9. In Fig. 4 can the bins be changed for the max. precip charts? As so much of the data is skewed toward low precip rates some more detail max be gained from limiting the y-axis to 20 or 30 mm.
10. Line 440-445: Some of the studies you have referenced (especially Mailier et. al., 2006) show that D is often negative in parts of the northeastern USA, indicating regularity. Would the authors be able to hypothesise/discuss at all why these 50 storms indicate a clustered behaviour.
11. Figure 6. Is the heatmap of cyclone density over the NE USA just for cyclones that pass through your area of interest or is this a total climatology. This needs to be stated in either the caption or the text. I also find it very hard to identify the different sized circles in (c) and (d), as a suggestion it may be clearer to just include the circles when above the XXth percentile? Another suggestion for this figure as I

still ifnd the different coloured lines very hard to distinguish, would it be possible to have the different cyclone types as varying shades of the same colour?

12. Line 449: Please rephrase the start of the sentence ‘Also in accord with expectations…’ as it is unclear if you are referring to the climatology of tracks or your 10 intense windstorms.
13. Line 456: would you be able to quote some values here? E.g. most cyclones exceed 10x10**-5 whereas the climatology is approx. 1x10&**-5?
14. Line 461-462: I assume you mean that the NE USA is dominated by CL-type cyclones?
15. Section 3.2. One thing that may be discussed is what stage in the lifecycle are the cyclones at when they cause the most damage/are over the built-up regions. This is information that is included in Fig. 6 but not really discussed by the authors. This may be something that separates the high loss storms with those of lower losses.
16. Line 501: Is the median RP the median RP for all cells in the NE USA region? If so is this including a lot of grid points that are not part of the windstorm/cyclone area and substantially lowering the RP? This ties in a bit with points 6 and 7 above as to setting a cyclone area.
17. Section 3.4: I understand that the authors have added this in in response to the previous round of comments, however I feel it does not add any substantial discussion to the paper in its current form. Perhaps the figure included in the responses would be a better option to illustrate the differences between the loss model and the actual losses and the role of other hazards?