Response to Comments

The specific comments have, for the most part, been accepted and the appropriate revisions made to the paper. Individual responses are given below. Here I will address the general comments. The reading suggested by the reviewer has been included in the paper, which now contains 42 references. Given the short length of the paper, I do not believe it is possible to cover much more of the literature in this short study. The work does refer to a widely accepted text book from 2002 on the statistical methods used and, thanks to the reviewer, it includes a review paper from the 90s on deriving extreme wind speeds. It also references several works from the last decade which have done similar studies (e.g. Beniston et al. 2007, Kunz et al. 2010, Nikulin et al. 2011, Pryor et al. 2012a, etc.). I appreciate that the literature on this subject is vast and I do not suggest that I have complete covered all of it, but I do believe the literature reviewed here is sufficient to support this work.

Regarding the typos errors in the work, the paper has now been checked by two people who have English as their natural language, and minor correction have been made to clarify the language at some points. I believe these have now been addressed.

The abstract does state the conclusions drawn from the analysis. (i.e. the errors associated with the extreme wind estimates are larger than the future change predicted by the models, and that the largest source of uncertainty is the inter-model spread.) We examined the A1b scenario for two reasons: firstly, I believe it is the most realistic scenario based on observations since the CMIP3, but I accept that there is some debate regarding this; and secondly, because it was what I had available. The ENSEBLES project was a downscaling of the A1b scenario and I am using the downscalings that are available to me. If I started this analysis today, I would use the new CORDEX downscalings, which include the RCP4.5 and the RCP 8.5 scenarios. This will most likely be covered in a new study later this year.

The comment in the conclusions regarding the areas of the domains that failed the likelihood ratio test refers only to a methodology that was tested and not used. The method would remove the uncertainties that arise from estimating the shape parameter if it were applicable. However, the method was not included in the results and the conclusions are not based on it. The comment regarding the method has been included so as to make it clear that this alternate approach was tested. Hopefully this clarifies the meaning of this comment, but obviously I would be happy to discuss it further if there is still some doubt, and change it should it be deemed valuable to do so. The reviewer has asked about the added value of “using such ensembles.” Perhaps the reviewer could clarify this question. The study includes no ensemble of data. Each of the downscalings has been examined on its own and they have not been combined or averaged in any way.

Response to Specific Comments

L36 Changed as suggested.

L40 Reference to the WMO report and summary of the cold wave of 2012 is given.

L53 This has been changed to be self-explanatory.

L55 Very good point. Coming from a background of modelling myself I instinctively assumed that the benefits of RCMs were known to all. I have added 4 sentences stating the benefits of RCMs to correct this oversight.

L59 I believe “Unfortunately” is a reasonably good description for the situation. To complete the task they are designed for, they need to be one of the last steps in the chain. This means that no matter how good a RCM is at modelling the dynamics and physics of the atmosphere, they will always have a huge uncertainty associated with their predictions of future change before they are even run. However, I do take your point that it is part of the design and thus I have rephrased the statement in the final version to be “In point of fact, regional climate models are near the end of a long chain of predictions…..etc.”

L67-73 The suggested reference has been included.

L110 Changed as suggested.

L111-2 Exact length of periods given at this point as suggested. A work by Cook in 1986 suggested that at least 20 years of data were needed to reliably derive a 50 year return event. This was also used by other works on the subject (e.g. Pryor et al. 2012). In this study I have used 30 years of data derive my 50 year return estimates.

L138 The wind speeds used in the study were the maximum daily 10m wind speeds. I have adjusted the section on source data to make this clear. I have also noted it a various points later in the text and in the figure captions. The last paragraph of the introduction stated that “..the 50-year return winds (U_{50})..” I have changed this to state “..the 50-year return value of the 10 m winds (U_{50})..” This change should not only further clarify what U_{50} stands for, but also makes it clear that I am examining 10 m winds in this study.

L138 The ENSEMBLES data provides a good source of RCM output since it consists of multiple GCMs downscaled by multiple RCMs using a standard domain and grid. This makes the resulting downscalings easy to compare. This also allows for an easy comparison of parameters derived from the different downscalings by applying a standard method; in this case the 50-year return value of the 10 m wind speed.

L369 I have not updates the table as suggested. One of the benefits of using the ENSEBLES data is that the horizontal resolution of the models is all the same so there is no need to list it on the table for each model separately. I have not listed the altitude of the first model level since I am using 10 m wind speeds. This was not made clear in the original draft but that has now been corrected so I think it makes sense not to include the first model level. Finally, since I am using the maximum daily 10 m wind speed from all the models, the archival times are the same for my data in all cases.