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Interactive comment on "Human influence on European winter wind storms such as those of January 2018" *by* Robert Vautard et al.

Anonymous Referee #2

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General comments:

This manuscript compares observed trends in extreme values of near surface wind speed over specific areas in Europe with trends predicted by climate models. This is an important topic that fits well with the scope of Earth System Dynamics journal. The observed trends are calculated based on measurements from stations located in unevenly spaced locations over Europe. For the statistical analysis of the observations the authors use the data from all stations in two specific latitude-longitude boxes, which roughly include the main areas affected by two specific storms during January 2018. For the analysis of trends in climate models, the authors use the same boxes used for the observational analysis, but the data is from the evenly spaced grid points in the box. The trends in four different ensembles of climate models are compared. The

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Discussion paper

trends are evaluated using a numerical fit of the data to a generalized extreme value function (GEV). The results presented in this manuscript show a contradiction between the negative trend in extreme values of near surface wind speed seen in observations and the positive trend predicted by climate models. The authors suggest that this contradiction arises from factors that are not taken into account in climate models such as changes in the surface roughness and aerosol concentration. Though the results are interesting and the mathematical analysis is appropriate for the purpose of the research question, I think some choices in the analysis methods are not well justified. Specifically, the choice of the boxes is not well justified, nor is any sensitivity test presented for this choice. In addition, I found the text and some of the figures to be unclear in several places of the manuscript and some of the data are not properly explained. I therefore recommend on major revisions, according to the comments below.

Specific comments:

1) The focus on the two storms "Friederike" and "Eleanor" is understandable from the point of view of the motivation. However, I see no added scientific value in using the specific locations and maximum wind speeds of these storms as the criteria for comparing observed trends in wind speed with the predicted model trends. Instead, the comparison could be made over various location in central Europe and referring to general trends in extreme wind speed. This would justify the more general conclusions drawn from the results. 2) In accord with comment 1, I suggest shortening section 2, which describes the storms of January 2018 in detail. Figures 1 and 2, which are discussed in this section, describe the intensity of these storms and the specific meteorological conditions that prevailed during that stormy month. These conditions are not discussed any further and are not related to the analysis in the rest of the paper or to the conclusions. I therefore suggest to remove them (especially figure 2). 3) Figure 3 shows the locations of the stations from which the observed data is derived, as well as the boxes chosen for the analysis and the values of the maximum wind speed during the two storms. I didn't find in the text a justification for the choice of the boxes,

except for a general statement that wind speeds were largest in these boxes for the respective storm. The boxes are also indicated in figure 1, however, they do not exactly cover the regions of strongest wind speeds. My two concerns regarding the use of the station-based data statistics over the boxes are: a) Whether a different choice of the boxes would change the results and the conclusions. b) Whether the station-based data is comparable with the grid-based data of the models. I therefore suggest that the authors add a sensitivity test to justify this choice. 4) Table 1 is not organized in a clear way. there are two titles for each column, and it is not clear which numbers in the cells refer to which title, not are the initials in the titles defined. Also, not all the initials of the model ensembles are defined in the text. 5) It is not explained what the KNMI dataset is based on (perhaps satellite measurements?) and for which of the analysis shown in the manuscript it is used. 6) The four ensembles are presented without an explanation for the choice of ensembles and what each of them contributes in addition to the others in terms of the goals and conclusions. 7) Line 28 page 7: The meaning of "winter maximum of the daily maximum of three-hourly maximum wind" is not clear. 8) The description of figure 4a (page 7) refers to the x-axis as time, but it is actually temperature anomaly. It is not explained how the time series is converted to a function of temperature and for what purpose. 9) Line 2 in page 8: what do the values in the parenthesis mean? what is CI? Also, in many other places in the manuscript there are values in the parenthesis without any explanation of their meaning. 10) Lines 20-21 on page 8: "The observed indicator value. . . has a present return period of about 13 years. . . which is longer than for the observations". The phrasing is confusing. If by "present" the authors refer to the simulations of the current climate then this should be mentioned explicitly. Secondly, it is mentioned earlier that the return period in observations is about 20 years, which is contradictory to this statement. 11) Figure 5: the caption doesn't explain what the dashed lines indicate. 12) Line 23, page 8: Define "probability ratio". What are the two probabilities for which the ratio is calculated? 13) Last sentence in page 8: the comment about the increase of variability is not clear. 14) The analysis of the HadGEM3A ensemble: It is explained that since this model only

produces daily mean values it is difficult to detect storms such as "Friederike" with this model's data. If so, it would make more sense to either: a) examine the general trends in extreme wind speed, without referring to this specific storm. b) removing this data from the manuscript. The same goes for the weather@home data. 15) Line 12, page 9: It says that in the EURO-CORDEX ensemble the PR is generally not significantly different from one. It is not clear what this statement is based on. The PR values in figure 7 are similar to those in the figure 5 for the RACMO ensemble, for which the authors did identify values significantly different from one. 16) Discussion of figure 14: The choice to scale the probability with CO_2 concentration instead of temperature is not well justified or discussed in the text. 17) Section 6: There is not enough discussion of the problems in comparing the observations and model data. Specifically the consequence of having a very different concentration of data points in the two sets is not discussed and the fact that the observations end at 2018, while the climate models continue to much longer periods (or predictions) with a much stronger climate change signal is not discussed.

Technical corrections:

1) Line 29 on page 5: I assume that the second future period was supposed to be [2041-2070]. 2) Line 5 on page 6: the HistoricalNat ensemble is not defined (maybe these are the same simulations defined as natural/counterfactual?). 3) "Global mean temperature" is used in line 29 in page 6 and in figures 4 and 9, instead of "global mean temperature anomaly". 4) The font size of the figure labels and legend is too small. 5) Caption of figure 12: change to "same as figure 9".

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