We thank Gerard van der Schrier for his comments and suggestions, which helped to improve the manuscript and to remove ambiguities/misunderstandings. Below are point-to-point responses to each comment, including plans how to incorporate them in our manuscript.

Major concern

Line 105-110. There is a bit of concern on the parameterization of wind gusts from the reanalysis data. One approach is to use the local near-surface wind speed and its standard deviation in order to estimate the gust (like the Panofsky et al 1977 approach used in the manuscript). This approach makes use of similarity theory, and relates the gust to the friction velocity. The approach performs well in flat terrain, but is sensitive to the parametrization of the local roughness length. The accuracy of the estimated gusts relies heavily on the roughness map that is used, especially when the resolution of the NWP model increases and detailed information about the land-use (and the associated roughness lengths) is required. Errors in the supplied roughness lengths will directly influence the calculated gusts, which is a disadvantage of this approach.

My suggestion is to add a brief analysis where the Panofsky et al (1977) approach is compared to an alternative approach which is specifically suitable for use in a reanalysis product (van den Brink 2019). It links the 1-hour wind speeds at height (which is a standard output of the reanalysis) to 10m wind gusts. This comparison can be done over the entire domain or for specific storms.

To complete this analysis, actual observations of wind gusts should be combined to this assessment. Wind gust values for Europe can be obtained from the European Climate Assessment & Dataset at www.ecad.eu. If you have troubles finding the right data, simply contact ECAD staff.

This additional analysis assess the quality of the parameterization used for the wind gust calculations which is central to this study. The quality of the parameterization is therefore essential and requires a bit more scrutiny that the brief comments that is currently found in the manuscript.

Answer: We believe the reviewer may have misunderstood us here. We did not apply a wind gust parameterization ourselves. Instead, we use the officially published wind gust data from ERA5 and ERA-Interim, which is based on the parameterization approach by Panofsky et al. (1977) and Bechthold & Bidlot (2009). In the present study, we compare for the first time a simple meteorological index to the output of a full insurance windstorm model. Therefore, the assessment of the quality of wind gust parameterization itself is not the aim in our study. However, we would like to note that we have performed such assessments before in dedicated studies, e.g. by comparing different wind gust estimation methods (Born et al., 2012) and by comparing wind gust data from reanalysis to station observations (Seregina et al., 2014). Other authors (e.g. Minola et al., 2020) have also performed such analysis. We will clarify this in the revised manuscript and include some discussion on the effect of wind gust parameterization on the differences between LI and the Aon model output.

Other aspects

Section 3.1: fig. 2c shows that ERA5 generally has higher wind gust values than ERA-Interim, but the strongest differences are found over area with complex topography, like the Pyrennees, Alps, Norwegian coast and (perhaps) the Scottish highlands. The higher values over Europe are likely related to a mix of better physics and higher spatial resolution - as the authors correctly state. Could you explicitly state the spatial resolutions of ERA5 and ERA-Interim in the section where they are introduced? Now the resolution of ERA-Interim is mentioned on line 308 in the very last section of the manuscript. With a coarser resolution, complex topography will be much less well represented and peaks and vallayes will be less pronounced which directly affects the wind gust. Perhaps good to make this explicit in the discussion of the reanalysis.

Answer: The spatial resolution of the different datasets is already stated in the Data and Methods section – in lines 104 and 106, respectively. We agree with the reviewer's comment that the effect of the different spatial resolutions can be important. Thus, we have decided to add some analysis (and corresponding discussion) with using ERA5 re-gridded to the ERA-Interim grid. See also detailed comment further below.

Line 204-206. I am afraid that I fail to see why storm Irina is such an outlier. The Loss Index for ERA-Interim is for this storm much larger than for ERA5, but the storm footprint (fig. S1) does not really show a much larger region where the footprint != 0 over the UK. ERA5 does show higher values (mostly because of the high resolution of ERA5 I guess). So, what do I fail to see in the explanation?

Answer: We thank the reviewer for this comment. The footprint for Irina is overall flatter in ERA5 compared to ERA-Interim. This is particularly the case for the UK, where the mean wind gust over land is 12.1 m/s for ERA5 and 24.6 m/s for ERA-Interim. Therefore, the LI for ERA-Interim is higher due to the cumulative effect (summation of v/v_{98}). Additionally, Figure RC1.1 shows an extension of Figure S1 including another panel showing the ERA5 footprint re-gridded to the ERA-Interim resolution. This confirms the overall flatter structure of footprint. We agree this is an important point and we will add more details and Figure RC1.1 in the revised manuscript.

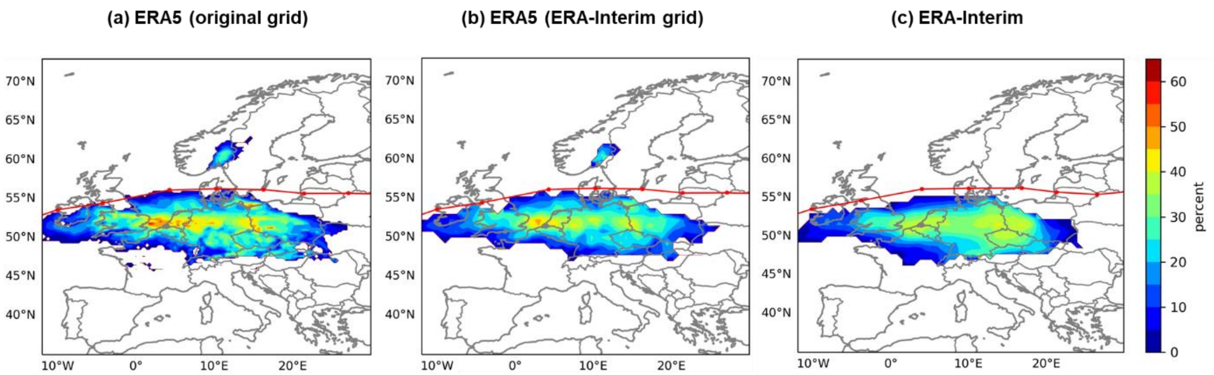

Figure RC1.1: *Wind gust footprint for storm Irina in October 2002 based on ERA5 (a), ERA5 re-gridded to the ERA-Interim grid (b), and ERA-Interim (c). Shown is the largest exceedance (in percent) of the 98th percentile of daily maximum wind gust within 72 hours. The red line and dots denote the cyclone track derived from ERA5 (a, b) and ERA-Interim (c) using the tracking algorithm of Pinto et al. (2005).*

Figure 3 and 4: it would be interesting to add an analysis where ERA5 is first regridded to the ERA-Interim resolution, and then the LI diagrams are made. This analysis gives a clue if it is the improved physics in ERA5 that makes the difference or that the increase in spatial resolution makes the difference. This would be nice to add to the Supplementary material. This analysis could then provide the basis for Section 5, bullet 1: I have not seen evidence that it is the resolution that makes ERA5 better than ERA-Interim (although this is likely).

Answer: We thank the reviewer for this comment. Figure RC1.2 shows the LI for ERA5 re-gridded to the ERA-Interim resolution for different regions, compared to the original Figure S2. After re-gridding, LI ERA5 and LI ERA-Interim are in the same order of magnitude, while the overall behavior/order of storms does not change (as can be seen by the small changes in R^2). This confirms that the different resolution of the datasets is not decisive – and that differences may most likely result from differences in the wind gust distribution (see also Figure RC1.3). We will add the right part of Figure RC1.2 to the Supplementary and enhance the discussion in the revised manuscript.

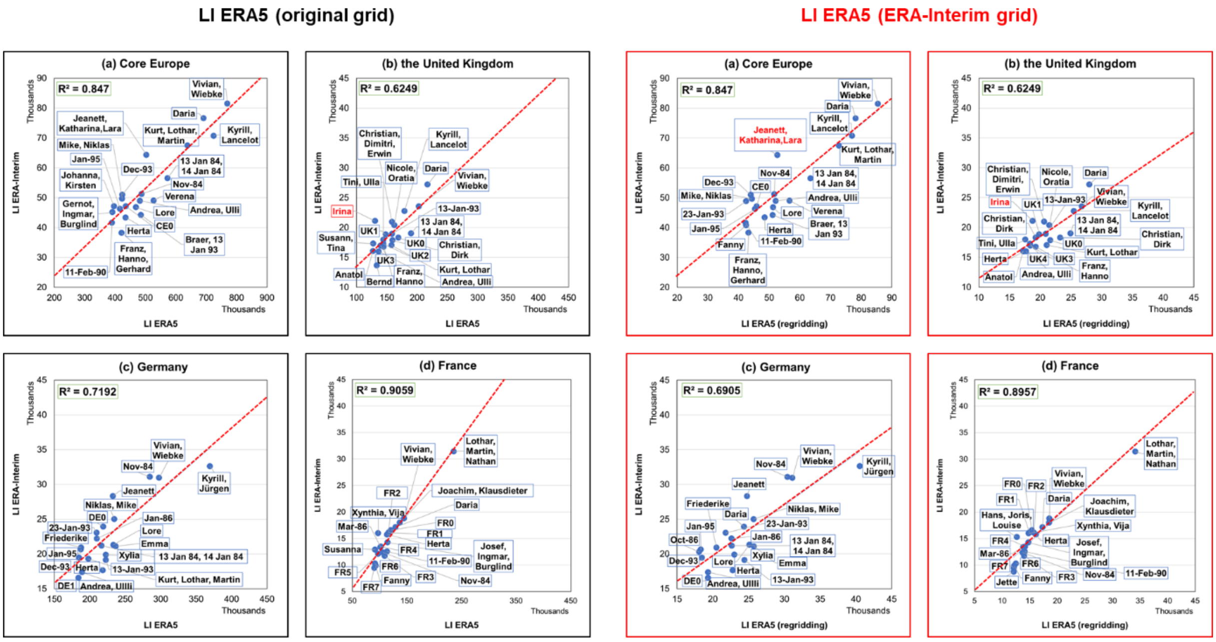

***Figure RC1.2:** Comparison of loss values (in thousands) based on LI ERA5 (x-axis) and LI ERA-Interim (y-axis) for the common 20 most extremes storms in the period 1979-2019. LI ERA5 is calculated from the original ERA5 gust data (left) and from the ERA5 gust data re-gridded to the ERA-Interim resolution (right).*

Section 5: bullet 1: the distribution of wind gusts may be shifted right in ERA5, but the footprint uses the 98th percentile - which is also shifted right. So this argument does not make sense.

Answer: The reviewer is correct here – a shift in the wind gust distribution also implies a shift in the 98th percentile and therefore cannot explain the higher LI values for ERA5 alone. What we meant is that besides the overall shift in the distribution, the tail of the distribution is longer for ERA5, which then leads to higher LI values. To illustrate this, Figure RC1.3 shows both the 98th and the 99.9th percentile for ERA5 and ERA-Interim as well as the difference. The figure clearly shows larger differences for higher (99.9th) percentiles, thereby confirming the longer tail of the distribution in ERA5. We apologize for the misunderstanding. We will replace Figure 2 by Figure RC1.3 and clarify this point in the revised paper.

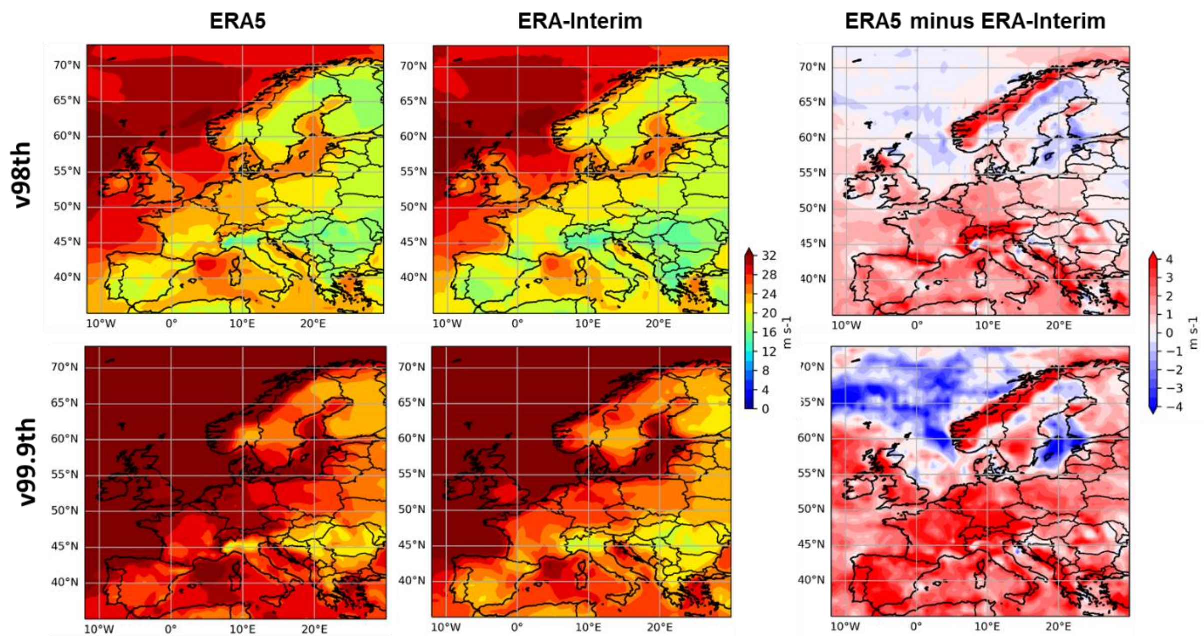

Figure RC1.3: *98th percentile (upper row) and 99.9th percentile (lower row) of daily maximum wind gust for the winter half year (October – March, ONDJFM) for the period 1979-2019.*

Figure S1: In the caption of the figure you write ”Shown is the percentage of the maximum wind gust in 72 hours that exceed the 98th percentile of daily maximum wind gust.” If you aim to show the outcome of equation 2, the this should be something like ”Shown is the strength of the maximum wind gust in 72 hours as deviation from the 98th percentile and normalized with the 98th percentile.”

Answer: We apologize for the misleading figure caption. We will change it to “Shown is the largest exceedance (in percent) of the 98th percentile of daily maximum wind gust within 72 hours” in the revised manuscript.

References

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