

Review of "Insurance loss model vs meteorological loss index – How comparable are their loss estimates for European windstorms?" by Julia Moemken, Inovasita Alifdini, Alexandre M. Ramos, Alexandros Georgiadis, Aidan Brocklehurst, Lukas Braun and Joaquim G. Pinto

The authors describe a comparison between reanalysis-based Loss Index with losses obtained from the European Windstorm Model of Aon Impact Forecasting. In addition, two different reanalysis products (of different generations) are compared to assess the impact of the reanalysis on the actual Loss Index. The study addresses a very interesting subject and the ability of the authors to compare reanalysis-based Loss Index with the Aon Impact Forecasting model is an opportunity and very relevant for the community. However, I found the manuscript a bit difficult to follow at times but perhaps more importantly, I am concerned about the representation of the wind gusts that is used in the manuscript. The authors are right with their statement on line 137 that "the maximum daily wind gust speed is assumed to be the most relevant factor in these models". My most serious concern relates to this. A bit more detail is provided below. There are a few other issues the authors may want to look into.

My advice to the editor is to accept with major revisions.

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 than the brief comments that is currently found in the manuscript.

Other aspects the authors may want to look at:

- 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.
- 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?
- 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).
- 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.
- 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."

REFERENCE

van den Brink, H. W. (2019). An effective parametrization of gust profiles during severe wind conditions. *Environmental Research Communications*, 2(1), 011001. DOI 10.1088/2515-7620/ab5777