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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Spatial variability and potential maximum intensity of winter storms over Europe" *by* Michael A. Walz and Gregor C. Leckebusch

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We would like to thank the reviewer for their valuable comments on our study. In the following we would like to address every comment individually and present our opinion and also indicate changes that have been made.

1. I cannot understand the advantage of employing ECMWF operational climatic predictions (system 4) to study the windstorms on a climatological basis and not reanalysis datasets, such as the ERA-20C at a similar resolution? Recognizing the meritof the great data amount, how the authors are confident about the reliability of these datasets?

The great merit of using the seasonal hindcast ensembles is the shear amount of data. Reanalysis products (like ERA-20C) only entail around 100 winter seasons, whereas with the seasonal hindcasts we obtain more than 1500 seasons. There have been studies investigating the quality of wind(storms) in ECMWF System 4 (e.g. Befort et al., 2018 or Walz et al., 2018b). The overall conclusion is that despite the reliability not being perfect they feature skill in year-to-year variability and spatial coherence. The reliability of reanalysis products, especially in the beginning of the observation period, is also not entirely given. Befort et al. (2016) for example found strong trends for the first 50 years of windstorms in ERA-20C. We have added a bit of explanation why we think the seasonal hindcasts represent a great addition to climatological data and hope this answers the reviewers question

2. Following the same comment, I cannot understand statement in page 3"That way the ensemble serves as a unique data archive which can be used to assess the statistical uncertainty more precisely compared to exploiting observational reanal-ysis data for this kind of estimation". The authors should clarify and verify this point,since determines the novelty of the paper as compared to previous related studies. In any case, this statement is not discussed or verified in section 4.

Similar to above, just based on the pure amount of data statistical inferences can be made more easily. We have added more explanation in the text, especially in section 4.

3. Similarly, the authors should clarify the statement in page 2: "Clearly none of the windstorms found in these forecasts ever happened, however each of them represents one possible physical consistent realisation of a potential reality"

We are not entirely sure what the reviewer wants to be clarified here. All the storms that we could track in the data are "fictional". Some might be similar to a storm that has already happened in reality, some might happen in a similar way in the future. The seasonal forecast itself however does not try to "replicate" storms that happened in

reality.

4. In section 2: “31 years of data, is equivalent to 1581 “virtual” years”. Please clarify. Changed years to winter seasons. 31 years times 51 members equals 1581 trackable winter seasons.

5. The spectral resolution of th edata is T255. Is this the same with reanalysis data ERA-interim or ERA-20C?

Same as ERA-interim.

6. In section 2: “...This is implemented by only taking into account windstorms that affect a country at least once in their lifetime, i.e. by defining a radius around a country/an area through which a windstorm or a cyclone has to pass”. Please clarify. How this radius is defined? Is it defined a priori?

The radius is defined a priori by choosing a centre point within a country and setting the radius so that it encompasses the entire country. Naturally small bits of other countries are included sometimes, this, however, does not pose a problem as a storms also do not care about political borders. We have added a figure in the supplements showing the radii for the different regions.

7. Section 2: As the cyclones identified by the Murray and Simmonds algorithm are not necessarily extreme in terms of impact,the minimal core pressure and the maximum curvature of an identified track both have to be within the lowest respectively highest 5% of all tracks at least once within the defined radius” I think this percentile is somewhat arbitrary. Is it based on statistical analysis? Or in previous studies?

In a way it is arbitrary. However it represents a good compromise between choosing enough events to still be able to produce significant statistical analysis and to choose MSLPs that only occur only in every 20th on average. The 95th (or 5th) percentile has been used in previous studies as an indicator of “extremeness” (e.g. Walz et al., 2018b or Della-Marta et al., 2009)

8. What do you mean by “local maximum” or local 98th percentile” in practice? At every grid point? Yes it is a grid point based approach. So the tracking is based on the local 98th percentile to account for geographically different wind climatologies. Please refer to Leckebusch et al. (2008) for more details.

9. In section 3: apart from central pressure and curvature as measures of the cyclone intensity, the local pressure drop is an important measure that determines intense and mainly explosive cyclones that are responsible for wind storms.

Indeed that would also be an interesting thing to investigate as it has been done in numerous studies before. However we decided to focus on the two characteristics. We have changed (or transformed) the curvature however into means of a Rossby number to make the numbers a bit more tangible as especially the unit of the curvature seems very abstract.

10. Section 4: From Table 1, I assume that the 3 clusters are identical for all 4 regions. Is this true? For this reason the clusters are displayed fort he two regions?

You are correct, the three found clusters are identical for all the regions, that is why we chose only two show results for the two presented areas. We have added that in the manuscript.

Other comments 1. Abstract: Main findings are missing. A large part is devoted to explain the advantages of using seasonal forecasts. This is not the scope of the abstract. Changed the abstract.

2. Section 1: why the ETCs responsible for windstorms in Europe have their origin over the NW Europe? A reference is required. Added a reference.

3. Section 1: “..they usually follow an eastward trajectory”. Similar comment See above.

4. Section 1 is not structured in paragraphs. The overall structure was not great, we have added paragraphs throughout the manuscript.

5. Section 2 is not structured in paragraphs. For instance, in page 3, line 13 a new-paragraph could start with the statement “Two different types...” The overall structure was not great, we have added paragraphs throughout the manuscript.

6. Since section 2 is entitled “Data” , the authors should focus only on the data used. The remaining part refers to methodology that is described in section 3. Therefore, section 2 and 3 should be reformulated

7. Section 2, page 4, line 4: “Due to the constraint for the tracked cyclones..” What do the authors mean? This is a reference to the 95% (5%) constraint of only using the top 5% of extreme MSLP and curvature/Rossby number. We have changed the wording accordingly.

8. Section 5 is not structured in paragraphs. For instance, in page 12 at lines 14, 24 and page 13 at lines 7, 23

Changed the formatting.

9. Legend of Figure1: Replace “Km2” by upper case “Km2”

Changed it.

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