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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Revisiting the synoptic-scale predictability of severe European winter storms using ECMWF ensemble reforecasts" *by* Florian Pantillon et al.

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Summary

The study assesses the predictability of severe storms over Europe in the most important season winter using the ECMWF ensemble forecasts. The authors concentrate on 25 events in the period 1995 to 2015 applying different metrics finding that these high impact events are predicted with skill up to 4 days. They also find skill for the area covered by these extreme events up to 10 days which may provide early warning opportunities. Still, the limited sample of only 25 storms shows strong inter-case variability. The small sample is a clear drawback of this study as it limits the reliabil-

ity of the deduced skills and the author tend to overemphasize the results. Still the manuscript is nicely written and well structured. It certainly contains new findings, which are fruitful for how to identify predictive skill for extreme events, so I certainly see that the manuscript is suitable for NHESS, if my minor to major comments are treated seriously.

We thank the reviewer for his/her comments on the manuscript.

We will address all the comments below. In particular, we will clarify that we explore the physical characteristics of some outliers that exhibit a particularly high or low predictability and avoid any suggestion of a systematic link between dynamics and predictability among the sample of storms. We will also detail and discuss the representation of wind gusts in the ensemble reforecast and in the reanalysis datasets. We hope that these revisions will better support the results of the paper.

Comments

P1,L9: Please change to 'potential for an early warning'.

We prefer to change to "potential for early warnings".

P2,L1-7: You may add the study of Stucki et al. (2014, Nat. Hazards Earth Syst. Sci.) here.

We will cite the Stucki et al. (2014) paper in the selection of storms, as stated below.

P2,L29: Please change 'manuscript' to 'study'.

We prefer to change "manuscript" to "paper".

P3,L15-16: As wind gusts are an impor- tant metric used in this study, you need to explain how this is derived in the reforecasts and how these gusts compare to observations.

In both the ensemble reforecast and ERA-Interim, the wind gusts are computed from

the wind speed on the lowest model level and a turbulent component based on a similarity relation between the variability of the surface wind and the friction velocity. In the ensemble reforecast, which uses a more recent model version, the computation of wind gusts includes an additional component based on the low-level wind shear in convective situations. This additional component is expected to contribute to the strongest wind gusts when convection is embedded in the cold front. The resolution of the ensemble reforecast and ERA-Interim is known not to be sufficient to capture the strongest gusts due to mesoscale structures such as sting jets and to steep topography. However, as the focus here is on synoptic-scale aspects of winter storms, these limitations are likely rather unimportant. The comparison with ensemble forecasts remains fair, because their horizontal resolution is not sufficient to capture the strongest gusts either, and because the verification of wind gusts is based on values relative to the model climate rather than on absolute values.

We will add a paragraph in Section 2.1 to detail and discuss the representation of gusts in the model data.

P3,L24-25: How do the selected European wind storms compare to the storm catalogue provided by Stucki et al. (2014, Nat. Hazards Earth Syst. Sci.).

We will discuss the focus of the selection of storms on the United Kingdom due to a fixed threshold for wind gusts above 25 m s^{-1}, which is less often exceeded over continental Europe. We will further mention the Stucki et al. (2014) paper as another catalogue based on alternative criteria for the specific region of Switzerland.

P4,L13: It would be nice to include the publication by Raible et al. (2008) who were the first to inter-compare cyclone tracking methods.

The publication by Raible et al. (2008) will be included.

P4,L16: Please change to 'Neu et al. (2013) emphasized. . .'

We will implement the suggested change.

P5,L11: It remains unclear which level is used for the wind – is it 10-m wind? Another question is whether the authors use wind gusts as v_max *or sustained wind. If the authors use wind gusts they need to include a discussion on the parametrization used.*

As stated above, we will add a paragraph in Section 2.1 to detail the representation of gusts in the model data.

P5,L17-18: This could also a problem of the wind gust parameterization and not just a problem of the spin-up of the model. Stucki et al. (2016, Tellus) showed this how different gust parameterizations work over complex terrain showing strong changes from one to another parameterization.

We will cite Stucki et al. (2016) in the discussion of Section 2.1 about the representation of gusts over complex terrain. However, the problem seems to be different here, as it occurs in the first 6-h output of the reforecast only and not during the subsequent outputs. This suggests that the problem is due to the model spin-up when the higher-resolution reforecast is initialized from the lower-resolution reanalysis. We will clarify this in the manuscript.

P6, bottom line: This is why it is so important to say something about the gust parameterization and why the authors shall be encouraged to compare their result to direct observations also on areas with complex terrain.

As stated above, we will add a paragraph in Section 2.1 to detail the representation of gusts in the model data.

P7,L27-29: If I understand the results correctly you only have two cases so such a strong statement that poor predictability is linked to process of extra tropical transition and convective dynamics cannot be derived, so the authors need to weak this statement and elsewhere in the manuscript.

We will clarify that the case of ex-Lili emphasizes the poor predictability of the position during extratropical transition due to the difficulty at representing convective dynamics

but that it is unique among the selected storms and that other cases that exhibit strong biases formed over very different regions, as e.g. Patrick over the southeastern United States and Jennifer (1996) over the eastern North Atlantic.

P8,L34: It seems to be a bit awkward that the authors argue a high storm to storm dependency as in the rest of the paper they use all the cases to get some robust conclusion about predictability of severe storms which implies averaging over as much cases as possible, also the dependency to the threshold is expected as it is a matter of statistics that there is dependency to thresholds.

We agree that the results depend on the exact thresholds but we believe that a limit of 2–4 days is realistic for the large majority of storms with a reasonable definition of a useful forecast of the actual storm for an operational forecaster. We will omit the storm-to-storm variability here, which is not necessary at this point of the discussion, and further revise and clarify the choice of thresholds.

P10,L32-33: Change to ' further suggested to maximize . . . optimal threshold is used to predict gusts'

We will change to "further suggested to maximize the Heidke Skill Score to define the optimal threshold".

P11,L5: From Figure 9 I think that the hit rate decrease but the false alarm rate increase, correct?

This is indeed confusing. We will clarify that this is due to a different balance between hit rate and false alarm rate.

P11-12, section 4.3: Well single storms are always special so I do not see why there is a need for this section.

The purpose of this section is double. Firstly, it illustrates how the verification of forecasts can be biased by focusing on observed events only. Secondly, it explores possible links between the predictability of the storms and their physical characteristics.

We will clarify the separation between the characteristics of some outliers and the absence of a systematic link dynamics and predictability. We will further extend the discussion of the results and include a comparison with findings of previous studies.

P12,L16-26: Please shorten this part – it is a summary and not a conclusion.

We will shorten this part as suggested.

P13,L8: Please cite the earlier studies and change 'should' to 'shall'.

We will implement the suggested change and cite the earlier studies.

P13,L20: I think the cases to case variability is expected.

Again, we will clarify the separation between the characteristics of some outliers and the absence of a systematic link between dynamics and predictability. We will further discuss the limitation for the verification of extreme events and compare alternative methods.

P13,L21: The conclusion on low predictability for storms of tropical origin only relys on 2 cases so weaken this statement here.

We will clarify that the link with extratropical transition concerns a unique case in the dataset.

References: Please get rid of the numerous errors in the reference list – this is annoying!

There appears to be a problem with the URL of several references. We will therefore omit the URL whenever the DOI is available.

Figs. 5, 6, 7 and 10 needs to have increase axis labels as e.g. Fig. 8 has.

We will increase the axis label in those figures as suggested.

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