

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

Anonymous Referee #1

Received and published: 6 May 2017

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 reliability of the deduced skills and the authors tend to overemphasize the results. Still the manuscript is nicely written and well structured. It certainly contains new findings,

[Printer-friendly version](#)

[Discussion paper](#)



which are fruitful for how to identify predictive skill for extreme events, so I certainly see that the manuscript is suitable for NHESD, if my minor to major comments are treated seriously.

Comments

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

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

P2,L29: Please change 'manuscript' to 'study'. P3,L15-16: As wind gusts are an important metric used in this study, you need to explain how this is derived in the reforecasts and how these gusts compare to observations.

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.).

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.

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

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.

P5,L17-18: This could also be 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.

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.

[Printer-friendly version](#)[Discussion paper](#)

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.

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.

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

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

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

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

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

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

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

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

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

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2017-122,

2017.

NHESSD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)

