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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Statistical characteristics of convective wind gusts in Germany" *by* Susanna Mohr et al.

Anonymous Referee #2

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General comments

The manuscript "Statistical characteristics of convective wind gusts in Germany" written by Susanna Mohr et al. describes a methodology to identify and select convective wind gusts from station measurements at 110 stations within Germany. Characteristics regarding the seasonality as well as spatial variations over Germany are considered and rare convective gusts are characterised by means of extreme value statistics. Additionally, by comparing the convective gust measurements to mean winds, gust factors are quantified.

Generally, the study presents very relevant work and is an important contribution to the understanding of local small scale convective wind gusts. The manuscript is well written and the chosen methods to assess the statistical characteristics of convective gusts in

general seem appropriate and well suited. However, I notice several minor flaws (which I listed below) in the methodological and statistical approach which I would recommend the authors to consider. I thus suggest the paper to be accepted after minor revisions.

Specific comments

P. 3, L. 17: Results show, that no significant differences are found in the intensity of rare convective gusts with respect to orography. Why are stations at higher ground excluded? It might be particularly worthwhile to also consider stations at higher altitudes!

P. 4, L. 13-14: The choice of a 50-km radius does not seem to be justified by the given explanation. Since a gust front can occur several kilometers ahead of a storm center this might suggest a radius of 5, possibly 10 km.

P. 4, L. 20 "proximity to the wind station": Pressure gradients are calculated by means of a small set of 6 climate stations. It should be explained how the pressure gradients "in proximity to the wind station" are determined and in how far it can be expected that small scale depressions can be captured (or why such small scale depressions are disregarded!).

P. 4, L. 20-22: This additional filter criterion seems a bit random/unsystematic. I suspect, that not only in April but also in autumn such weaker pressure gradients do occur. I would thus favor a more systematic treatment of seasonality. Also, this additional criterion might hinder the interpretation of spatial as well as the seasonal variance discussed later in the text.

P. 4, L 23-24: Sensitivity of what? It should be specified in which respect the sensitivity has been considered!

P. 5, L. 2: What is meant by "as the approach reproduced the sample better"?

P. 5, L 3-4: Does "uncertainties of the method itself" refer to confidence intervals on estimated return values? Should be clarified!

P. 6, L. 19-26: In Figure 1, I would propose adding confidence intervals to indicate the uncertainties in the seasonal variations. As noted above, a single event can cause the peak in beginning of April. Without a proper estimation of uncertainties (confidence intervals) I would challenge the statistical robustness of the results presented here!

P. 7, L. 7-9: Has this been tested explicitely or is this just an interpretation of the missing north-to-south gradient? Of course this could be explicitly done correlating orographic height of the station against percentile value?! This is also related to my previous comment on excluding stations at higher locations.

P. 7, L. 27-28: Although I do not want to object to the threshold choice itself, I do want to mention that this is not how the parameter stability criterion should be interpreted! In the GPD framework, it can be inferred that if the distribution of values above a certain threshold (u_0) follows a GPD, then it follows a GPD above all thresholds higher than u_0 with a modified sigma. Shape and modified scale should thus be constant above (not near) the chosen threshold within confidence intervals! For details see Coles: An Introduction to Statistical Modeling of Extremes, 2001 p. 78/79.

P. 8, L. 3-5: A comparison of the empirical estimates (95% percentile) and estimates from extreme value statistics might be interesting here. According to the numbers that are specified in lines 10-11 on page 7 we are then talking about a return period of about 1 year.

P. 8, L. 14-15: It should be clarified how the statistical uncertainty is calculated for a region. In the caption of figure 5 it is mentioned that it corresponds to the mean of 95% confidence levels. I do not see why and how this should compare to the standard deviation for different stations (regional variability)!

P. 10, L. 24: As mentioned in my previous comment, it should be clarified if this has been explicitly tested or weather this is simply the interpretation of Figures 2 and 4 (which do not contain an explicit information on orographic height).

P. 10, L. 26: By definition, an event with a 20-year return period has a fixed occurrence frequency of 20 years! Please rewrite!

Technical corrections

P. 7, L. 7: slight variability instead of slightly variability

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