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Interactive comment

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

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Dear Referees,

thank you very much for your work and the useful and valuable comments how to improve the scientific quality of our manuscript. Please find below our reply to the individual points, marked with an "AC" (author's comment).

Best regards, Susanna Mohr on behalf of all co-authors

Response to the referee comments: Referee #1:

Separating the occurrences and measurements of different Aeolian phenomena such as synoptic cyclones, thunderstorms, tornadoes and so on is a key topic of modern wind engineering in order to perform distinct statistical analysis, to extract the main

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Discussion paper

statistical parameters related to each phenomenon, and to build wind field models suitable to represent the wind loading and response of structures. Merging these separate evaluations in a unitary formulation is a further aim still in the embryonic stage.

This paper provides very interesting and new information on several aspects in the above framework, thus represents a useful and pertinent contribution to the advance of the knowledge in this field. In its whole I appreciate it and support its publication.

This paper contains a broad literature both in the fields of atmospheric sciences and wind engineering, perhaps a little biased towards the first field. Despite this I believe that some relevant contributions to this topic are not considered and some choices inherent the methods herein applied seem to be based on a limited view of some previous contributions. Under this point of view, without changing anything in the substance of this paper, I believe that a wider critical discussion on the advantages and shortcomings related to such choices may improve the quality of this paper and inspire future step forwards.

More in detail, I recommend Authors to take into account the following remarks and observations:

Section 1: At least two additional references should be considered. The first (Gomes and Vickery,1978) is the fundamental paper that in 1977 introduced the concept of mixed wind climate and the idea of processing separately the statistical analysis of different wind phenomena. The second (Kasperski, 2002) published in 2002, deals with the same topic of the present paper just with reference to Germany. A comparison with previous methods and results is recommended.

AC: We will add both literatures in the introductions at the corresponding passages.

Section 2: I am quite doubtful on the decision of restricting analyses to the summer half-year. In my experience thunderstorm events are concentrated in this part of the year but are present also, in minor proportion, all over the year. Restricting analyses to a period is even more dangerous considering the aim of performing a statistical analysis of the extreme wind speed. Unavoidably this produces underestimated results. I suggest to revise this choice in next contributions.

AC: In Germany, thunderstorms do not occur very often during the inter half year and, when they occur, those events are in general embedded in frontal systems, which is not our interest in this work (avoid mixed climate). For example, Wapler (2013) shows that in Germany the number of strokes during the winter half year is a power of 10 to 100 smaller as during the summer time. Further, she demonstrated exemplary for a few weather stations an extremely small number of thunderstorm days during the winter half season (< 1 per month). However, in future work we will investigate the sensitivity on the results considering the winter events more accurately.

Section 2.1: Authors base their analyses on the daily peak and subsequent mean wind speeds on 10-min and 1-h periods. They also use pressure measurements. A very similar approach is used in Uruguay and described in Duranona (2015). I suggest to examine this contribution.

AC: Thanks for the reference of this work. However, the author uses another definition to separate strong convective wind events from a mixed wind climate (sudden increases in wind speed, temperature drops, wind direction shifts) and they did not used pressure measurements/gradients. And the overlap between the studies concerns only the results regarding the (convective) gust factors. Therefore, we will incorporate the proposed literature only in the result section about the gust factor.

Section 2.1: Also in the light of the occurrence of gust factors in the order of 6-10, I suggest Authors to consider the possibility that some peak values in the database may be wrong (Cook, 2014). The potential presence of some mistakes and the difficulty of recognizing them is a major shortcoming of this kind of analyses, where the control is very good in terms on mean values but almost impossible with reference to single peaks.

AC: Again, we explicitly checked each event with high values above 6 and could not identify some mistakes. For example, in those cases the mean hourly wind are < 6 m/s. Choi and Hidayat (2002) have already stated that gust factors obtained close to the storm center may reach values between 7 and 8. Furthermore, those high values can certainly result when the event duration of a gust is very small or the event happens in the end of the measuring time and, thus, the gust affects not so much the hourly mean wind.

Section 2.3: I understand that Authors have probably no other opportunity than this use of lightning data. In my experience the presence of cloud-to-cloud lightning not detected by measurements may provide some relevant drawback. I verified this by comparing similar lightning data with high-sampling velocity records.

AC: That's right and we are also aware of this. But unfortunately cloud-to-cloud lightning (CC) was not recorded by BLIDS entirely due to the lower frequency range. However, studies show that thunderstorms connected with only CC occur predominantly during the winter time or are in general "weaker" (cf., Rakov and Uman, 2003) and, thus, less associated with strong downdrafts or straight-line winds. We will add a comment about this aspect/uncertainty in this section.

Section 2.4: The problem of the separation of different wind events is a key topic because any mistake in this stage may compromise the quality of further evaluations. I suggest Authors to dedicate a few more words to this problem for instance using a citation to Lombardo et al. (2009) (included in references but not cited here) and to De Gaentano et al. (2013).

AC: We will add more comments about this problematic in the section "Definition of convective gusts" considering the mentioned studies.

Section 2.5: Authors speak of GEV and POT/GPD and make the choice of using POT/GPD. This is fine but again, without changing the substance of this paper, this topic is a "world" that may necessitate a some more "delicate" approach. First of all the use of POT/GPD is widely supported by some Authors but drastically opposed by others. The reference (Harris, 2005), for instance, is fully devoted to demonstrate that this method is wrong or at least unreliable. Our research group recently published a series of papers based on long-term Monte Carlo simulations (Torrielli et al., 2013; 2014) that confirms the limited reliability of the POT/GPD technique and arrives to the conclusion that the Process Analysis (Gomez and Vickery, 1977) (probably not easy to apply to thunderstorms) and the Penultimate distribution (Cook and Harris, 2004; 2008) are the best methods.

AC: We will address this issue and the related uncertainties (and literature) more in detail in the conclusion. âĂČ Section 2.5: At the end of this section Authors write "that the differences between the return values estimated by both methods are considerably smaller than the uncertainties of the method itself". This is absolutely correct with reference to return periods in the order of the number of years of available data, for instance 20-50 years. Structural safety, however, needs evaluations extrapolated to return periods in the order of 500-1000 years. Here, different methods lead to divergent results (Cook and Harris, 2004; 2008; Lagomarsino et al., 1992).

AC: Regarding the first comment, that's correct and we will specify that. Regarding the structural safety: In general, the reference velocity in national standards like DIN or EUROCODE is based on average on a return value of 50-year.

Section 3.1: Authors write: "we considered every single measurement at each station, which means that one event can be recorded on two or more stations". I think that this sentence may result misleading. Downbursts are phenomenon with a radius of a few km. It is almost impossible that the same downburst may be detected by two stations of this network. The situation is different if Authors refer to the large scale wind event that generates downbursts. This point should be clarified.

AC: We will specify that in the corresponding passage.

Section 3.3: The last sentence deserves a citation to Authors that first expressed this concept (Gomes and Vickery,1978).

AC: We will include the literature at this point.

Section 3.4: Line 11. The dependence of the gust factor on the averaging period is discussed also by Solari et al. (2015).

AC: We will also refer the literature at this passage.

Section 3.4: Line 20. I do not agree on the sentence according to which “turbulent factors (to replace with gust factors ?) fluctuate usually between 1.2 and 2.3”. Such a large variability necessarily depends not only on the roughness length but even more on the stability conditions. If wind is intense and of synoptic type, then atmosphere is neutrally stratified and the gust factor may vary between 1.25 and 1.75 with an average value around 1.5. In my opinion gust factors in the order of 1.75-2.3 may be ascribed to unstable conditions and intermediate events between large scale depressions and mesoscale downbursts (De Gaetano et al., 2013).

AC: We will correct that and will integrate the dependency of the gust factor values regarding to the atmospheric stability conditions.

Independently of the above remarks, that I wrote in a fully constructive spirit, I confirm my appreciation towards this contribution and that I consider it appropriate for publication. I hope that Authors may consider or discuss my remarks. I suggest that this paper is accepted subject to minor revisions but I believe that a quick re-review may be useful.

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âĂČ References: 1. Gomes L, Vickery BJ (1978). Extreme wind speeds in mixed climates. J Ind Aerod, 2, 331-344. 2. Kasperski M (2002). A new wind zone map of Germany. J Wind Eng Ind Aerod, 90, 1271-1287. 3. Duranona V (2015). The significance of non-synoptic winds in the extreme wind climate of Uruguay. Proc 14th Int

Conf on Wind Engineering, Porto Alegre, Brasil. 4. Cook NJ (2014) Review of errors in archived wind data. Weather 69: 72-78. 5. De Gaetano P, Repetto MP, Repetto T, Solari G (2013). Separation and classification of extreme wind events from anemometric data. J Wind Eng Ind Aerod, 126, 132-143. 6. Harris, R.I., 2005. Generalized Pareto methods for wind extremes. Useful tool or mathematical mirage? JWEIA 93, 897-918. 7. Torrielli A, Repetto MP, Solari G (2013). Extreme wind speeds from long-term synthetic records, J Wind Eng Ind Aerod, 115, 22-38. 8. Torrielli A, Repetto MP, Solari G (2014). A refined analysis and simulation of the wind speed macro-meteorological components. J Wind Eng Ind Aerod, 132, 54-65. 9. Gomes, L., Vickery, B.J., 1977. On the prediction of extreme wind speeds from the parent distribution. J. Ind. Aerodyn. 2, 21-36. 10. Cook, J., Harris, I., 2004. Exact and general FT1 penultimate distributions of extreme wind speeds drawn from tail-equivalent Weibull parents. Struct. Saf. 26, 391-420. 11. Cook, J., Harris, I., 2008. Postscript to “Exact and general FT1 penultimate distributions of extreme wind speeds drawn from tail-equivalent Weibull parents”. Struct. Saf 30, 1-10. 12. Lagomarsino, S., Piccardo, G., Solari G., 1992. Statistical analysis of high return period wind speeds, JWEIA 41, 485-496.

AC: Thanks for the many literature suggestions. In the meantime, we have a rather comprehensive literature database, however, with a focus more on convective wind gusts and not so detailed on the general perspective of strong wind events. âĂČ

Response to the referee comments: Referee #2 (Anonymous):

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!

AC: At higher stations the separation and classification of strong wind events into homogeneous families (convectively driven) is very difficult and we explicitly want to exclude events caused by a mixed climate (large-scale / convective conditions). We will add a comment about this.

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.

AC: A gust front can occur several tens of kilometers ahead of a storm center with lightning activity, as already shown by Klingle et al. (1987) and Pantillion et al. (2015). Note that we consider only clout-to-ground lightning. We also refer to the concept of a “trailing gust front” (typically south or southwest of the flank of a storm; see also Houze, 2014; chapter 9), where higher distance to the storm center are possible. By the way, we tested a varied radius and a distance reducing does not modify the overall results. We will add comments about that in the revised version.

Klingle D. L.; Smith D. R. & Wolfson M. M. Gust front characteristics as detected by

Doppler radar Mon. Weather Rev., 1987, 115, 905-918.

Pantillon, F.; Knippertz, P.; Marsham, J. H. & Birch, C. E. A parameterization of convective dust storms for models with mass-flux convection schemes J. Atmos. Sci., 2015, 72, 2545-2561.

Houze R. A. Cloud Dynamics (2nd Ed.) Elsevier Inc., Oxford, UK, 2014.

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!).

AC: The six climate stations are located over Germany and the distance between the stations is always smaller than 250 km (mean 210 km). Means that greatest pressure gradient of the nearest station in dependency to the others five stations is investigated. Therefore, we should capture with the filter approach also small scale depressions. We will discuss that more in detail in the revised version.

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.

AC: You are right. This additional criterion seems to be a bit random. However, we performed several comparisons using synoptic weather charts and found an additional criterion is necessary, but only in April, where large-scale storm over the North Sea (not so common in September) may affect the gust statistics in the North German Plain (> 52°N). We will specify this.

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

AC: Both the distance to a lightning recording and the thresholds of pressure gradients were identified by sensitivity and individual case studies by investigation of the impact of varying thresholds on the sample size and the following results. We will add a comment.

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

AC: We mean “as that approach reproduced the observed gusts better”. We will change the formulation.

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

AC: Yes. We will clarify that.

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!

AC: We will include the confidence intervals in the figure. Probably, only in Figure 1a to show the uncertainties and not in Figure 1b, since it could be confusing with the North/South information. We have to test this.

P. 7, L. 7-9: Has this been tested explicitly 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.

AC: Yes. We have not recognized any correlation between the percentile values and station heights. The correlation is in general < - 0.1. In addition, it is striking that stations above 500 m (16 station means 15%) have smaller values than lower located stations. We will add a new Figure and more comments about that in the revised version.

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.

AC: That's correct. We will modify our unhappily chosen formulation. However, this point has already taken into account in the investigations. âĂČ 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.

AC: A comparison between percentiles and RV1a show that values with a return period of 1 year corresponding to a 95 % to 98 % percentile–depending on the stations. We will add a sentence.

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)!

AC: We will modify the figure caption to clarify this.

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).

AC: Yes. See comments above. Depending on RV the correlations (to station height) are between 0.2 – 0.3.

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

AC: Yes, that is correct. We will rewrite this in “A comparison of the 20-year return values of convective gusts with those of turbulent gusts demonstrates that the latter have higher return values.”

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

AC: Thank you for close reading. We will correct that.

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