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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.

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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 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: âĂć Section 1: at least two additional references should be considered. The first ïĄŻ1ïĄİ 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 ïĄŻ2ïĄİ, 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. âĂć 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. âĂć 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 ïĄŻ3ïĄİ. I suggest to examine this contribution. âĂć 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 ïĄŻ4ïĄİ. 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. âĂć 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. âĂć 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 ïĄŻ5ïĄİ. âĂć 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. Ref. ïĄŻ5ïĄİ, 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 ïĄŻ6,7ïĄİ that confirms the limited reliability of the POT/GPD technique and arrives to the conclusion that the Process Analysis ïĄŻ8ïĄİ (probably not easy to apply to thunderstorms) and the Penultimate distribution ïĄŻ9,10ïĄİ are the best methods. âĂć 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 ïĄŻ9,10,11ïĄİ. âĂć 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. Downburst 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. âĂć Section 3.3: The last sentence deserves a citation to Authors that first expressed this concept ïĄŻ1ïĄİ. âĂć Section 3.4: Line 11. The dependence

of the gust factor on the averaging period is discussed also by Solari et al. (2015). âĂć 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 ïĄŻ5ïĄİ.

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 subjevt to minor revisions but I believe that a quick re-review may be useful.

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