

*Dear reviewer, thank you for your work. We regret that you do not consider our article to be of sufficient quality. In our italicized answers, we try to convince you and the editor otherwise.*

The manuscript by Müller et al., aims to analyze Central European wind and precipitation “compound” events. Despite the topic is of interest, the manuscript fails in several key points:

- The authors are analyzing concurrent events and not real compound events. Please use the correct wording;

*In our opinion, the term “wind and precipitation compound event” is correctly used because strong winds and heavy precipitation not only co-occur but the damage produced by such an event can be larger than the sum of damage due to both phenomena independently. We have mentioned this fact in the second paragraph of the Introduction (there are also other examples of the possible compounding effect, e.g. rainfall damage in a house with its roof damaged by wind). Moreover, the term has already been used in many existing papers (e.g., Zscheischler et al., 2021: Evaluating the dependence structure of compound precipitation and wind speed extremes. Earth Syst. Dynam., 12, 1–16).*

- More information regarding the weather data is needed (weather stations with full data, station % in altitude, missing value, etc). Data homogeneity is still a key issue when analyzing station data. Despite the authors discussed homogenization in section 4.1, there is not enough information and the methodology points for a paper from 2017 without any further information.

*With respect to your comment, we intend to add an electronic supplement with details on all station data series, including the station altitude and time period covered. Thank you for your comment on the homogeneity of data: we make an additional homogeneity test of the data series and exclude those which may not meet its criterion.*

- I am puzzle with the use of the outdated NCEP/NCAR reanalysis. I don't agree with the authors that the coarse resolution of 2.5°, represents well the large-scale processes. The authors should be aware that both in winter and summer, some local features (sting jets, dry intrusions, convective storms) will not be well represented in a 2.5 course resolution. The use of the state-of-the-art ERA-5 reanalysis in its native grid it's imperative.

*Though ERA5 contains only preliminary results for first 18 years of our study period (1961–1978), we agree that it could probably be used in our study. However, we are still convinced that our decision for the coarse NCEP/NCAR reanalysis makes sense. We want to stress what we use the reanalysis data for: it serves for expressing and classifying the atmospheric circulation patterns at the synoptic (=large) scale in Central Europe, not for detailed analysis of the wind field. Each event is represented by two numbers only (zonal and meridional wind components) which need to be as general as possible to avoid effects of local factors. It is similar to the question of the appropriate horizontal resolution of a GEM: to express the position of a certain location in terms of the entire mountain range, it is not appropriate to use a detailed model, where the orientation of the slopes is influenced by local relief shapes.*

- Processes (drivers of the concurrent events) based analysis is missing from the entire manuscript. What summer and winter concurrent events differ? Which different drives take a role on different seasons?

*Due to the length of the paper, our intention was to leave the detailed analysis of the meteorological causes of the events for another, separate paper, because adding such an analysis to the existing text would approximately double its length. However, we can include several brief case studies to illustrate our findings if necessary.*

The novelty of the results is also an issue. In addition, no clear objective is clearly stated in the manuscript. I don't believe the papers as enough novelty to be included in just a journal like the Natural Hazards and Earth System Sciences.

*We regret that obviously, we have not managed to emphasize more the goals and novelty of our research. Our objective was to determine the regularities of the spatial distribution of the frequency and the seasonality of W-P events within Central Europe. We consider the following aspects of our work and findings to be new:*

*1/ we process data for the whole year, not only the cold half-year, so unlike previous studies, we document W-P events also in areas where strong winds typically occur in the warm part of a year;*

*2/ we process station data, so we also capture local strong winds apparently related to severe convection, not captured by gridded values of maximum wind speed in reanalyses; in areas where W-P events of this kind are typical, we therefore document a much higher frequency of W-P events than previous studies;*

*3/ we consider the altitude of the stations and demonstrate that the frequency of W-P events significantly depends on the altitude but this dependence is not uniform: in the west, the frequency increases with increasing altitude, while it decreases with increasing altitude in the east of Central Europe; we also explain how this fact is related to circulation patterns;*

*4/ we demonstrate the fact that the higher the threshold of strong wind and heavy rain, the higher the relative frequency of W-P events at weather stations where W-P events are rather frequent.*

The authors fail to put the manuscript into a larger context and discuss with the main drivers. In the present form it's just a description of the statistical results.

*We can't quite agree. If there is a high value of wind speed at 850 hPa level (yes, in the coarse reanalysis!), it proves that the event was due to a big pressure gradient within a cyclone – its position can be estimated from the 850 hPa level wind direction. On the contrary, if 850 hPa level wind was slow, the wind gust at the surface had to be due to another factor, very probably due to severe convection. Of course, this assumption could – and should – be further elaborated in our next paper.*

Therefore, I strongly suggest the rejection of the manuscript.

*We respect your opinion, even if we don't agree with it.*

*Miloslav Müller*