

# ***Interactive comment on* “Characterizing Wind Gusts in Complex Terrain” by Frederick Letson et al.**

## **Anonymous Referee #2**

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### General Comments

The manuscript entitled “Characterizing Wind Gusts in Complex Terrain” by Letson et al. addresses various aspects of wind gusts based on nine meteorological masts and two Doppler lidars within a region of about 3 km x 3 km in size, that is characterized by two topographic ridges separated by a valley, which are oriented in the northwest to southeast direction. Although it is not explicitly expressed in the manuscript, the study seems to be a follow-up of the work by the same authors (Hu et al., 2018), but here the attempt is to generalize the findings of Hu et al. (2018) by analyzing several mast measurements instead of one only. The present work focuses in understanding the statistical behavior of wind gusts as a function of height, in the horizontal plane, and as a function of atmospheric conditions (stability, wind direction, turbulence inten-

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sity). The paper presents in-depth analysis of various gust parameters using different types of statistical methods. However, due to this, in many parts the paper is laborious to read and to understand. Therefore, it is strongly recommended to restructure the manuscript by focusing more on the presentation of the key objectives of the work and addressing the results and conclusions accordingly. In other words, substantial revision is recommended.

### Specific Comments

Introduction (page 1, lines 24-37): It is not clear what you mean by a wind gust. You refer to gusts generated by mesoscale convective systems and downdrafts and by mountain waves, but is the topographic channeling also causing wind gusts? Recommendation: provide a clear definition for a wind gust already in the very beginning of the manuscript.

The use of terminology is confusing in some parts of the work:

- Gust definitions are provided only on page 6, and before that you use terminology “descriptors of wind gusts” (p. 3, line 1), “wind gust characteristics” (p. 3, line 33) and “gust parameters” (p. 4, lines 19-20), without any explanations. Recommendation: define the gust already in the beginning (with a reference to Section 3.1) and use the terminology consistent within the whole manuscript.

- Doppler lidar scanning technique: In the abstract and in different parts of the manuscript you write that the Doppler lidars are “vertically pointing”, “vertically-pointing conically scanning” and “vertically-scanning”, which is confusing. Moreover, in Section 2 (p. 5, lines 12-30) you indicate that the lidar is a scanning lidar with cone angle of  $\pm 15^\circ$ , but you don’t provide any information on, e.g., how many lines-of-sight the Doppler lidar has, etc. Recommendation: provide detailed technical information of the Doppler lidars and the scanning techniques in Section 2. If the Doppler lidar was not applied in “stare mode” (i.e. measuring only in vertical direction) you can write “vertical profiles derived from Doppler lidar measurements” instead of “vertically pointing”.

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Objectives are expressed quite broadly in Section 1 (pages 3-5) and the conclusions in Section 5 do not include answers to all objectives. Recommendation: Provide a more concise list of objectives and check that you answer them in Section 5.

Section 2: Reorganize the Section: start with the most important data set (met masts), then Doppler lidars, and in the end other supporting data (terrain elevation + canopy height). Adding subtitles would make the Section more organized.

Figure 2c: percentages within each sector are difficult to read. Please, consider using bars instead of a line to indicate the histogram of wind directions. Section 3.1: Gust parameters were easier to understand after seeing the Figure 3a of Hu et al. (2018). Please, consider showing a similar picture also here. Furthermore, consider providing an equation for the gust length scale.

Section 3.1: Move the probability distributions (p. 6, line 34 – p. 8, line 5) into a separate subsection. Add the “gust period” (p. 8, lines 15-25) to the list of gust definitions on page 6. Consider also a new subsection for the gust parameterizations (p. 8, lines 6-14), or combine them to the list of gust parameters on page 6 (or consider leaving them out from the whole study).

Page 12, lines 10-11: comparison of 3-5 s averaging window and the gust rise time is unfair, as the gust rise time is defined based on 3 s moving averaged wind speed time series. Moreover, in Figure 4c, the 1%  $t_{\text{rise}}$  data are scattered around 4 s. Is this related to the presence of the lower limit for  $t_{\text{rise}}$  arising from its definition? Please, comment.

Page 12, line 18: At this point it was necessary to read the paper by Hu et al. (2018) in order to understand the work in this manuscript. Recommendation: mention already in the introduction, that this present paper is strongly based on the work by Hu et al. (2018). You may even list their main findings in the introduction of this paper and then build the objectives of the present work on top of these findings, by addressing what else will be done here. This way, it will be probably easier to improve the structure of

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the present paper when everything is not presented as “new”.

Figure 5 and Figures S1-S5: Could these results be shown as a Table? The images are not very informative. Without seeing aspect ratios, it is impossible to visually see the differences between the cases where the aspect ratio changes e.g. from 6.97 to 7.30 or from 1.18 to 1.32 or even to 1.60. Recommendation: show a couple of examples of these joint distributions in Section 3, where the methodology is described, to illustrate visually, how the aspect ratio is calculated. Then, in the results section, you could summarize all the results in a Table providing only the aspect ratios, maybe by grouping the results into high, medium and low values. This way, the main results from the Supplementary material could also be shown in the main manuscript, supporting the nice results on page 13 lines 20-25, which are relevant and a key part of this study.

Figure 7 on page 16 is very difficult to interpret, because all the lines are close together and panels are very small. What is actually the added value of the upper panels (a1-f1) compared to the lower ones (a2-f2)? Moreover, the main results of Figure 7c2 are shown also in Figures 8b-c. Recommendation: remove all other panels except Figures 7e2 and 7f2, and modify the text accordingly. Only these panels provide something new compared to the other Figures.

Page 16, lines 17-25, and p. 17, lines 1-4: you discuss the Doppler lidar volume averaging, but in Section 2 you don't describe in detail what is meant by the “volume” in case of the continuous wave ZephIR lidar (and how it differs from pulsed Doppler lidars). Please, provide explanations.

Figure 9: Already Hu et al. (2018) showed that the peak factor is not a function of mean wind speed. Why do you show this picture? Please, consider removing it. Comparison to other parameterizations is not very relevant for this work, maybe you could remove the parameterization aspect from the whole paper, and leave it open for future studies. There is now enough material already for one paper, even without the parameterization.

Section 4.3. and Figure 10:

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- In the text, you introduce a new concept “mean marginal probability” without defining it. Please, explain.

- Figure 10 is extremely difficult to interpret: color scale is continuous, but the results are presented as boxes. It is very difficult to interpret the color of each box; please, use categories in the color scale, for example at 0.1 intervals of probability.

- Concerning panel (a), you conclude that the mean co-occurrence probability is 0.27 for 10 min data and 0.43 for 30 min data. This means, that in 73% and 57% cases the gusts do not occur simultaneously. Moreover, the Figure 10a illustrates the small differences in co-occurrence probabilities especially in the low range (blue colors). Is that really necessary? Recommendation: provide results only for probabilities > 0.5 (and with 0.1 intervals in color scale as suggested in the previous comment).

- The color scale in panel (a) for 10 min data is different from the scale of 30 min data, because it is possible to distinguish the circles in the diagonal. Please, comment this.

- What is causing the asymmetry across the diagonal in panels b-d? It is especially pronounced in panels (b) and (d).

- Overall, Figure 10 is extremely difficult to interpret. What are the parameters on each side of the Figure? Why do you give percentages only on the vertical axes? Why the results are not symmetric with respect to the diagonal? Have you calculated the average probabilities using data from the diagonals too? It is possible to find the answers to these questions based on the provided information but it takes a lot of time. Due to this complexity, it is foreseen that many readers will probably skip the Figure while reading the paper. Therefore, the value of the illustration is questionable. Consider removing the Figure, or simplify it substantially.

Figure 11: Why do you show separately gusty conditions and all cases? Why not to show separately gusty and non-gusty cases (i.e. no overlap of underlying data)? The contribution of these spectra to the understanding of the characteristics of wind gusts

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is questionable, please, consider removing this Figure.

Figure 13 and text on p. 21, lines 17-29, and p. 22, lines 1-7: you have recognized a coherence floor, but still you perform exponential fits with respect to zero – why? What is the parameter “d” on the horizontal axes?

Section 5:

- Conclusions do not answer directly to the objectives of this study. Please, check and modify.

- The third paragraph (p.23, lines 8-15) is too complicated. It is impossible to identify to which parts of the results section these conclusions refer to.

Technical Comments

p. 3, lines 1-3: long and complicated sentence, please simplify.

p. 4, line 1: Repetition: “characteristics”, “characterized”

p. 4, lines 3-5: Complicated sentence: “Horizontal coherence. . . . across the study domain.” Please, clarify.

p. 5, lines 1-2: information in parentheses not necessary, please remove.

p. 5 line 12: “Some analyses reported herein employ. . .” Quite arbitrary approach, please, be more specific.

p. 10, line 19: Please provide the ZephIR lidar sampling rate here.

p. 12, line 17: Add: “Figure 3f-g”.

p. 12, line 18: Remove parentheses: “with (Hu et al., 2018); Table 2”, i.e., change to “with Hu et al. (2018); their Table 2”.

p. 12, lines 18-20: Does this sentence summarize the results of the present study or those from the literature? Please, clarify.

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p. 13, lines 3-7: Too complicated and long sentence. Please, simplify. Explain also what is meant by “all three time parameters”.

p. 13, line 11: Please, define “gust intensity”.

p. 13, line 14: Consider splitting the paragraph before “Joint distributions of gust...”

p. 15, line 18: Consider starting the sentence with “In Figure 7, data from all 10 minute periods within 64 days. . .”

p. 16, line 25: Add to the end of the sentence: “. . .the fit vary between sampling location and instrument (Figure 8c).”

p. 18, line 9: Is the referred Figure 3g correct here? Figure 3g shows the distribution of lapse time, not gust length scale.

p. 21, line 6: Should it read “Reversal height estimates”?

p. 21, line 25: “Tower 29, Figure 13b) have C values between 8.4 and 10.6” - is this wrong? According to caption, Figure 13b shows the results for Tower 20, and in panel (b) the C values are smaller.

p. 21, lines 25-26: “sensors with vertical separation of 6 to 16.9 (Solari, 1987)” - this is not understood: what is the “vertical separation” here? Does it refer to the instruments here or in the study by Solari (1987)?

p. 21, line 29: “Tower 25 exhibit C values of 4.6 to 6.7 (Figure 13c)” - these are probably also wrong. Please, check to be consistent with Figure 13.

p.23, lines 9-10: “Joint probability distributions of the gust properties indicate high aspect ratios for gust intensity metrics. . .” - What is the difference between the “gust properties” and “gust intensity metrics”? Please, explain.

p. 23, lines 10-14: the sentence starting from “However, low aspect ratios. . .” and ending to “...by two parallel ridges (Figure 1).” is very difficult to understand. Please,

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simplify.

p. 23, line 19: Should it read “change in C”?

p. 23, line 20: Should it be “Figure 13d”?

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-906>, 2018.

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