

## ***Interactive comment on “Empirical high-resolution wind field and gust model in mountainous and hilly terrain based on the dense WegenerNet station networks” by C. Schlager et al.***

**C. Schlager et al.**

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We thank the Reviewer very much for the valuable and quite detailed feedback to our manuscript. We carefully considered all comments and made due effort to account for the concerns expressed; and we think it really helped improving the comprehensibility and quality of the text and how we convey the findings.

Suggestions for technical corrections: 1) In the statistical analysis of the paper, I suggest changing the correlation coefficient R by the coefficient of determination R<sup>2</sup>.

Answer: Thank you for this hint; we understand that the R-squared is sometimes pre-

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ferred in these evaluation methods and carefully considered to change this. To avoid inconsistency with our Schlager et al. 2017 paper published to WAF, however, we preferred to keep it in the case of this paper as the correlation coefficient R.

2) On page 10, between lines 20-25, which means the concept of "good accordance", how this concept evaluates from the statistical point of view. I suggest calculating, because it is very simple to do so, to use some "agreement index", such as Index of Agreement (d) developed by Willmott (1981). Willmott, C. J. 1981. On the validation of models. *Physical Geography*, 2, 184-194 With this index I believe that the concept of "good accordance" can be applied and discussed in the article.

Answer: In this context we used the term "good accordance" just for the visual interpretation and explanation of Figure 8. To clarify the statements, we improved the wording of the sentences for the description of this Figure (now p11 L5-6 and p11 L12-13).

Thank you for your suggestion regarding the Index of Agreement (IOA). We agree, and in fact we calculated it in our Schlager et al. 2017 paper as well. To be consistent with this paper, we use the redefined IOA of Wilmott (2002) and added a description about this parameter to the manuscript (now p9 L13-18). We now also discuss the calculated IOA values in the results section (now p11 L24, p13 L15-16, p13 L23).

3) When discussing the results in terms of the RMSE, please indicate some kind of qualitative qualification, for example: high, medium or low.

Answer: Thank you for this proposal; we implemented some text changes to give some qualitative qualification (now p11 L19-21, p13 L8-12, p13 L21).

4) Please, in the article, mention how the effect of the pixel size of the DEM can affect the results of the modeling. In addition, the modification made by the authors to the CALMET model to improve the estimation of solar radiation, carried out previously in another article, also affects the data of the modeling, however it is not well developed in the article. I would have expected a comparison between the results of the unmodified

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CALMET model and the modified CALMET model, using some agreement index like the Akaike information criterion (AIC).

Answer: OK, we agree the description related to the DEM was a bit crude. Therefore we added a paragraph to the manuscript, which explains the performed sensitivity tests regarding different spatial resolutions (now p5 L28-33).

We emphasize that the main motivation why we modified the original CALMET was the generation of overly simplified temperature fields by this original model. The original CALMET produces these fields by a simple (horizontal) interpolation of point-specific temperature observations. Especially in the JBT, with its large differences in altitude, large temperature gradients can occur, however, which may affect the wind field and should hence be allowed for. Because of this and since it creates no other disadvantages, we used the modified CALMET, since it produces a more realistic temperature field accounting for vertical gradients, which are estimated from the range of meteorological stations located at different altitudes. The algorithms further empirically take into account the shading through the relief based on the DEM and the leaf area index. An example of such a generated temperature field is illustrated in Figure 2.

We reconsidered also our description related to this; we think that the description of our motivation for using the modified version is already detailed enough (see p5 L11-L21).

5 Between lines 11-12, this paragraph should be rewritten. Answer: OK, could you please indicate the page number. We checked through the pages but were not sure which page and hence which text-piece was perhaps meant. We will of course rewrite/improve this paragraph in our final version of the manuscript as needed.

6 The sources of error are not adequately evaluated in the conclusions. Again, "good agreement" is mentioned, without having calculated any index of the literature that accounts for this concept. Answer: Thank you for this hint; with this statement we refer to all statistical performance measures applied to wind speeds (B, SDo, RMSE, R, and now also IOA). To avoid ambiguities we replaced "statistical agreement" with

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“overall statistical agreement” in the manuscript, in order to better express it is a type of summarizing statement (now p1 L11-12 and p14 L16). We similarly did that in Schlager et al. 2017.

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Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2018-31, 2018.

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