Answers to Reviewer 1 comments

Maksims Pogumirskis and co-authors

April 2026

We would like to thank the reviewers for their useful comments and suggestions that have helped to improve the manuscript. Our detailed answers follow.

The reviewers' comments are in **bold** while our answers are not. Line numbers refer to the revised version of the manuscript.

1 Major points

The authors find compelling evidence correlating the bias in wind speed and direction with the discrepancy in the terrain elevation between numerical models and reality. Then, the authors argue that larger elevation discrepancies are present in complex terrain scenarios, which, in the authors' view, explains why synoptic and mesoscale models under perform in complex terrain. This last passage is not necessarily true and should be discussed on a case-by-case basis, although it may broaden the discussion too much. As an alternative, the authors could limit themselves by saying that elevation discrepancies are a major cause of model biases without relating it to local terrain complexity.

In complex terrain it is statistically more likely that the observation campaign is located further away from the mean grid-cell elevation. In case with our dataset, observation campaigns are more likely to be located above the model grid-cell elevation. We added plots and text to demonstrate these more clearly. Our results show that the bias is caused by the elevation difference and that its magnitude is correlated with the magnitude of the elevation difference. We agree that this does not necessarily mean that elevation discrepancies are the cause of models underperforming in complex terrain.

We have adjusted the wording throughout the manuscript to be more precise.

A major result arising from the current analysis is the dependence of the wind speed (and direction) bias on the hour of the day and month of the year. This emerges from several figures, both in the model vs. observation and WRF environments. In the Discussion section, the authors link the diurnal bias to the cycle of thermal stability, yet they do not provide a comprehensive discussion of the seasonal trend of bias.

Added a paragraph about a potential explanation (lines 771-775).

Minor grammar errors are present throughout the manuscript. Please revise the text and address them.

Grammar errors were fixed.

2 Specific comments

Line 35: I would not refer to the year-long timescale as "climate" since climate typically spans over 10 years or more. Please consider replacing this word with "meteorology" or something similar.

Replaced “climate” with “conditions”.

Line 71: Please add a short paragraph to summarize the content of the remaining Sections.

A paragraph was added at the end of the introduction section (lines 123-128).

Line 144: The authors claim to focus on the physical mechanisms causing wind speed biases on complex terrain specifically. By looking at the data availability at country level (Figure 1), it is unclear how many of the 580 observational locations are indeed on complex terrain. I recommend that the authors add a metric to quantify the local terrain complexity.

Added a map and a plot showing terrain complexity metric (figures 3 (b) and 4).

Lines 159 - 160: I wonder whether the authors discarded temporal periods with too large variability of 10-minutes averaged wind speed values. Any intra-hour variability may cause uncertainties comparable to the biases quantified between observations and models.

While these periods can cause some uncertainty, their expected effect on the average bias is zero. Excluding these periods would introduce a sampling bias. Added a few sentences to the paragraph motivating the choice of the averaging method (lines 257-259).

Lines 179-180: Although the authors discuss potential limitations of this interpolation technique, I believe that more details are needed about the vertical interpolation of wind speed. To my understanding, you utilize the power law to determine wind speed at the observed height; do you calibrate reference wind speed and shear exponent on numerical predictions? If so, how reliable are they considering that some models provide only 2 levels?

Interpolation procedure was described more explicitly (lines 283-294). Added a paragraph about the reliability of results derived using only 2 height levels and 10m winds to the discussion section (lines 709-715).

Line 185: The roughness length may vary throughout the year depending on the site and vegetation type. Are you modelling this effect as well?

The paragraph illustrates why the use of 10 m should be generally avoided when interpolating winds to higher levels. No, the effect is not modelled and is outside the scope of this study.

Lines 240-246: This paragraph is a bit hard to read. Please consider adding further equations to clarify the normalization process you adopted.

Normalisation process was explained more clearly and equations were added (lines 360-381).

Lines 258 - 259: This point deserves more in-depth discussion. The authors claim that, within a certain area, the terrain variability is not well captured by the model resolution and, thus, the modeled average elevation is erroneous because it results from a smoother terrain – which is true. However, considering that models operate over 1 km-by-1 km grids, the local wind speed is not only determined by the average elevation but also by flow distortions induced by sub-grid terrain variability. In other words, the authors should not only consider the bias in average elevation, but also the standard deviation of the terrain elevation within the averaging area.

WRF model generally does not use subgrid terrain properties, unless gravity wave drag or topo_wind option is enabled, which wasn't the case for our runs. Even if we used these options, the subgrid terrain properties in WRF are not derived dynamically from terrain input datasets. They are pre-calculated as static input data to the model, and they do not change if the terrain elevation dataset is changed. Therefore, our WRF experiments didn't consider any subgrid terrain properties. It was clarified in the text (lines 465-468).

As for comparing models and observations, we considered only the elevation difference. We acknowledge that spatial terrain aggregation on a grid can affect not only the mean elevation, but also how other terrain sub-grid properties are calculated. It is relevant for model datasets that use sub-grid orographic drag parametrisations. However, it is outside the scope of our paper. For clarity we replaced ”terrain representation” with more appropriate wording throughout the text.

Lines 267 - 268: I am not sure about this statement. Local topographic features can induce channeling and other speed-up effects downstream a certain location. This can occur for spatial scales smaller than the model grid but relevant for wind energy. In other words, although fixing local biases is definitely valuable, its footprint on nonlocal model improvement still has to be determined.

The idea behind this statement was the advection of the biases, which follows directly from the Navier-Stokes equation. We agree that this might not be true for the complex terrain. The statement was removed.

Section 3.1: I found this Section really hard to read, in particular the interpretation of temporal distribution of PCs. Why is there a distinction between different geographical areas if the PCA is not carried on separately for different areas? What is the reason of calculating the spatial average of PC1? My suggestion is to thoroughly review this Section at least up to line 395 and provide more clarity to the scope of this analysis.

The section was improved. Clarification was also added to the methods section.

PCA is applied to the entire region and it allows to identify main modes of differences in the bias tables between different parts of the Europe.

Spatial average of PC1 is never calculated. We calculate the average bias table for the region. And PCA allows to identify how bias at a particular location differs from the average model bias table.

Lines 328 - 329: The entire section is based on distinct biases found for different regions - Central European Plain, Southern Europe and Scandinavian Mountains. However, the precise boundaries of these regions should be directly reported in Fig. 4 for a meaningful correlation with the spatial bias distribution.

Added a figure defining the boundaries of regions as referred to in this work (figure 1).

Lines 367 - 369: I am a bit confused by these lines. The caption of Fig. 4 (as well as line 330) describes the spatial distribution of PC1 of wind speed bias, whereas at lines 367 - 369 the authors state that this figure reports the values of PC2 of wind direction bias. Please clarify.

A figure showing the spatial patterns of PC2 of the wind direction bias was added (figure 11).

Line 401: In Figure 9, PC1 of wind speed shows a high correlation coefficient with elevation errors (0.71), whereas this relationship is less obvious for wind direction PC1 and PC2 (correlation coefficients of 0.5 and -0.41). Please make this distinction clearer.

It was already stated in the paper:

While correlations between elevation difference and bias variables are statistically significant, in some cases they are weak and cannot be used to provide a hypothesis about a potential physical cause. However, it has to be kept in mind that the atmosphere is chaotic and model biases are caused by a lot of different physical phenomena. Therefore, if the fact of causation can be established, low correlation implies presence of noise in the data. In that case, correlation is used to measure the strength of the effect rather than to indicate the presence of the effect. For example, a correlation (R) of 0.3 between bias and elevation difference indicates that elevation difference explains 9 % of the variance (R^2) in model biases.

The paragraph was moved before the correlation table. We also included the explicit statement that in observations wind direction biases induced by the elevation difference are described by PC1 and PC2.

Line 403: What is P?

That was meant to be the p-value, but for clarity and consistency with the rest of the manuscript it was replaced with "at the significance level of 0.05".

Line 425-432: The authors claim to interpret the observed correlations between PCs and elevation differences between models and reality, but the following lines provide no interpretation, rather they are a mere description of what emerges from the figures. Inter-changing result's

description and results' interpretation is a recurring theme of this manuscript which I suggest to avoid in the future.

We fixed the wording throughout the manuscript.

Line 504: The authors argue that observational campaigns are more often located on top of hills, which would explain the negative PC1 values. This statement is not generally true as it depends both on the observation site and the scope of the field campaign. If this is true for your set of observations, please state it.

Added a paragraph and figures quantifying differences between model and the real-world elevation in our dataset (figure 5).

Lines 525-526: This result is shown for synoptic spatial scales in the model vs. observation case, but it is unclear how you can infer it for mesoscales since there is no analysis for individual observational sites.

We added information about the local terrain complexity and elevation differences between models and the real world to the paper (lines 221-250). Based on these we provided a more detailed explanation of the results.

Lines 570-572: Again, this is true for flat terrains, while on complex terrain this is not necessarily true and depends on the local topography as well as on the distance between probed locations.

The paragraph was adjusted (lines 784-786).

Line 584: Please provide a brief explanation of the Laplace of the elevation.

Explanation of the Laplace of the elevation has been added (lines 789-793).