

Wind Energ. Sci. Discuss., author comment AC1
<https://doi.org/10.5194/wes-2021-28-AC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Reply on RC1

Rogier Floors et al.

Author comment on "Satellite-based estimation of roughness lengths and displacement heights for wind resource modelling" by Rogier Floors et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-28-AC1>, 2021

Reviewer 1

Comments on the paper from Floors et al. 'Satellite-based estimation of roughness lengths and displacement heights for wind-resource modelling'

General comments

The presented paper is concerned with improving wind-energy potential prediction in forested areas or inhomogeneous terrain, where obstacles influence the local wind profile often unpredictably. The authors' approach is to use satellite-based spatial data to retrieve the roughness length z_0 and displacement length d , instead of commonly used tables or even manual assigned parameters, e.g. derived from in-situ measurements or 'hand-digitising'. The authors compare their novel satellite-based flow modelling results to predictions based on global land cover maps, lidar scans and manual digitisation. Their goal is to increase the accuracy of wind cross-prediction and reduce uncertainties that come with the traditional methods in cross-prediction in heterogeneous terrain. Their results show a slightly better cross-prediction. The results depend on the complexity of the terrain though. The significance of the paper, however, is not only based on the potential increase of accuracy in cross-prediction, but that the satellite-based approach is using an available and suitable source for (seasonable) land coverage. The authors present a lot of data bases, methods and models aside with an exemplary data evaluation. This can be a lot at times and needs to be carefully balanced to keep it to necessary information for the reader. In general the paper is well structured and their methods are well presented. However, there are some (minor) issues with the current state of the manuscript. All issues are listed in the 'Specific comments' section.

We have reduced the amount of acronyms and databases by removing the sites Alaiz and Perdigao, which were not used in the analysis anyway, except for figure 7. We also grouped the hand-digitized and lidar scan maps in Fig. 7-8 and Table 4, which makes it easier to compare the results. We specified this class also with a different color in the figures, to make it stand out as the 'reference' which we compare the sentinel data to. Tables 1 and 2 have been simplified. All figures have improved labelling to make them stand-alone and easier to understand.

Specific comments

- I.91: Please introduce h properly as the canopy height.

The canopy height has been introduced.

- I.130: Here you discuss the plot shown in Fig. 1, especially around the shown LAI around 1. This is where the models differentiate from each other. However, the legend of the plot is overlapping with a large portion of the plot that is being discussed. This can be solved easily by moving the legend in a way that it does not interfere with the graphs. Alternatively increase the y-axis limits and lift the legend up to around 1.0-1.3 on the y-axis.

The legend has been moved outside the figure and colours have been adjusted based on comments of reviewer 2.

- I.134: You mention the differences for different α and β values in the respective models. You should state in a small statement which one you will use in the study and if you stick to the temperate pine forest. Also add the information where these kind of forest parameters shown in line 134 apply to.

We added "and we use them throughout this work due to the absence of information on the canopy profile from satellite data."

- I.256: 'Ryningsnäs is used as an example throughout this section to illustrate our approach'. This is a sentence one would expect at section 3.6. where you introduce the example 'Ryningsnäs'. You could even move Sec. 3.6 into Sec. 4.

We moved this sentence as suggested.

- I.296: You introduce the spider-grid analysis or zooming-grid analysis. Can you elaborate why this is used instead of an orthogonal grid as in the later predictions?

We added an explanation: "The advantage of using a zooming grid is that it concentrates the resolution where it is most needed and we can use arbitrarily distributed points. The latter is for example beneficial for calculating the wind climate at exact positions of wind turbines."

- I.365: Please introduce γ from Eq. 13.

Gamma is now introduced.

- I.398: [...] 'mostly' lead to lower RMS ... Maybe use more like 'in half of the cases' (6/10). And even then only by a small margin. It would be more representative of the figure.

We added 'However, at M^erida and Østerild the RMS of ϵ_p from the Sentinel based maps is not lower than those from the standard land cover databases.'

- I.404: Are those ϵ_p values averages for all the sites combined, you mention in line 404? Since you mention different sites but only one RMS and method.

We added the word "combined" to make this more clear.

- I.405: Can you elaborate why those improvements can not be shown? Or how to understand this conclusion. Do you mean the data does not show this? Because for the Cuauhtémoc site the hand-digitised results show a higher RMS. Maybe clear this paragraph up.

We changed this to: "Thus, averaged over these four sites, satellite-derived estimates of z_0 do not yield better power predictions than those based on manually digitized maps."

- I.411f: Please concretise 'westerly sector', e.g. using the easting that can be used in Fig. 6., as it additionally seems that the values for d in average double in the easterly part of the plot (more yellow and green).

We have changed this to "For the westerly sector (i.e. winds coming from 265-285), d is nearly doubled after reducing the zooming grid to sectorwise displacements (see Fig. 1), which has large implications for the predicted power density (see Fig. 6)."

- I.413: This is unfortunate as you try to make a case for the Sentinel satellite data, especially a tool for large area with no mast data to validate.

Yes we agree, but there is still added value of the Sentinel based maps as shown in the rest of the paper, despite these limitations.

- I.420: Why does Fig. 9 exist? It shows four bars which height difference is not really quantifiable from the graph. The information it is supposed to deliver could be added to Table 4 instead by adding an extra line for the original data base. Or add a column that deals with it.

We have removed the figure and added the results to Table 4 as suggested.

- I.453: This should be moved to the conclusions part of the study.

We think that discussions about future work are more suitable for the discussion.

Technical corrections

- I.165: You refer to Table 3 long before you mention Table 2 in line 206. I would prefer it, if you swap the labelling to avoid confusion.

We swapped the order.

- I.420: Wrong cross-reference. I think you mean Fig. 9 not Fig. 3.

Corrected.

- I.451: Please spell 'meters' out in this context.

This has now been spelled out.