

# 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.

## Specific comments

- I. 91: Please introduce  $h$  properly as the canopy height.
- 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.
- I. 134: You mention the differences for different  $\alpha$  and  $\beta$  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.
- 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.

- I. 296ff: 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?
- I. 365: Please introduce  $\Gamma$  from Eq. 13.
- 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.
- I. 404: Are those  $\epsilon_p$  values averages for all the sites combined, you mention in line 404? Since you mention different sites but only one RMS and method.
- 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.
- 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).
- 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.
- 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.
- I 453f: This should be moved to the conclusions part of the study.

## 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.
- I. 420: Wrong cross-reference. I think you mean Fig. 9 not Fig. 3.
- I. 451: Please spell 'meters' out in this context.