

Wind Energ. Sci. Discuss., referee comment RC2  
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## **Comment on wes-2021-28**

Anonymous Referee #2

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Referee 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-RC2>, 2021

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The authors investigate the ability of satellites to provide roughness length and displacement heights for wind resource assessment. They consider 3 satellite platforms which provide tree height, land cover, and leaf area index. Then, they use 3 forest modules to convert these data to roughness and displacement. They consider these satellite-derived estimates, along with other traditional ways (global land-cover maps; aerial lidar scans; manual digitalization) of obtaining these quantities, and perform power predictions at various sites using WASP. They take each sensor on a met tower and use it to predict the wind speed and power at all other sensors on that tower (vertical predictions) and other towers at the same site (horizontal predictions). A total of 10 sites are considered.

The main result is that WASP-derived power predictions obtained with satellite vs lidar land characteristics were comparable (similar error of  $\sim 10\text{-}11\%$ ), indicating that satellite data can be a more affordable alternative to costly aerial lidar campaigns over forested terrain.

### Major comments

The writing quality is somewhat poor. Subsection 5.3 and Conclusion are the most well-written ones in the manuscript. The rest is certainly readable, but it takes extra effort from the reader to sort through ambiguities and incoherencies. I would expect to be able to focus on the science and results being presented while performing a review, but the writing made it difficult and I spent a substantial amount of time just trying to understand the content that was being presented. Each paragraph should have a clear message and a reason to exist. Figures should stand alone with well-labeled axes and captions, and always show the units. The reader shouldn't have to read the manuscript in detail to understand what the figure is trying to show. Especially since there are so many acronyms for the datasets/models and foreign names for the site locations, it can become difficult to keep track of what's what if the text is not easy to follow.

The results presented implicitly include a validation of the WASP models themselves. I suggest that the analysis be reframed to focus exclusively on the effects of what the authors are actually testing: several ways of obtaining  $z_0$  and  $d$ . Either that, or the

narrative of the paper should be expanded to include the WASP validation that is being carried out (even if WASP is being employed in a less traditional way, using only one sensor, without the ability to fit to the entire mast profile).

It feels like 90% of my time was spent reading the introduction, data, and methodology (19 pages) and then not many results were presented (less than 3 pages). Maybe that's because some results are shown in the methodology? Either way, please consider including some of the results that are not shown (as per manuscript text) and more examples that show spatial variations. It would help the interpretation of the results if the readers could see for themselves the spatial distribution of  $z_0$  and/or  $d$  at all sites, which could be accomplished by adding a single figure with subpanels. It might also be helpful to spatially see the progression of data from satellite-derived quantities ( $h$ , land cover,  $lai$ ) to  $z_0$  and  $d$  for the best-performing and worst-performing sites. Figure 7 is also lacking discussion of some key points: e.g. I don't understand why the hand-digitized map is so bad for one of the MX sites; I don't understand why the proposed method performs worse than MODIS/GLOB/CORINE for Østerild and the two MX sites, and it's not discussed. It seems like Figure 7 can be modified and milked for a much more valuable discussion (and shown more than once, highlighting different aspects of the results, like e.g. vertical vs horizontal predictions). A key, unanswered question is: in what types of sites should one go through the trouble of deriving  $z_0$  and  $d$  from satellites vs using the global datasets?

I would like to see the authors using WASP as they say it's usually used (page 25, lines 483-487) for the sites with two masts: Alaiz, Finland, Perdigao, Sweden, Østerild to get the best prediction possible and then evaluate the effect of different  $z_0$  and  $d$  estimates. By using WASP the way it's usually used, won't you get results that are more interesting to your audience, and be able to tease out the real sensitivity to these lower-boundary forcing parameters?

Minor comments

A very large amount of minor comments is annotated directly onto the pdf

Please also note the supplement to this comment:

<https://wes.copernicus.org/preprints/wes-2021-28/wes-2021-28-RC2-supplement.pdf>