Wind Energ. Sci. Discuss., author comment AC3
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Reply on RC3

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-AC3, 2021

Reviewer 3

The authors here present a way to represent wind resource maps using new satellite products from the Sentinel-1 and -2 and the ICESat-2 satellites. They attempt to demonstrate how these new products can be comparable, or even better than, existing best practices such as aerial lidar scan and hand-digitized maps. A large emphasis is placed on the land cover datasets and the forest roughness models used in conjunction with micro-scale flow modeling with WAsP. Ultimately, a cross-prediction is performed against a variety of sites with different roughness/forest characteristics is shown, with the new satellite products outperforming the coarser land cover datasets. Finally, it is emphasized that these new satellite products are not only more accurate for wind resource assessment, but quicker to perform and are continually updated.

In general, the authors do an okay job of presenting their work. Overall, though, it does seem that the delivery of the results is a bit muffled. For example, a great deal of the paper is spent laying out the methods of the work and introducing the different datasets/models. After this, very few pages are concerned with the results. The opportunity does present itself to show more results (especially regarding sites where WaSP performs poorly).

We have largely restructured the manuscript by moving results that were presented in the methods to the results section. The methodology section has been shortened and made more concise. We discuss in more detail the sites where the model performs poorly (see comments to reviewer 2). We moved the abbreviations column to the left to make it easier to look up the landcover database abbreviations in Table 2. The sites were WAsP is performing poorly (Østerild and Ryningsnäs) are now discussed in detail.We also included roughness maps of all sites to let the reader better understand the discussion in the text.

Specific Comments

• There are a lot of acronyms. I realized this can be hard to avoid, but I found myself constantly jumping around to recall which one meant what. This is done somewhat in Table 3. A simple table in the Appendix would prove beneficial here.

We have reduced the number of names, by removing some of the sites (Perdigao and Alaiz) and moving the lidar and hand-digitized maps into one class. We moved the

abbreviations column to the left to make it easier to look up the landcover database abbreviations in Table 2.

• Time frames are generally not explained in the figures, such as Figure 6. The satellite time coverage is laid out in Table 3, but it is lost in the text and not explained in the figure captions.

We now added a reference to the time labels in the figure caption.

• Line 158: A space is needed in between "in" and "mountainous".

This sentence has been deleted due to other reviewers' comments.

• Line 182: What are the consequences of assuming that each subclass of z0 is the same as the class it inherits?

We added some discussion around this topic: "It is mostly classes with forests that have been split up and one could get a better estimation of z0by a more detailed analysis of the canopy structure in these subclasses. However, this approach is not attempted here."

• Line 190-200: I feel this discussion of these machine learning models (Random Forest and Support Vector Regression) came out of nowhere and hardly any attention is given to the specificities of these. Please elaborate the discussion around these and why they were used in the first place.

This section has been extended with references and more specific information has been added.

• Line 387 (Results in general): Results are not shown for the WAsP model in complex terrain with steep slopes, since WAsP is known not to perform well in these conditions. Shouldn't at the very least some of these results be shown? I feel we are missing some of the picture if not.

We did not show these results because the IBZ model is not applicable there and in fact the results were not used when aggrerating all sites. Instead we therefore removed these sites from the manuscript and instead focus on the sites where WAsP is within its operational envelope.

• I realize atmospheric stability is not considered in this study, but I do think that would be an interesting addition, especially since we are dealing with various forested sites, and the interactions with these sites and different stabilities could tease out further insights not previously considered. I will leave it up to the authors to include this or not.

We agree that this is an important topic, but we feel this is more suitable to treat in another paper. In fact, a paper about a new stability treatment in WAsP is currently in preparation where we go into much more detail on this.

Overall, I think there is a solid paper in here… somewhere. In essence, a greater balance between the methods and results section is needed. Reading 15-20 pages of methods for only ~5 pages of results/discussion is tough. There are some opportunities to include some more results (as explained above), and I believe this could help round out the paper, along with a restructuring of the methods.

The methods have been largely restructured and the results section has been extended discussing more about the different sites.