Brief communication: An elliptical parameterisation of the wind direction rose (wes-2024-187)

Response to Reviewer 1

Dear Reviewer,

Thank you for taking the time to consider this submission to WES. I am grateful for your insights, and your helpful comments and suggestions. I will revise the manuscript in-line with your suggestions, and I believe it will be much improved as a result.

Please find my detailed responses below, where I also include your comments in **blue**:

This paper presents a parametric wind direction rose model based on an ellipse and demonstrates how the model, which includes 3 parameters, can be fit to measured wind direction data for a variety of sites. There is little published on parametric wind direction rose models, so this is a novel contribution to the literature. When combined with wind speed distributions, the model could potentially serve as a standard wind rose definition for computing wake losses, lifetime loads from wakes, and wind farm control benefits for a wind farm, similar to how the Weibull distribution is used to model wind speed probabilities for annual energy production and fatigue load calculations for individual turbines. The parameters of the elliptical wind direction rose model could be used to standardize the characterization of wind direction distributions in the wind industry. They could also be varied to explore the sensitivity of wind plant performance, loads, and wind farm control strategies to different wind direction distributions.

I am glad you agree this is a novel contribution to the field, with a number of potential applications.

The main comment I have is about how the elliptical parameterized wind rose can be made more useful for sites with more complex wind direction distributions. The presented parameterization works well for sites with unimodal wind roses or bimodal wind roses where the prevailing wind directions are in opposite directions, as shown in Fig. 3. However, similar to what is shown in Fig. 3e, there are many sites where the most common wind directions are not 180 degrees apart. To provide another example, in Fig. 3 of Bensason et al. 2021 (https://pubs.aip.org/aip/jrse/article/13/3/033303/285076/Evaluation-of-the-potential-for-wake-steering-for), the most common wind direction sectors are from the northwest and south. It would greatly strengthen the paper to discuss possible extensions of the elliptical wind rose model that could more accurately describe these types of wind direction distributions. For example, could you consider a linear combination of elliptical wind roses with different prevailing wind directions such that the sum of the distributions integrates to 1? This could be a nice solution if the different prevailing wind directions could be included as optimization parameters, rather than being identified manually. Of course the idea behind a parameterized wind rose model is to keep it relatively simple, but considering only wind roses with prevailing wind directions 180 degrees apart might be too much of a simplification for many sites. Among other issues, this could be important when estimating wake losses at a site with long distances between rows of turbines but close spacing in the perpendicular direction. Underrepresenting the likelihood of common wind directions aligned with the close turbine spacing in order to fit the wind rose model to the prevailing direction might cause wake losses to be significantly underestimated.

You raise an excellent point here, and I agree that this brief communication would be improved by discussing/outlining possible extensions to the more complex cases you describe. While developing the parametric wind direction rose model (I'll refer to this as

PWDM) I did indeed consider the opportunity to extend to a PWDM mixture-module by taking a weighted linear combination, and constraining the weights to sum to 1. A benefit of this approach would be that more complex sites could be represented, a drawback would be the associated uplift in the numbers of parameters to be fitted via optimisation (both of which you identified in your comment). Having said that, most mixture-model cases would likely only require two PWDMs to be included, and one might simplify further by assuming the prevailing wind directions for each might be set manually (which still allows for standardisation and sensitivity analyses, if not complete automation). Irrespective of the particular implementation, I agree these possible extensions should be discussed in the manuscript. I will therefore add a new section on “Mixture-model extensions for more complex sites”.

Comments:

1. Pg. 3, ln. 60: Is this equation only supposed to be valid when theta^tilde_1 and theta^tilde_2 are less than or equal to pi/2? If so, please clarify. Also, looking at Fig. 1b, how is y^tilde_+(x) defined for x_2 < x <= x_1? The segment area is no longer bounded by two lines like it is for x < x_2.

Yes you’re right that the equation is valid (and later applied) only where both angles are less than or equal to pi/2, but you’re right that this should be clarified here.

In the queried region, y+ is determined by the circle itself. This should be clarified in the manuscript also, thanks!

2. Pg. 4, ln. 82: "not be equally" -> "not equally"

Will fix, thanks!

3. Section 2.4: Scaling the wind direction probability by 1-f for pi/2 < theta_c,i < 3pi/2 and 1+f for 0 <= theta_c,i < pi/2 or 3pi/2 < theta_c,i <= 2pi causes a sharp discontinuity at theta_c,i = pi/2 and 3pi/2, which doesn’t seem very realistic. Would a smooth (e.g., linear) transition from 1-f to 1+f be more appropriate? One example would be scaling P_el by (1 + f - (2*f/pi)*theta_c,i) for 0 <= theta_c,i < pi. This way you would still get 1 - f for theta_c,i = pi, 1 for theta_c,i = pi/2 and 1 + f for theta_c,i = 0.

This is an interesting point you raise. My thoughts are as follows:

1) The discontinuity in scaling occurs across the smallest probability bins of the elliptical wind rose (with symmetrically larger bins either side). As a result the outcome of this tends to be a scaled wind rose in which the bin probabilities increase fairly smoothly across these points. In many cases there is therefore no clear discontinuity present for the final parametric wind rose. But you are right that a more marked discontinuity will appear at small bin sizes, even given the above factors.

2) While a discontinuity is introduced in the scaling factor as you describe, and in many scenarios discontinuities can be a problematic, those problematic cases tend to be where the rate-of-change or smoothness of a function has a bearing on the result. For a wind rose we’re simply representing the probabilistic “weight” associated with each direction bin, independently of the others (barring the restriction that the total probability must be 1). I’d argue that a discontinuity therefore isn’t *intrinsically* a problem here, and that the key question is more along the lines of: “do we produce realistic looking wind roses and/or obtain a good fit to real data?” The current model seems to allow us to

answer yes to both of these, and so I don't believe the highlighted discontinuity is a major issue or impediment to the usefulness of this model at this stage.

3) Having said that, alternative "folding" parameterisations (such as the linear one you suggest) would open up new shape variations and could provide superior fits in some cases. Therefore, while I don't believe there is an immediate need to alter the original folding approach, I believe there is value in the possibility for alternative approaches being described in manuscript, supporting more flexible implementations within future work. I will therefore include such a discussion when revising the manuscript, and I thank you for this valuable suggestion!

4. Section 2.5: Could you also optimize the prevailing wind direction theta_prev when fitting a wind rose to empirical data?

You could indeed include the prevailing wind direction as an optimisation parameter, however I believe local minima would become a problem when trying to ensure robust parameter identification. Another option would be to exhaustively test each possible direction, but this feels inelegant as a solution. Instead I worked with the heuristic that the "best" prevailing wind direction for the model would likely be either the highest probability direction, or the (circular) mean direction, and that if neither of these resulted in a good fit (indicated by high R^2) then a good fit was unlikely to be obtained using any direction. This heuristic has worked well across all cases tested thus far. There is certainly room for this aspect of model fitting to be explored in more detail, and so I will include the above discussion when updating the manuscript.

5. Pg. 6, equation after line 110: in the first two lines, it would clarify the equation if "i" were added as a subscript for P^dagger_g because this represents the probability of the specific bin "i".

Agreed, I'll add that!

6. Pg. 6, ln. 115: "The partial derivative del P^dagger_g / del a is readily obtained using del A_theta_1,theta_2 / del a…": To help the reader, it would be good to refer to the specific equations earlier in the text that show how these two partial derivatives are linked. This might require more equations to be numbered.

Good point, I'll do that!

7. Section 3: It would be helpful to discuss the choice of bin sizes shown. What are typical wind direction bin widths for wind roses in the wind industry, for example for energy yield assessments or controls analysis?

Agreed. In my experience it's from 5 to 30 degrees, but I'll double check the certification requirements so I can add that helpful additional context.

8. Pg. 7, ln. 130: "the RMSE-scale is dependent on the number of wind direction bins." Couldn't the RMSE be normalized to account for the number of wind direction bins so it can be used to compare the goodness-of-fit for roses with different bin sizes?

Yes, this is essentially what R^2 is doing for us. I'll point that out when revising the manuscript. The reason why it's nice to have both is that it ensures one has a normalised (R^2) and absolute (RMSE) measure for goodness of fit.

9. Pg. 7, ln. 131: "Limitations of R^2 should be kept in mind" Please briefly discuss these limitations here.

I'll add some details to the manuscript as you suggest. This will also allow me to indicate more explicitly why those limitations aren't a major issue in this application.

Thanks again,

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