General comments:

The manuscript “Wind data assessment and energy estimation for potential future offshore wind farm development areas in the Scotian Shelf” by the authors Yongying Ma, Jinshan Xu, Yongsheng Wu, Michael Z. Li, Ryan Stanley, Brent Law and Marc Skinner presents a study on the assessment of the wind potential in the Scotian shelf. Wind speed and wind direction data from four different reanalysis data sets (ERA5, CFSv2, NARR, HRDPS) are compared vs. observational data collected in that region in order to identify those model data sets that show the best agreement with observations (ERA5, HRDPS). Data from these data sets is then used to provide input data for wind farm simulations with the tool PyWake for several potential wind farm sites in the Scotian Shelf. The authors carry out a sensitivity study in which they investigate the dependency of the power output of the simulated wind farms from the distance between two wind turbines in those wind farms.

The manuscript does not supply the reader with new methodologies, but applies for the first-time standard tools and data sets to the assessment of wind resources in the geographic region of the Scotian shelf. While the language and overall structure of the manuscript is fine, in my opinion the readability of the paper could profit considerably from slightly restructuring its contents. My suggestion would be e.g. to organize the report on the performance of the different reanalysis data sets not by the metrics but strictly by the data sets. The reader might be most interested in an overall assessment of the data sets and less in a slightly too detailed presentation which data set provides the smallest bias, which the smallest RMSE and so on.

We thank the reviewer for this suggestion. We have adjusted the structure of Section 3 to present the evaluation results in a flow for each wind dataset of ERA5, HRDPS, CFSv2, and NARR.

I have a couple of specific comments which I ask the authors to consider when revising their manuscript.

Specific comments:

- **Lacking wind measurements at larger height:**
  According to table 1 the largest measurement height from which data was accessible to the authors was 16 m. Modern wind turbines operate in much larger heights. I’m wondering how transferable the results on the quality of the different reanalysis data sets assessed by the authors for heights close to the ground actually are to larger heights. In my opinion the authors should discuss this point when assessing their results.

We thank the reviewer for pointing out this limitation. Due to limitation of available observation data, there is potential uncertainty of assessing the performance of wind datasets at hub height. We added a discussion to Section 5.1 addressing this fact that the observational wind data used for validation were from relatively low heights (maximum 16 m), which is much lower than the hub height of modern offshore wind turbines (e.g., 150 m).

We acknowledge that while extrapolation was used, performance at higher altitudes may differ due to atmospheric conditions and vertical wind shear. We recognize this as a limitation of our study and suggest that future work include validation using higher-elevation measurements, such as lidar or tall towers, to improve assessment accuracy.

- **Lacking inclusion of atmospheric stability:**
  The vertical wind profile, the turbulence in the marine atmospheric boundary layer and the wake recovery are in practice dependent on the atmospheric stability. However, this parameter is not discussed at all in the manuscript. The authors should at least explain why they have excluded this parameter from their analysis and what the non-consideration of atmospheric stability means for the uncertainty of the results presented by the authors.

  Moreover, I'm lacking a discussion on the impact of atmospheric stability on the seasonal changes of the wind conditions in the Scotian Shelf area. Can seasonal changes in the error metrics be related to lacking consideration of atmospheric stability in the analysis?

  We thank the reviewer for highlighting the importance of atmospheric stability in shaping wind profiles and wake recovery. As suggested, we have added a discussion of atmospheric stability to the revised manuscript.

  In the section on wind speed extrapolation (Section 2.4), we now clarify that a constant wind shear exponent ($\alpha = 1/7$) was used to extrapolate observed wind speeds from 5 m to 10 m at buoy sites. The choice to exclude the effects of atmospheric stability in this step was due to the lack of available vertical wind profile or stability-related observation data at those locations.

  However, for extrapolating wind speeds from 10 m to the turbine hub height (150 m) using wind datasets (i.e., ERA5), we implemented a more refined approach. Specifically, a spatially and temporally varying wind shear exponent was calculated using wind speeds at two heights (10 m and 100 m), allowing for a more realistic representation of vertical wind shear in the ambient atmosphere.

  Additionally, in the Discussion section (Section 5.1), we have included comments on the uncertainty introduced by excluding atmospheric stability in the extrapolation of observed buoy data. We also added text in Section 5.1 discussing how seasonal patterns in wind speed error metrics may, in part, reflect the unaccounted influence of atmospheric stability. For instance, stronger stratification in summer and more unstable conditions in winter could contribute to the observed seasonal variation in the metric of bias.

- **Interpolation of NARR data in time:**
  My suggestion would be to compare the different data sets for a temporal resolution of three hours with each other. The interpolation in time might introduce another uncertainty that is not in the original NARR data itself. It should be possible to quantify the impact of the interpolation in time by comparing error metrics for the original NARR data in the gap-filled NARR data with each other.

  We thank the reviewer for this thoughtful comment. Upon careful re-examination of our methodology, we realized that the original description in the manuscript was inaccurate. In fact, for all datasets, including NARR, we interpolated the wind speed and wind direction data to match the observation timestamps, not to match the hourly resolution of ERA5 dataset as initially stated. We have corrected the description in the revised manuscript to accurately reflect this processing step.

- **Filtering for wind speeds between 2 m/s and 17 m/s:**
  In my opinion it would be also an important criterion whether a reanalysis data set gives the right number of events with wind speeds above cut-out wind speed. I suggest to add such an analysis to the existing analysis. Or is the number of such events too low to have an impact on the calculation of the energy yield in the end?

  We thank the reviewer for this suggestion. As recommended, we have examined the percentage of wind speed events above the cut-out threshold (17 m/s) at 10 m height. At the end of Section 2.5, we have added the following text:
  "*The percentage of time with 10-m wind speeds exceeding 17 m/s was estimated using the ERA5 dataset. These strong wind events occurred approximately 0.3% of the time at both nearshore sites and between 1.8% and 2.5% at offshore sites.*"

- **PyWake:**
  In my opinion the current description of the wind farm model does not contain all the information that would be required by the user to repeat the calculations of the authors. Therefore, I ask the authors to extent the description of the setup of their PyWake runs. E.g., how has the background turbulence intensity been considered in these simulations? Is the model applicable also for calculations of wind turbines that operate in the near wake of other wind turbines? With the smallest turbine distances assumed in the sensitivity study of the authors they might already be in the near-wake range. This is an important comment e.g. for the accuracy of the results presented in figure 9.

  We thank the reviewer for pointing out the need for a more detailed description of the PyWake setup. In the revised manuscript, we have expanded Section 2.6 to include additional information on the wake model configuration and input parameters used in our simulations.

Specifically, we used the Gaussian-profile wake deficit model developed by Bastankhah and Porté-Agel (2014), as implemented in PyWake. This model assumes self-similarity in the velocity deficit profile, which is supported by experimental findings from Medici and Alfredsson (2006). The model has been validated against both wind tunnel measurements and Large Eddy Simulations by Bastankhah and Porté-Agel (2014), and has shown good agreement for downstream distances greater than approximately 2–3 rotor diameters (D). In our sensitivity study, the minimum turbine spacing was set to 2 D, which lies at the lower bound of the model's validated range. We have clarified this point in the revised manuscript and now include a statement acknowledging that some near-wake effects may not be fully captured at this lower spacings of 2–3 D.

Additionally, we have added a description of the ambient turbulence intensity used in our simulations. A constant turbulence intensity of 0.1 was assumed for all scenarios, as now stated in Section 2.6. We also acknowledge in the Discussion section that the use of a constant turbulence intensity and the application of the Gaussian wake model at short turbine spacings may introduce uncertainty into the results.

- **Error metrics for the wind direction:**
  As averaging of wind directions is often not made correct I encourage the authors to sensitize the readers and present more details in how they handled the jump of the wind direction at 360°/0° in their analysis.

  We thank the reviewer for this comment. Wind direction, as a circular variable, indeed requires careful handling during interpolation and averaging to avoid spurious results. In the revised manuscript (Section 2.3), we have added a detailed explanation of how angular data were treated.

  Specifically, we noted that direct arithmetic operations on wind direction (e.g., averaging 10° and 350° yielding 180°) can lead to incorrect conclusions due to the discontinuity at 360°/0°. To properly address this, we adopted the method described by Berens (2009). Wind directions were first converted to unit vectors via their sine and cosine components. Interpolation and averaging were then applied separately to these components, after which the result was converted back to an angle using the four-quadrant inverse tangent function.

- Page 3, line 81: What is meant by characteristic wind speed in this context?

  We appreciate the reviewer pointing out this ambiguity. Our original use of the term "characteristic wind speed" was intended to refer to the typical time-mean wind speeds across the five stations, which were around 6 m/s. However, we agree that the term may be unclear or misleading in this context. We have revised the sentence to the following for improved clarity: "*The all-time mean wind speeds at the five stations ranged from 5.35 m/s to 6.18 m/s, with biases between −0.64 m/s and 0.59 m/s, and correlation coefficients close to 0.8.*"

- Table 2: I'm wondering whether this table is actually required. E.g., the information on the time range and the spatial coverage is not of importance for this manuscript.

  We thank the reviewer for this suggestion. After reconsidering the content, we agree that Table 2 is not essential to the manuscript. The key information it contained, such as temporal and spatial resolution, has already been clearly described in the text. To avoid redundancy, we have removed Table 2 in the revised manuscript.

- Page 7, line 174: What is a "naturally-stable" atmospheric condition?

  We thank the reviewer for pointing this out. The term "naturally-stable" was a typographical error. It should have read "neutrally stable atmosphere." We have corrected this in the revised manuscript and also expanded the relevant discussion regarding atmospheric stability in the extrapolation of wind speeds.

- Page 8, line 175: Power law exponent 1/7. Don't ERA5 and HRDPS provide data on other heights as 10 m? If they provide such data, I suggest to determined the power law exponent from the reanalysis data sets. Or is there a special reason why the authors trust more in the 10 m wind speeds than in the wind speeds from other heights in these data sets?

  We appreciate the reviewer's helpful suggestion. In the revised manuscript, we now use ERA5 wind speeds at both 10 m and 100 m heights to calculate a time-varying wind shear exponent, α, at each hourly time step. This α is then used to extrapolate wind speeds to the turbine hub height of 150 m. This approach provides a more realistic extrapolation than that using a fixed constant exponent, e.g., 1/7. The methodology has been described in Section 2.4.

- Section 4.1: I'm wondering whether the wind farm simulations for just the seasonal mean wind speed are sufficient here. What does this tell us concerning the energy yield to be expected when the power in the wind is actually depending on the cube of the wind speed?

  We thank the reviewer for raising this important point. The use of seasonal mean wind speed and direction in Section 4.1 was intended to isolate and better understand the impact of turbine spacing on total power production, while also reducing computational costs by avoiding year-long simulations. We acknowledge that wind energy yield is proportional to the cube of wind speed, and therefore, using mean wind speeds alone may not accurately capture the true seasonal energy output.

To clarify this limitation, we have revised the second paragraph of Section 4.1 to include the following text:
"*However, it is important to note that because wind turbine power output is proportional to the cube of wind speed, the simulation results using seasonal mean wind speed do not accurately represent seasonal mean energy yield. In Section 4.3, simulations for each PFDA were performed using time-varying wind speed and direction data to provide more realistic estimates of energy production.*"

- Table 5: The explanation of xm and xt should be presented before table 5 is presented. I had difficulties to interpret these parameters without having read the information on these parameters in the text.

  We appreciate the reviewer's comment. To improve clarity, we have revised the caption of Table 5 to explicitly note that the parameters x_m and x_t refer to the results of the piecewise function fitting described in Section 4.2.

Technical corrections:

- Abstract, line 1: "The Scotian Shelf is one of the top wind regimes in the world." Regimes should be replaced by regions.

  We thank the reviewer for catching this wording issue. We have replaced "wind regimes" with "wind regions" in the abstract.