This study presents a valuable attempt to address the scarcity of offshore wind speed profile data by developing a machine learning-based extrapolation model utilizing satellite-derived wind data, which could benefit wind resource assessment along U.S. coasts. The authors should be commended for their extensive data collection and model validation efforts across multiple coasts, showcasing the model's performance relative to traditional methods like the Logarithmic and Power Laws. However, while the work offers interesting results, there are notable areas where the study could be improved. Primarily, the claimed novelty of the research is somewhat diluted, as the Random Forest Regression (RFR) model has already been widely applied in similar extrapolation studies, and the spatial and temporal resolution of the resulting dataset is still coarser than other products like NOW23. Additionally, the reliance on standard stability assumptions and shear exponent limitations in the comparison methods may not fully reflect real-world conditions, and the leave-one-out cross-validation (LOSOCV) approach, though thorough, detracts from the goal of developing a single, generalizable model. Furthermore, the study would be strengthened by more focus on the practical application of the new dataset (NOSP) and its insights for wind energy stakeholders, potentially incorporating offshore wind resource assessments and Levelized Cost of Energy (LCOE) analyses. These enhancements, along with restructuring of certain sections and clarifications in the visual elements, could provide a clearer scientific contribution and highlight the value of the NOSP dataset for the wind energy community.

After careful evaluation, I find that the manuscript, in its current form, lacks sufficient novelty to warrant publication. To enhance the paper's contribution, I recommend the authors consider incorporating novel elements that extend beyond current literature, such as using the derived dataset (NOSP) for offshore wind resource assessments or cost analyses (e.g., LCOE calculations) that can directly benefit the wind energy industry. If such advancements are added, the work would more clearly demonstrate its unique value.

Specific comments:

1. With the current pace of growing wind turbine sizes, it is of paramount importance to accurately measure the wind speed profiles, rather than just hub height wind speed. Introduce the importance of wind speed profiles, in terms of their interaction with wind turbines and their impact on loading and power estimation.
2. In addition to tall towers and lidars, sodars also serve as wind profile measuring instruments. Introduce them. Also, lidars have several limitations, such as failed to measure wind speed during raining, snowing, or other precipitation events; no measurement without aerosols. Instead of "lidars are very accurate alternative devices", at lines 54, introduce the limitations as well.

3. Line 59: 20 years of mesoscale model simulated hindcasts of wind profiles (NOW23) can offer a concise resource assessment, which are sufficient in temporal scale, covering annual to decadal analysis, and at a high resolution of 2 km. It doesn't appear to be a gap in the long-term wind speed profile knowledge along the US coast. Rephrase this sentence.
4. Lines 60-64: The authors claim the novelty lies in the idea of bridging critical database gap. However, the NBSv2.0 gridded dataset available at a resolution of 0.25 deg, and at a temporal resolution of 6 hours, which is poorer in temporal and spatial resolution, than NOW23, except for the coverage. Here it dilutes the novelty.
5. There are several studies extrapolated wind speed from surface to 200 m level, utilizing random forest regression (RFR). A very recent study under review is Rouholahnejad et al. (2024), which did exact similar work. The authors could have referred to this article. In addition, the RFR methodology has been adopted in several other studies, showcasing the better predictability in low-level jet cases and high shear events, compared to the conventional low-logarithmic law power law. In comparison to the existing studies, there seems little added value in this study to the scientific community.

   Rouholahnejad, F. and Gottschall, J.: Characterization of Local Wind Profiles: A Random Forest Approach for Enhanced Wind Profile Extrapolation, Wind Energ. Sci. Discuss. [preprint], https://doi.org/10.5194/wes-2023-178, in review, 2024.
6. In fact, this current study resembles extension of Optis et al., 2021. However, there is limited scientific addition in novelty.
7. Lines 94-96: the authors state that the main novelty of this present work lie in extrapolating wind speed profiles on a larger spatial scale covering multiple coasts. However, they focused vastly on the RFR model and its validity compared to the conventional methods, which in fact several researchers have already reported. Similar to the previous studies, the current study also failed to accurately estimate the LLJ cases. If the authors have captured the LLJ events, it could be a stand-alone novelty.
8. Lines 143-150: this paragraph seems out of order and introduces confusion, since the ML methodology has not been explained. For a clarity, remove this from here and explain it when introducing the RFR model training.
9. One major drawback of this study is using the conventional Logarithmic Law and Power Law for comparison, which have been proven to be inadequate in extrapolating wind profiles, due to their dependance on stability conditions and shear exponent. Also, the assumption of neutral conditions for Logarithmic Law and a constant sear exponent for Power Law already introduces substantial errors in wind profile

extrapolation. To make the study novel, it is better to consider all the stability conditions and variable shear exponent in estimating wind speed profiles.

10. The authors gave a tremendous importance to the leave-out-station-out cross validation. It is common practice to split the data into training and validation, to optimize the model parameters. Instead, the authors constructed five RFR models by leaving one station at a time, and testing the model accuracies on these stations. This actually comes into "testing" the model accuracy, but not the "validation". The authors should understand that the wind energy community surpassed validating the accuracy of RFR model, rather a unique model accurate enough to extrapolate the entire wind speed profile is needed. Instead of bombarding the manuscript with LOSOCV model, the authors should bring new insights from the one single optimized RFR model.
11. The authors conducted feature importance and came up with only two variables, namely 10 m wind speed and temperature difference. The model trained with these parameters is termed as "Optimal LOSOCV RFR", while the model trained with all features is termed as "Basic LOSOCV RFR". However, training a RFR model requires least computational resources, thus eliminating features doesn't necessary. The entire manuscript compares Optimal vs Basic LOSOCV RFR models, which makes the manuscript more like a lab report, rather than a scientific article. The authors should come out of the thought of evaluating multiple RFR, but rather bring new novelty.
12. The authors should consider feature engineering, rather than merely feature importance.
13. Same as previous studies, this study also reports poor performance in LLJ cases. In fact, the RFR model predicted profiles doesn't even fall under LLJ cases, since no jet nose and increased/decreased shear below/above the nose are identified (though the authors did not show the RFR predicted profiles, it is evident from the bias plots). This further hinders the novelty of this study.
14. Sections 5.1 and 5.2 should be reduced with limited metrics in the text. Rather keep the metrics in the figures. Instead of reporting the metrics in quantitative way, explain the reasons behind the poor/better performance, which gives a scientific reasoning to the reader.
15. Figures 3-12: Increase the font size and make them clearer.
16. Tables 2-3: Comparing with the conventional Logarithmic Law and Power Law does not bring any new scientific insights, rather repeats what the previous studies have found.
17. The authors stressed that the RFR model can perform skillfully around the coast of the contiguous US, including regions not included in the training data. However, it is

paramount of importance to note that the RFR model doesn't know which locations the data comes from (since no lat/lon information are provided), rather it only knows the correlations within the data.

18. The authors spent vastly on validating the RFR model in comparison with conventional methods but spent very little on the generation of NOSP and the insights from this data. To make this study novel, conduct offshore wind resource assessment using NOSP, and several wind turbine models. A Levelized Coast of Energy (LCOE) using NOSP could be a novelty.
19. Lines 537-657: this analysis is not necessarily in the main manuscript, since the ERA5 and NOW23 are expected to be correlated, due to their parent/child relation. Move it to the Supplement.
20. The last paragraph of results seems be out of context. Could you elaborate why this was explained here?

Minor comments:

1. Limit abstract to one paragraph, only providing necessary overview, without describing in detailed.
2. Lines 134-135: the NOW23 is generated by using the WRF model, provided ERA5 reanalysis. Rewrite the sentence "this product implements the WRF ..."
3. At this point, the RFR model has been widely used in wind speed extrapolation. Thus, move the RFR model description to supplement, and only explain the training with detailed flowchart of inputs and targets.
4. Tables: Put the captions on top of the table.