

Synopsis:

The manuscript with the title “Characterization of HRRR simulated Rotor Layer Wind Speeds and Clouds along Coast of California”, which was submitted for publication to the journal Wind Energy Science by the authors Jungmin Leem, Virendra P. Ghate, Arka Mitra, Lee M. Miller, Raghavendra Krishnamurthy and Ulrike Egerer, deals with the evaluation of wind speed and cloud forecasts by the HRRR model for two offshore sites off the coast of California. Data from 3-hour forecast from HRRR are compared with data collected with lidars and additional meteorological sensors on two buoys as well as with satellite data. According to the results presented by the authors the observed seasonal cycle of cloud top height is well reproduced at both sites by the HRRR model. However, in the warm season stratocumulus cloud top heights are underestimated by HRRR, especially at one of the two sites. Another finding by the authors is that clear sky conditions come along with larger wind speeds at the the two sites investigated than cloudy conditions. Clear-sky conditions come also along with a larger bias of the wind speeds predicted by HRRR, although the sign of the bias is different at the two sites for which the authors did the analysis.

Evaluation:

Understanding how the presence of clouds inside the atmospheric boundary layer changes the wind speed in the atmospheric boundary layer is a topic of high relevance for the wind energy community. The understanding of such processes is a key for modeling these processes correctly and thus enabling also an improved modeling of wind conditions. Thus, in my opinion the topic of the manuscript is interesting for the wind energy community and is basically suitable for being published in Wind Energy Science. However, I have a few comments on the current version of the manuscript that might support sharpening of the clarity of the paper. Moreover, I have a couple of minor comments. Therefore, my recommendation is that in the next step the authors should apply some corrections to the current version of the manuscript before a decision on its publication in Wind Energy Science can be taken.

We would like to sincerely thank you as a reviewer for your thoughtful and constructive comments. Your insights have greatly helped us improve the clarity and overall quality of the manuscript. We have carefully addressed all suggestions in blue and believe the revised version benefits substantially from your valuable feedback.

Comments on the content:

#1: Abstract: From my point of view the authors could and should present the objectives of the paper more clearly in the abstract.

Following your suggestion, I revised the abstract section. The revised abstract is provided below in my response to comment #2.

#2: Abstract: The authors claim that “the findings from this study will potentially inform how to improve the modeling of wind resources off the coast of Northern California”. However, there are no clear advices for the next steps towards improving the modeling of wind resources given in the manuscript. Therefore, I think that either the respective sentence in the abstract should be revised or the conclusions part of the manuscript should be extended with corresponding content.

Following your advice, I revised the abstract to have a clear objective that is not for the model improvement. Below is the revised abstract.

Stratocumulus clouds, with their low cloud base and top, affects the atmospheric boundary layer wind and turbulence profile, thereby modulating wind energy resources. GOES satellite data reveal an abundance of stratocumulus clouds in late spring and summer months off the coast of Northern and Central California, where there are active plans to deploy floating offshore wind farms at two lease areas (near Morro Bay and Humboldt). From fall 2020, two buoys equipped with multiple instruments including Doppler lidar were deployed for about one year in these wind farm lease areas to assess the rotor-layer wind conditions in these locations. The objective of this study is to evaluate how well the High Resolution Rapid Refresh (HRRR) model represents stratocumulus cloud characteristics and turbine-relevant rotor-layer winds (surface to 300 m) by comparing HRRR simulations with buoy and satellite observations. We first find that the HRRR model reproduces the seasonal cycle of cloud-top height reasonably well in these regions. However, during the warm season—especially at Morro Bay—the HRRR-simulated stratocumulus clouds tend to have lower tops by about 150 m and exhibit weaker diurnal cycles than satellite observations. Our analysis also shows that rotor-layer wind speeds and vertical shear are stronger at Humboldt than at Morro Bay, and both are generally stronger under clear-sky conditions. Finally, the HRRR model bias in rotor-layer wind speed is small under cloudy conditions but larger and dependent on observed wind speed under clear skies. Specifically, HRRR underestimates wind speeds at Morro Bay and overestimates them at Humboldt under clear-sky conditions.

#3: General comment: The literature review presented in the introduction is rather short. A more extensive introduction e.g. into stratocumulus-topped ABL could be valuable for the reader. I think e.g. from the paper by Kopec et al. (2016) some relevant information on stratocumulus-topped ABLs could be presented. Concerning the HRRR model I'm missing some examples of previous studies that evaluated that model (especially for wind energy purposes, but also information on other evaluation studies

might be interesting). Moreover, the model itself could be presented in more detail. E.g., what are the initial and boundary conditions used?

Kopec, M. K., Malinowski, S. P., Piotrowski, Z. P., 2016: Effects of wind shear and radiative cooling on the stratocumulus-topped boundary layer, *Q. J. R. Meteorol. Soc.*, 142, 3222-3233, <https://doi.org/10.1002/qj.2903>

Thank you so much for the suggestion. I added findings from Kopec et al (2016) in the introduction to provide detailed processes in stratocumulus-topped ABLs. Below is the paragraph added in the Introduction:

Recent modeling and observational studies have provided deeper insight into the physical processes governing the stratocumulus-topped boundary layer (STBL). In particular, Kopec et al. (2016) investigated how radiative cooling at the cloud top and wind shear across the capping inversion jointly influence turbulence generation and cloud-top structure using large-eddy simulations based on the POST field campaign. Their analysis demonstrated that radiative cooling intensifies convective circulations within the STBL, while wind shear enhances turbulence and mixing in the inversion layer above the cloud, often producing a distinct turbulent sublayer that is dynamically decoupled from the convective motions below. These findings highlight the importance of representing both shear- and radiation-driven turbulence for accurate modeling of stratocumulus dynamics and their influence on boundary-layer winds. Such processes are particularly relevant for offshore wind resource assessment, where low-level clouds modulate turbulence and wind shear within the turbine rotor layer.

I also added a paragraph in the revised introduction section with a couple examples of HRRR evaluation in the wind energy context. The added paragraph is provided below:

Several prior studies have evaluated the HRRR model relevant to hub-height wind speed assessment. Liu et al. (2025) benchmarked hub-height wind speeds from HRRR analyses against multi-source observations across the southeastern United States and found that wind-speed biases were strongly influenced by local topography and land surface characteristics such as forest canopy height. Complementary efforts under the Second Wind Forecast Improvement Project (WFIP2) assessed experimental updates to the HRRR model using scanning Doppler lidar measurements in the complex terrain of the Columbia River Basin (Pichugina et al., 2020). That study demonstrated that the HRRR model's wind-speed errors were largest below about 150 m above ground level and that improvements in model physics and grid resolution led to modest but consistent reductions in mean wind-speed bias. Together, these findings highlight the importance of evaluating HRRR performance in diverse environments, including

offshore regions where boundary-layer cloud processes can further influence rotor-layer winds.

Also, HRRR description is extended, and the change is presented in the response of minor comment #8.

Liu, Y., et al. (2025). Benchmarking near-surface winds in the HRRR analyses using multi-source observations over complex terrain. *Journal of Applied Meteorology and Climatology*. <https://doi.org/10.1175/JAMC-D-24-0163.1>

Pichugina, Y., Banta, R., Brewer, W., Bianco, L., Draxl, C., Kenyon, J., Lundquist, J., Olson, J., Turner, D., Wharton, S., Wilczak, J., Baidar, S., Berg, L., Fernando, H. J. S., McCarty, B., Rai, R., Roberts, B., Sharp, J., Shaw, W., ... Worsnop, R. (2020). Evaluating the WFIP2 Updates to the HRRR Model Using Scanning Doppler Lidar Measurements in the Complex Terrain of the Columbia River Basin. *Journal of Renewable and Sustainable Energy*, 12(4), Article 043301. <https://doi.org/10.1063/5.0009138>, <https://doi.org/10.1063/5.0009138>

#4: Line 53/54: "... despite HRRR being one of the most widely used forecasting tools in wind energy resource assessments" Please add references for this statement. Is this forecasting tool applied for wind resource assessment (i.e. derivation of wind speed information for periods with a length of decades)? My understanding is that mostly models run in hindcast mode are used for wind resource assessment studies. The limited time available for producing a forecast limits e.g. the time that can be spent on the data assimilation process. Moreover, later on it is stated that HRRR is applied for wind power forecasting. I would see this as something different from a wind resource assessment.. HRRR does not output wind power directly. Thus, there might be wind power forecasting systems that make use of HRRR results for generating a wind power forecast, but I think using HRRR alone will not allow you to do a wind power forecast. To summarize, I ask the authors to add clarifications concerning the mentioned points to their manuscript.

You have the valid point. We apologize for any confusion with the wording "wind resource assessment". We clarified it by replacing "wind resource assessment" with something in line of "skill in representing rotor-layer wind conditions" throughout the manuscript.

The reference for "... despite HRRR being one of the most widely used forecasting tools in wind energy resource assessments" is provided as part of the response to comment #3.

#5: Line 94/95: "The HRRR reported wind profiles were linearly interpolated ..." Why has a linear interpolation been applied. In the surface layer the wind profile is expected to be logarithmic. Wouldn't it therefore make more sense to apply a logarithmic interpolation in this area? Another possibility would be to apply a power law profile for the wind speed and use this for interpolation.

You are right about this. However, the heights where HRRR data is available (for instance 38.4, 87.5, 166.9 and 242.4 meter height) was close to the HRRR model level. Also, in the rotor-layer range, the logarithmic curvature is small especially over ocean or weakly stratified boundary layers. Therefore, we thought there would be a minimal impact from the interpolation method.

#6: Line 97-99: The current description "we extract the corresponding lidar data for each hour" could still be made more precise, e.g. as follows: As HRRR provides data only at a full hour we also use only 10-minute averaged lidar data with the same timestamp as that of the forecast.

Thank you for the suggestion. We revised the sentence following your suggestion as shown below:

Because HRRR outputs are available only at full-hour intervals, we selected the 10-minute averaged lidar measurements whose timestamps correspond exactly to each forecast hour, ensuring consistent temporal alignment between the HRRR time series and the buoy observations.

#7: How do the authors deal with the fact of HRRR and lidar data being effectively different types of averaged data? E.g. does 10 minute averaged lidar data compare better to HRRR than 30 minute averaged lidar data? Did the authors check for a possible phase shift between the lidar and the model data? Why did the authors decide not to interpolate the model data in the horizontal directions of space to the position of the lidar measurements?

HRRR hourly output is an instantaneous snapshot, not a temporal average. Accordingly, we expect 10-minute-averaged LiDAR data to compare better with HRRR than 30-minute averages. HRRR's horizontal resolution is 3 km, and the mean separation between the LiDAR buoy and the HRRR extraction point is ~2.5 km at Morro Bay and ~1.4 km at Humboldt. Although we could average over neighboring HRRR grid cells, the already fine 3-km resolution suggests such spatial averaging would have little impact on our results.

#8: Figure 3: The following comment is also connected to my impression that the manuscript could benefit from improving the clarity of its objectives. Is HRRR evaluated

with respect to its potential for resource assessment or wind power forecasting? Is the PDF of the wind speed the best quantity to assess the performance of a tool used for forecasting the wind speed? Even if the PDFs of the model and the measurements looked perfectly the same, this would not necessarily mean that it is well suited for the purpose of wind power forecasting. For that purpose other parameters like the absolute bias would be more relevant. Thus, if the objective were evaluating HRRR for the purpose of wind power forecasting I would recommend to not starting the evaluation with presenting the PDFs.

Following your suggestions in Comments #1, #2, and #4, we have clarified the objective of this manuscript to focus on evaluating the HRRR model for its skill in representing rotor-layer wind conditions. We have also replaced the term “wind resource assessment” with “rotor-layer wind speed assessment” throughout the text. Therefore, we would like to retain the PDFs in their current form, as they provide a useful overview of the rotor-layer wind characteristics.

#9: Line 172: “This suggests that HRRR model overestimates wind shear at Humboldt Bay location compared to the observations” The authors derive this statement from the PDFs of the wind speeds at different heights. I’m wondering why the authors do not directly investigate and present the wind shear itself before they conclude on it. I think this would strengthen the statements made on the wind shear.

Thank you for this valuable comment. Table 2 in the manuscript summarizes the shear exponent metric used to quantify the wind shear strength directly. From this analysis, we confirm that the shear is stronger at Humboldt and under clear-sky conditions. However, the shear exponent does not indicate that HRRR overestimates the shear at Humboldt, as was inferred from the PDFs in Figure 4. We believe this apparent difference arises from the distinct sampling approaches—PDFs emphasize higher wind-speed regimes, while the shear exponent is based on time-averaged wind speeds. We have updated the text in Section 3.2 to explicitly reference Table 2 and clarify that the discussion of wind-shear bias is based on the direct shear-exponent calculations rather than solely on the PDF comparisons. The revised sentence is provided below:

In Fig. 4, we repeated the analysis shown in Fig. 3 for Humboldt Bay location. At Humboldt, the lidar buoy measurements of wind speed in the rotor layer shows a more spread-out distribution spanning 0 – 20 m s⁻¹, while HRRR reported winds range from 0 to 30 m s⁻¹. The HRRR model simulates a peak in the PDF (Figure 4a) that shifts toward higher speeds with increasing heights, unlike what is observed (Figure 4d). At Humboldt, strong vertical shear is present regardless of cloud conditions, as the increase in wind speed with height is evident in both datasets, but HRRR model consistently overestimates this increase. During clear-sky conditions, the wind speed

tends to be stronger compared to cloudy conditions, with peaks around 10 – 18 m s⁻¹. Collectively the figure suggests that at Humboldt Bay wind shear is forcing the turbulence during both cloudy and clear-sky conditions, with HRRR model overestimating the winds and the wind shear. However, when the shear exponent is computed directly from the mean wind speeds (Table 2), HRRR slightly underestimates the average shear compared to the buoy data. This could be because that PDFs are dominated by high-wind events, which amplify apparent vertical gradients, while the mean shear exponent integrates over all wind-speed regimes and may therefore show weaker shear on average.

#10: Line 253-254: “As stated in Optis et al. (2016) and the references therein, similarity-based wind speed profile models produce a large bias under strongly stratified boundary layer.” Is it the aim here to provide an explanation for the bias in strongly stratified boundary layers observed by the authors? In that case the link between HRRR and similarity theory should be made clearer.

Thank you for this helpful comment. Our intention was indeed to suggest a possible explanation for the HRRR wind-speed bias under strongly stratified boundary layers. HRRR, like most numerical weather prediction models, uses parameterizations for surface-layer turbulence that are based on Monin–Obukhov similarity theory (MOST). As discussed in Optis et al. (2016), such similarity-based formulations can become inaccurate under stable or strongly stratified conditions, where turbulence is weak and the assumptions of constant flux and stationarity break down. We have revised the text to explicitly state this link between HRRR’s parameterization framework and the potential source of bias and is shown below:

As discussed by Optis et al. (2016) and references therein, similarity-based wind-speed profile formulations derived from Monin–Obukhov similarity theory can produce substantial bias under strongly stratified boundary layers. Because the HRRR model employs surface-layer and turbulence parameterizations grounded in this framework, such limitations likely contribute to the under- or overestimation of wind speeds observed under stable, clear-sky conditions in our analysis.

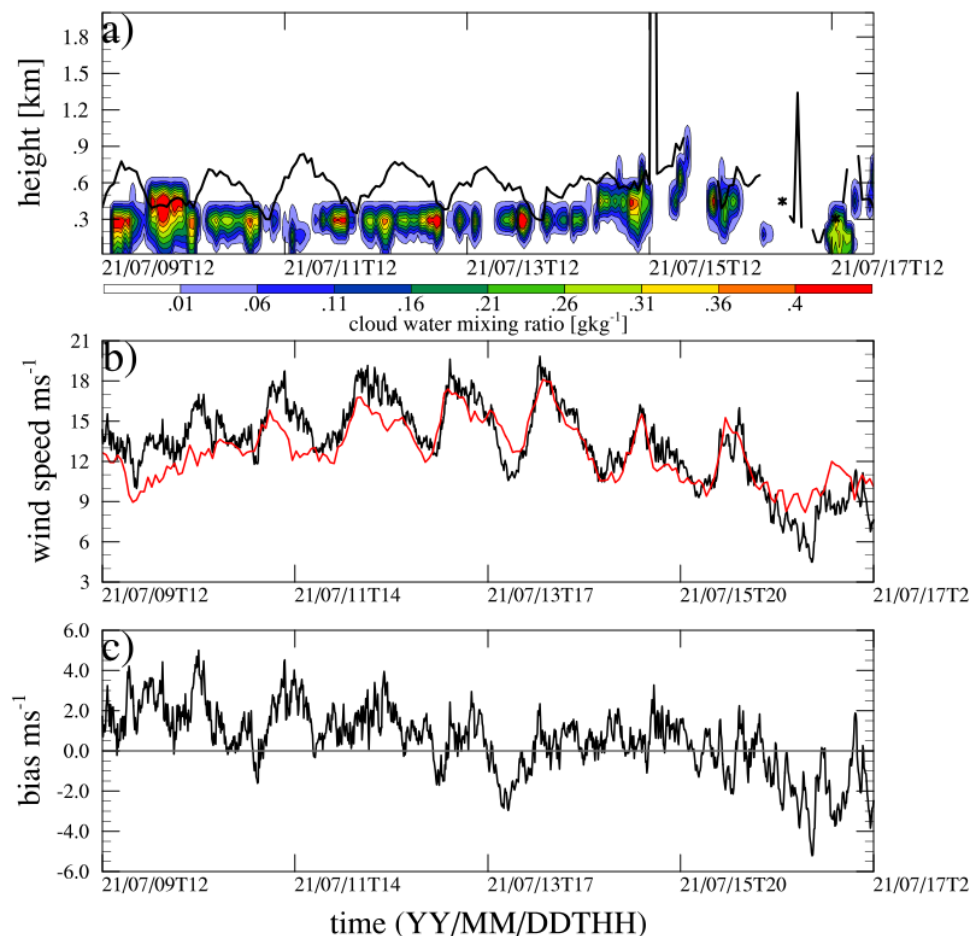
#11: Line 269: “Some cases had breaks in the cloud for less than 3 hours that got filled” I think the authors should elaborate on this a little bit further. For me the meaning of this statement was unclear. What was filled with what?

Thank you for your comment. We revised the sentence as provided below:

Some cases included brief breaks in cloud cover lasting less than 3 hours and were therefore treated as part of the same event.

#12: Figure 8b): Is there an explanation why the lidar data, although it is averaged over 10 minutes, is fluctuating quite strongly (and seemingly with some preferred frequency), while the model data is comparatively smooth?

Thank you for raising this question. We discovered that the jumps in the lidar data were caused by a timestamp error, which has been corrected in the revised manuscript and also is given below. The observed time series are not expected to appear perfectly smooth, as atmospheric turbulence naturally induces wind-speed fluctuations that the model does not fully capture. Therefore, it is reasonable that the HRRR results appear smoother than the observations, even at the 10-minute time scale.



#13: Line 317-319: "Therefore, better understanding the processes within the marine boundary layer bounded by marine stratocumulus clouds is important to better assess and predict wind resources for offshore wind farms in California." Is it necessary to limit this statement to offshore wind farms off the coast of California?

Thank you for your comment. We revised the sentence as given below:

Therefore, better understanding the processes within the marine boundary layer bounded by marine stratocumulus clouds is important to better assess and predict wind conditions in coastal regions dominated by these clouds

Minor comments:

#1: Line 20: Please change "... those fields are stronger ..." to "... those parameters have larger values ...".

Changed following your suggestion

#2: Line 40: Please change "... were made from a research flights" to "... were made from research flights".

Changed following your suggestion

#3: Line 46: Please change "Staring October 2020" to "Starting in October 2020".

Changed following your suggestion

#4: Line 59: Please change "model wind bias in relation to of various" to "model wind bias in relation to".

Changed following your suggestion

#5: Line 62: Please delete "at the Humboldt location".

Changed following your suggestion

#6: Line 71: Please change "from from 40" to "from 40".

Changed following your suggestion

#7: Line 75: I think the sentence "Temporal cloud mask is estimated to be cloudy" should be rephrased. What is a temporal cloud mask? This term needs to be better introduced.

Following your suggestion, I modified the sentence as shown below:

The temporal cloud mask, which provides a timeseries of sky conditions, was derived by quantifying the deviation between the observed and modelled clear-sky collar radiation. When the measured radiation was more than 10% lower than the modeled

clear-sky value, the condition was classified as cloudy. Otherwise, it was designated as cloud-free (Krishnamurthy et al., 2023).

#8: Section 2.3: The description of HRRR should be extended. In the current text there is no information on the data assimilation process that is used in the model. It would be good to add references to previous evaluation studies that support that HRRR provide forecast with high-fidelity as stated in the description by the authors.

Thank you for your suggestion. We extended HRRR description (section 2.3) by including initial and boundary data for the model as well as the data assimilation method. We also provide references to previous HRRR evaluation studies such as Liu et al. 2005; Pichugina et al. 2020 to the text and the change is provided in the response to major comment #3. Below is the addition in section 2.3.

HRRR's initial conditions are generated from the parent Rapid Refresh (RAP) model one hour prior to forecast initialization. RAP forecast is advanced forward, assimilating the most recent observations to provide a dynamically consistent state for HRRR initialization. Lateral boundary conditions for HRRR are supplied by RAP model at three-hour intervals. HRRR employs a hybrid ensemble-variational (EnVar) data-assimilation system that integrates a 36-member ensemble to represent flow-dependent background errors. The assimilation cycle updates every hour and incorporates a wide range of conventional, satellite, and radar observation. Within each cycle, radar reflectivity and radial-velocity data are assimilated every 15 minutes to improve the representation of cloud and convective structures. Surface and soil states are updated using short-term forecasts from the HRRR Data Assimilation System (HRRRDAS), which maintains temporal continuity between analysis cycles. This framework is designed to minimize spin-up errors and improve the depiction of boundary-layer and mesoscale processes critical to wind-forecast applications.

#9: Line 92-94: The corresponding sentence should be revised to avoid a possible misunderstanding. "... are close to the heights of the lidar measurement, which is at 40, 80, 160 and 240 meters" should therefore be changed to "... are close to the heights of 40, 80, 160 and 240 m at which data from lidar measurements is available"

Changed following your suggestion

#10: Line 116-118: "In December 2020, a large wave event at Humboldt buoy location results in the power outage and lead to a large data gap. The machines on the buoy were back online on May 25th, 2021, at Humboldt location." This information had been provided in the manuscript before. Therefore, there is a redundancy that should be removed.

Removed those sentences following your suggestion.

#11: Line 138, line 142: References are made to a figure S1, however, there is no figure S1 in the manuscript. Please correct these references.

Figure S1 refers to the figure in supplement material, which can be found here:
<https://wes.copernicus.org/preprints/wes-2025-108/wes-2025-108-supplement.pdf>

#12: Figure 3: The following comment is also connected to my impression that the manuscript could benefit from improving the clarity of its objectives. Is HRRR evaluated with respect to its potential for resource assessment or wind power forecasting? Is the PDF of the wind speed the best quantity to assess the performance of a tool used for forecasting the wind speed? Even if the PDFs of the model and the measurements looked perfectly the same, this would not necessarily mean that it is well suited for the wind power forecast. Other parameters like e.g. the absolute bias would be more relevant. Thus, if the objective were evaluating HRRR for the purpose of wind power forecasting I would suggest not starting with the presentation of the PDFs of wind speed.

Thank you for your suggestion. Following your earlier comments, we have revised and clarified the objective of the study to focus on evaluating the HRRR model for its representation of rotor-layer wind characteristics in the area of interest. We have decided to retain the PDFs in their current position, as they effectively introduce the discussion of wind characteristics.

#13: Line 260: Please change "As a results ..." to "As a result".

Changed following your suggestion

#14: Line 343: Please change "... wind bias ..." to "wind speed bias".

Changed following your suggestion