Response Letter

Ref.: *AMT-2021-279* *Atmospheric Measurement Techniques*

Title: *Wind Speed and Direction Estimation from Wave Spectra using Deep Learning*

Author: Haoyu Jiang

Dear Prof. Ad Stoffelen,

Thank you again for concerning the manuscript in *Atmospheric Measurement Techniques*. I appreciate you and the reviewers for your earnest work. The comments from the reviewers are very helpful, and the paper has been revised carefully according to some of them. The point-by-point response to the reviewers' comments are attached, as well as a tracked-changes version of the manuscript. For some comments I do not completely agree on, the explanation is also given in the point-by-point response.

I hope that this version of the manuscript is acceptable for publication. If you have any questions, please feel free to contact me. I appreciate your support very much.

Best regards,

Yours sincerely,
Haoyu Jiang

Responses to Reviewer 1:

Though the author replied the suggestions, the main issues are not resolved in the following aspects, which makes the manuscript not suitable for publication:

The author would like to thank the reviewer again for his helpful discussion. However, the author does not completely agree with the comments from the reviewer. The explanation has been given in the following point-by-point response.

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1) The samples (buoys) are not enough in space distributions to guarantee the robustness of the relationship established by the DNN.

The space distribution of the buoys is not always a key factor for the robustness of an empirical model. Otherwise, all remote sensing empirical algorithms or calibration relationships established using these NDBC buoys will lack robustness. Figure 1 in the manuscript has shown that the model is applicable for almost all buoy locations except for the region with strong tidal current (Station 46087 and 46088 at the Strait of Juan de Fuca, and even for them, the RMSE of U10 is still less than 2 m/s). If the spatial distribution of the buoys is insufficient to guarantee the robustness of the relationship established by the DNN, there will be some locations with much larger errors.

To further test the robustness of the DNN model in different locations, the author also tried to divide the training set and validation set according to the buoys' location. The data from buoys 45001-51101 (53 buoys) were selected as the training set and the buoys 41002-44066 (48 buoys) were selected as the validation set. The locations, wind-wave climate, and other environmental properties are significantly different for the two sets, because none of the buoys in the validation set is in the same basin as the buoys in the training set. However, the results remain quite good as shown in the figure below (Figure R1). This again shows that the resulting model can adapt the condition for different regions, which confirms that the robustness of the DNN model can be guaranteed.

Some of the above explanation has been added to the revised manuscript to make this point clearer for readers.

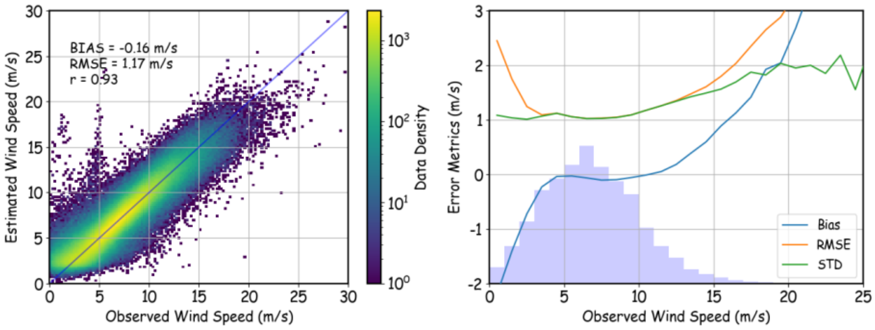

Figure R1. *The same as Figure 2b and 2c in the manuscript, but the training and validation sets are different. In this figure, the data from buoys 45001-51101 are used as the training set and the buoys 41002-44066 are used as the validation set, and the comparison is only conducted in the validation set.*

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2) The time delay between wind and buoy measured wave parameters is specifically discussed enough from neither the DNN model nor the physical principles.

Although it is assumed that wind input, breaking dissipation, and wave-wave interaction are in equilibrium in the high-frequency part of the wave spectrum in Phillips (1985), a time delay should be within expectance because the high-frequency portion of the wave spectrum integrates preceding wind conditions. Therefore, it is natural to consider whether the buoy wave spectrum can better represent the local wind information some time ago. Using the data, the author proved that it is best to use the wave spectra to estimate wind speeds and directions ~40 minutes ago. The author understands that the "best" time lags can be different for different parts of the wave spectrum (Jiang and Mu 2019), and different past wind can correspond to different "best" time lags. However, the aim here is only to find a time lag to best link the local wind and waves from a statistical point of view, to make the algorithm be able to form a time series with a fixed time interval between estimations.

Reference:

Jiang, H., and Mu, L. (2019). Wave Climate from Spectra and Its Connections with Local and Remote Wind Climate. *Jouranl of Physical Oceanography*, 49, 543-559.

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3) The analogy with scattermeter data against buoys is not reasonable.

The author needs to re-emphasize that this research did not match the buoy wind with the scatterometer products. This work only compared the wave-estimated wind and the anemometer-measured wind. However, as the author mentioned before, wind measured by buoys is time average that is comparable with remote sensing (spatial average) wind product based on the equivalence of space and time variability. Such comparison is common practice in the validation of wind products of remote sensors, including those from scatterometers and altimeters. Many related publications are doing so.

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4) Lack of application prospects.

Compact wave buoys are increasingly widely used in global wave observations. For example, more than 2,000 Spotter buoys have been deployed in global oceans by Sofar Ocean Technologies (locations of them can be viewed at http://weather.sofarocean.com/). Due to the compact size of such buoys, they cannot equip anemometers so that wind information is not directly available from them. The model can estimate the wind speed and direction with an accuracy of ~1.1 m/s from only wave spectrum information. Therefore, with compact wave buoys becoming increasingly widely used in observing wave parameters, they can also become a new good-quality data source of sea surface wind after applying the model presented in this study. Besides, the manuscript also showed that this model can also be used in the quality control of the wind and wave data from coastal meteorological buoys. Therefore, it is unreasonable to state that this model lacks application prospects.

Responses to Reviewer2:

The authors have addressed the reviewer comments with sufficient attention and improved the manuscript. It is understood that a neural network approach to mapping between the buoy-derived spectral coefficients and the buoy winds is intended to provide a more optimized wind inversion, and not to expressly, or readily infer the geophysical basis for the mapping.

The results demonstrate a significant improvement to that of Voermans et al. (2020). This is not unexpected given the approach that uses all freq. band data and Fourier components for 101 buoys over four year, and given the likely large size of the resulting DNN. As the authors note in review discussions, this likely indicates the limits for how well the NDBC discus buoy data can estimate wind speed and direction.

The author would like to thank the reviewer again for his/her encouragement and valuable comments. These comments are very helpful for the improvement of the manuscript, and revisions have been made according to them. A revised version of the manuscript with changes highlighted is also attached. The author hopes that the revised manuscript is acceptable for the reviewer.

Indeed, it is difficult to give the underlying physics of a DNN model especially when its input is a high-dimensional vector. Fortunately, the physics background of estimating wind information from wave spectra has been discussed in Voermans et al. (2020) so that this work can focus on the wind-estimation model itself. In the author’s opinion, the DNN (or other AI methods) is the best suited for the problem that we know there are some causal relationships between inputs and outputs, but the accurate analytical physical model is too sophisticated to establish. That is why the author thinks that this wind-inversion problem is a good (this seems not to be humble, but the author does think so) application of the DNN and this work deserves to be published in a relatively technical journal.

A remaining issue with the revised paper I see is the following...
The text near line 235-238 in the Discussion section should more clearly explain and support their conclusion that long wave modulation of capillary waves has little impact on wind estimation using buoy spectra. Their citations refer to wind remote sensing. But

this is a buoy wave vs. anemometer wind paper. Not a remote sensing paper.

Their assertion is off the mark. First, the NDBC wave buoys only accurately measure gravity waves up to f=0.4 Hz at best. These waves are much longer than those used in remote sensing and the buoys certainly do not measure capillary waves. Thus this suppostion that the DNN sensitivity tests illustrate that long wave-short wave interaction impacts on the model are absent is simply wrong. Rather - their sensitivity tests seem to imply that there is limited additional wind-correlated information carried in frequencies lower than 0.1-25 Hz when considering the overall training and validation datasets and this DNN approach. This is as much as one can conclude.

This seems to be an expression problem and the reviewer seems to misunderstand the author's conclusion (it is the fault of the author, sorry for that). The author did not mean that long wave modulation of *capillary waves* has little impact on wind estimation using buoy spectra (the buoy does not provide capillary wave information so that this cannot be concluded). What the author wants to express is that the long wave modulation of *wind-sea* (i.e., waves with frequencies within 0.1-0.5 Hz which are all gravity waves instead of capillary waves) has little impact on wind estimation using buoy spectra. By the way, the author also did not say that the long wave does not modulate wind-seas. It is just that this process has little impact on wind estimation.

The rationale of this method has some similarities to wind remote sensing: both of them are using wave information to indirectly retrieve the wind information. The difference is that remote sensing uses capillary waves while buoys use gravity waves to estimate the wind. So that some processes can be linked in the two methods. The impact long wave modulation of *capillary waves* has been proved to be significant for remote sensing. Then, one will wonder whether the impact long wave modulation of high-frequency gravity waves measured by buoys is significant for wave-spectrum-based wind estimation. The answer from the author's result is a NO. Because this is probably the second work (the first is Voermans et al. (2020)) of using gravity waves to retrieve wind speed, it is a bit difficult to find other related references.

This part has been revised to make the expression more clear to avoid the potential misunderstanding: "Previous studies of wind remote sensing showed that the modulation of swells on *capillary waves* [here we use italic to stress the difference] has some impacts on the wind speed retrievals (e.g., Stopa et al. 2016, Li et al. 2018, Jiang et al. 2020). Long swells also modulate short wind-seas (waves with relatively high frequencies measured by buoys, they are gravity waves instead of capillary waves). If this modulation process significantly impacts the buoy wind-estimation model, removing the long swell information will negatively impact the model accuracy. However, according to the results in Figure 5, the swell's modulation on wind-seas has little impact on wind estimation using buoy wave spectra."

Minor comments:

I don't believe the authors have yet clarified inside the new draft that the problematic Jaun de Fuca buoy data is included in the model training and that they they have confirmed that these (and other problematic coastal buoys in their set) do not significantly or adversely affect model performance.

This has been clarified in the new version of the manuscript (at Line 203): "If the aforementioned problematic data are excluded from the training and validation dataset (they are included in the results in Figures 1~4), the overall performance of the model will not be significantly improved (the overall RMSE only reduced by 0.02 m/s), because the number of samples for these corrupt data is very small compared to the overall sample size. However, the U10 RMSEs will be less than 1.5 m/s for all buoys at different locations."

Near lines 123-125 there should be some relevant supporting citation and more precise quantification of the expected growth rate of the 0.2-0.4 Hz part of the wave spectrum and relevant time scales. "short period to grow" is imprecise. Gravity-capillary waves are not instant equilibrium with the wind. And certainly the wave buoy measurements contain information on the wind before. This is not a case of "might". Please provide appropriate citation or evidence.

The growth of the high-frequency wave spectrum is mainly controlled by wind input, breaking dissipation, and wave-wave interaction, and this process responds rapidly to the local wind. In the open ocean, it is often assumed that dE/dt = 0 in the equilibrium range (*E* is the wave spectrum, *t* is the time) (Phillips 1985, which gives the shape of the spectrum tail as a function of U10). However, the author failed to find the supporting citation for evaluating the relevant time scales of wave growth. That is why the author used such an imprecise way of describing this. Another problem here is that the sensitivity test in Section 4 has not been conducted yet in this part of the text, thus, the readers have not known which part of the wave spectrum is important for the estimation of wind information yet. That is why the author did not try to discuss the growth rate of the wave spectrum here.

As pointed out by the reviewer, the expression, "gravity waves (wind-sea) need a short period to grow", is imprecise (and even wrong). Considering the suggestion of the reviewer, the author changed the expression in this part to: "Different from the capillary waves with very high frequencies always in instant equilibrium with the local wind, the growth of gravity waves is time-dependent. Besides the current wind information, the wave spectrum measured by a buoy at a given location and time also contains remote and past wind information (Jiang and Mu 2019), because the wave spectrum is, to some degree, integrated winds. Therefore, *it is possible that the buoy wave spectrum can better represent the local wind information some time ago*. Based on this idea, the wave spectra were also collocated with past wind measurements using different time lags." Although the "some time ago" in the italic sentence is still imprecise, this sentence does not contain the physics of wave growth and should be a reasonable expression.

Reference:

Phillips, O. M. (1985). Spectral and statistical properties of the equilibrium range in wind-generated gravity waves. *Journal of Fluid Mechanics*, 156, 505–531.

Jiang, H., and Mu, L. (2019). Wave Climate from Spectra and Its Connections with Local and Remote Wind Climate. *Jouranl of Physical Oceanography*, 49, 543-559.

Editorially, it seems peculiar to single out the two Juan de Fuca buoy datasets and ocean currents as a key issue to discuss or address in the concluding remarks, lines 251-258. Perhaps the overall issue of translating these DNN model results from the large NDBC buoys and their data to the emerging drifter wave buoy program should be introduced first. This is the third paragraph of the conclusion. Maybe switch these paragraphs and revise.

According to the reviewer's suggestion, the author slightly changed the logic of writing the concluding remarks. Now the concluding remarks are divided into four paragraphs.

The first paragraph is a short summary: “Ocean wave spectra can be used to sea surface winds. Here, we trained two DNNs that can estimate U10 and wind directions ~40 minutes ago from high-frequency wave spectra. The overall accuracy of the wind-estimation DNN models is comparable with the state-of-the-art scatterometers under moderate wind speeds. The two models can also be used as a quality control tool for wind and wave measurements from meteorological buoys.”

The second paragraph is the overall issue of translating these DNN model results from the large NDBC buoys and their data to the emerging drifter wave buoy program, as suggested by the reviewer: “The DNNs were trained using a large amount of data from only NDBC buoys but not compact wave buoys. However, applying the two models directly to compact wave buoy data (after interpolating the spectra from compact buoys into the frequency bins of NDBC buoys) will not result in significantly lower accuracy. This is because the DNN will automatically select the NDBC wave spectra in the frequency with relatively high accuracy, and the accuracy of measured spectra from compact wave buoys is usually higher.”

The third paragraph is to briefly review the problems of the models using NDBC buoys and to predict that these problems can be partly solved by compact wave drifters: “For the wave data from NDBC buoys, the performance of the U10 DNN is significantly biased when U10 is too high or too low, and the performance of the wind direction DNN becomes worse with the decrease of U10. Also, the accuracy of both models decreases when the surface currents are strong. We believe these shortcomings can be partly solved by compact wave drifters, resulting in better accuracy in estimating near-real-time wind properties. First, a smaller buoy size can resolve high-frequency wave spectra more accurately, which is helpful for wind estimation. Second, in the condition of strong wind or current, the moving velocity of the wave drifter is usually similar to that of the surface current, making the wavenumber and frequency spectra follow dispersion relation again in the buoy reference system. This can compensate for some of the errors induced by strong surface currents or wind-induced drifts. Therefore, significantly better accuracy can be achieved by training new DNN models with the spectral data (maybe also the drifting velocity data) from compact buoys using collocated wind and wave measurements. Such measurements can be obtained by placing some compact buoys near meteorological buoys or simply using the scatterometer or re-analysis wind as the training target.”

The last paragraph is the discussion about the prospect, which is still the last part of the original manuscript: “Finally, we hope to point out that such DNN models need not to be trained from the beginning using a large amount of data. The DNN models presented in this paper can serve as pre-trained models which will significantly reduce the complexity of training the new models. With the compact wave buoys becoming increasingly widely used in observing wave parameters, their global network can be a new good-quality data source for both waves and wind after applying these models."

Given that the concluding remarks suggest that their models could be built upon for future work, it would be useful to provide some access to the code and training sets that were used to develop them, or to provide some a subroutine or lookup table to implement their models. Is this included with the paper?

This is a very good suggestion. Regarding the data of training and validation sets, all the buoy data used in this study are available from the National Centers for Environmental Information (https://www.ncei.noaa.gov/data/oceans/ndbc/cmanwx/). This has been clarified in the acknowledgement section. As the total size of the five years' buoy wind and wave data from ~100 buoys is about two GB, there seems to be no need to upload them again. Meanwhile, according to the suggestion of the reviewer, the established DNN models were uploaded together with the revised version of the manuscript as supplement materials, and the Python code (with the annotation) of using the two DNNs was also attached. With these files, the readers/users can easily implement the models.

We also added the following statement to the acknowledge part: "The two established wind-estimation DNN models are available as Python .plk files in the supplement materials where the corresponding example (as Python code) of implementing the two models are also available."

There are numerous grammatical issues with the revision. These are minor and can be found near lines:
13-15 - this sentence should convey something like "because the high-frequency portion of the wave spectrum integrates preceding wind conditions"
23 73 77 86-89 124-125 129 169 200-201-provide citation 248

The author would like to thank the reviewer again for pointing out these minor issues. The manuscript has been revised accordingly. For the citation problem in L200-201, "The surface currents are generally larger in coastal regions (tides) and westerlies (wind drifts) than in low-latitude open oceans", this point has become "common sense" of the oceanography to some extent and can be found in a lot of textbooks (in the sections of tides and circulation). Therefore, the author failed to find a suitable citation here. It will be nice if the reviewer can recommend one or two.
