

Atmos. Meas. Tech. Discuss., referee comment RC3  
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## Comment on amt-2021-279

Anonymous Referee #3

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Referee comment on "Wind speed and direction estimation from wave spectra using deep learning" by Haoyu Jiang, Atmos. Meas. Tech. Discuss.,  
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This study is clearly presented and well written. The objective is to improve upon the recent work of Voerman et al. (2020) that attempted to invert near-surface wind speed and wind direction from ocean wave buoy datasets provided by the NDBC network of coastal and offshore buoys. That previous study provided a thorough review of wind-wave interaction as it pertains to buoy measurements and this inversion. The present study bypasses the geophysical basis and instead focuses on a sort of brute force neural network (DNN) approach to the wind estimation task using the NDBC data archive of five freq. dependent Fourier coefficients that are used to approximate the directional gravity wave spectrum from long to intermediate scale surface waves (both swell and wind sea). The study appears to use data from the entire buoy station network to develop separate wind speed and direction algorithms, provides detail on the network training and several relevant DNN adjustments during the training process, and then results that show some promising capability to provide wave-buoy derived wind estimate that agree better with the buoys' anemometer measurements. They also find that the winds derived in this manner appear to lag behind the actual surface winds in time by 30-60 minutes - and thus their final algorithm estimates not the wind at the present time, but actually the wind that occurred one hour before. They also find, as did the recent Voermans et al. study, that their best algorithms still have limitations at lower and higher wind speeds where the wave information does not unambiguously relate to the wind.

Comments to the authors:

While this paper does show some potential for a neural network algorithm that takes the basic directional wave information provided by NDBC and outputs wind information, it does not appear to move things too far forward from the Voermans study they follow on from and the low and higher wind speed regime limitations that were highlighted in that study. What it does illustrate is that a DNN can improve on the semi-analytical approach used in the previous investigation.

The finding that there is an apparent delay between the wind speed and the wave-inferred wind speed is not physically inconsistent with Voermans et al. (2020) Figure 9g where the wind acceleration is related to model error residuals. However, there is an additional issue for the authors to consider first. The wave buoy measurements provided by NDBC have a center time that is 30 min past the top of the hour with data collected  $\pm 10$  min of that time. The authors do not clearly provide detail on the NDBC wind products they are using, but if that product is the stdmet product then the center time for that 8 min. avg wind estimate is at minute 46 (measurements made from 42-50). Thus there is an inherent 15 min offset with the hourly wave data leading the wind. This factor may also color why the previous wind measurement is more highly correlated with the wave-inferred winds. Finally, the NDBC network does contain a large number of continuous wind measurement buoys where winds are measured every 10 minutes. Thus the authors have the opportunity to investigate the actual lagged correlation between DNN wave-derived winds and the anemometer data with 10 min resolution and perhaps at varying wind speeds.

Regardless, this issue points out that using a series of DNN models to sort this out is an indirect and poorly-posed reverse engineering approach to infer the growth or dissipation rate of wind waves, as well as an illustration of the fundamental limitation in the use of surface waves to provide accurate wind measurements under a full range of wind forcing and sea states discussed in Voermans et al. (2020).

A significant concern related to this time delay is the need to explain the potential implications of their DNN-derived estimates for users such as forecasters. The final DNN models are tuned to give wind speed and direction from the hour before. Thus I believe the first sentence of the Concluding Remarks should clarify this point. I believe the authors should consider a revisit of this product. Perhaps they should provide statistics and models for two wind options, the nearest time wind and the previous hour winds?

The model sensitivity tests in the discussion section are an ad hoc revisit of the more in-depth work of Voermans et al. (2020) and previous work (e.g. Jusko et al., J. Phys Ocean. 1995). But simply withholding part of the frequency spectrum from the inputs does not provide new results. It confirms, as the authors note (lines 205-210), what has already been shown in terms of the importance of the higher frequency portion of the spectrum closer to the wind sea peak frequency and the tail of the spectrum. The authors appear to perform this test in the same way for all wind speeds and conditions and perform the RMSE assessments similarly for all winds. This is a course sensitivity test. Perhaps something more creative could be done to investigate the potential to modify inputs with a goal to improve performance at low and high wind speeds?

General comments:

More detailed information on the specific wind and wave buoy products that they used in training, their data filtering and quality control, and references describing the approach that NDBC uses to extract the directional wave Fourier coefficients should be provided.

Given what is observed in terms of data quality in the section surrounding Figure 3, is there any concern that such corrupt data are present in the training and/or validation datasets? Moreover, as noted in the next paragraph, it would seem to be obvious that the algorithm training set should not include buoys where there is strong known wave/current interaction such as 46087 and 46088. This would be a highly unusual case of wind-wave-current interactions that would not be desired in a general-purpose wind algorithm that only uses the 5 Fourier coefficients and no surface current data as inputs.

Similarly, was there any consideration given to differentiating between coastal, offshore, and/or differing wind-wave climate buoys in the model input training sets to improve performance, for example at low or high wind speeds.

The authors seem to be interested to develop a wind measurement system that competes with a satellite scatterometer or altimeter, but this project is inherently dealing with in an in situ platform. Is not the goal to develop an in situ system that has precision and accuracy metrics similar to those of the 10 min averaged wind anemometers used at sea?