The submitted manuscript describes a new parameterization of the impact of wind farms on the wind speed and TKE within the boundary layer, whose novelty is that it is adapted to the characteristics of floating offshore turbines. As wind farms move to deeper waters, necessitating this design, such a parameterization will certainly be of great scientific value.

There is a lot of innovative analysis here, such as the consideration of impacts on wave heights, inertial effects from the piles, and the adaptation of a SWAN vegetation model to account for these effects. The machine learning approach is also interesting. However, overall I find the manuscript leaves out too many details of the theoretical justification, is unclear in presentation and organization (especially of the experimental design), and seems to provide insufficient evidence to justify its conclusions. So I can only recommend acceptance after major revisions have been performed.

Responses to the comments of Reviewer #1:

We sincerely thank the reviewer for the suggestions and comments that help us improve the quality of our manuscripts.

Comment 1: Line 27: You mention explicit and implicit methods here, and then state that explicit methods are superior, but you never describe what an implicit method is – you should put in either a brief description or remove the first sentence of the paragraph.

Response: Thank you for your suggestion. We briefly describe the implicit method for parameterized wind farms in line 31 of the revised manuscript.

Comment 2: Line 43: 'This suggests that the current wind farm parameterization is not suitable for floating wind farms because it does not account for the change in roughness length caused by large floating platforms.' Do you have additional evidence that existing parameterizations are deficient? Why would this deficiency only affect floating wind farms, and not also offshore but fixed wind farms?

Response: Thank you for your suggestion. As described in lines 34 to 36 of the manuscript, almost all current studies on wind farm parameterization focus only on sub-grid effects of wind turbines. Additional evidence, in our view, can still come from the equation derived in Fitch et al. (2012),

$$\frac{\partial V_{ijk}}{\partial t} = \frac{-\frac{1}{2} N_{ij} C_T V_{ijk}^2 A_{ijk}}{z_{k+1} - z_k}$$

From the above equation, it can be seen that the momentum tendency term is only related to the number of turbines in the grid, the thrust coefficient, the inflow wind speed, the rotor area, and the vertical resolution. If the surface roughness of the turbine location is changed, the results obtained from the above equation do not change.

We also believe that the new wind farm parameterization proposed in this study can also be applied to fixed offshore wind farms in a special case (large-diameter monopile foundation of offshore wind turbine). However, the diameter of the pile foundation of most of the current fixed wind turbines is only in the range of 1 to 4 meters (Figure R1). The weakening effect of such small diameter piles on the significant wave height is not obvious.

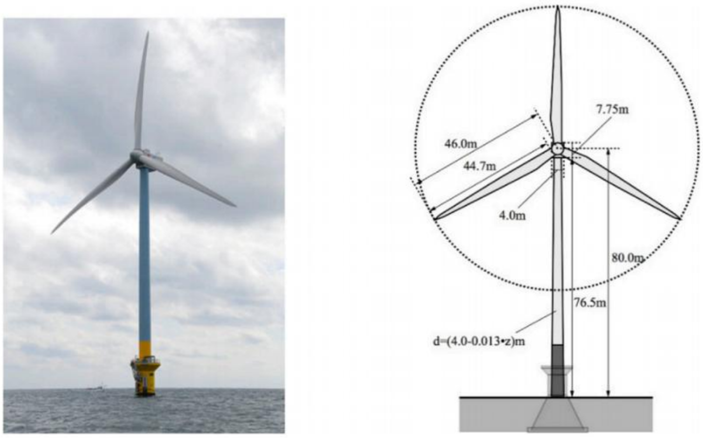

Figure R1 Wind turbine at Choshi demonstration site and its dimensions (Ishihara and Qian, 2018).

Comment 3: Lines 80 and following: Maybe include more brief descriptions of where all these equations and values are coming from, and why?

Response: Thank you for your suggestion. We apologize for missing some details. We have added relevant content in Section 2.

Comment 4: Line 102: I don't follow the Rayleigh equations. Why would H^3 equal its integral over p(H) dH?

Response: Thank you for your suggestion. Sorry for the confusion, we made changes in lines 123 to 130 of the revised manuscript. It is only when calculating the average energy dissipation that H^3 can be replaced by $\int_0^\infty H^3 p(H) dH$ in equation (11).

Comment 5: Line 160: Maybe this works for the South China Sea, but a range of water depth from only 53 m to 98 m leaves out other potential deep water applications, such as off the U.S. West Coast, where the depth could be hundreds of meters (though at some point additional depth won't matter, I suppose).

Response: This is a good comment, and we've taken it into account in our experimental design. In fact, using the high-resolution SWAN, we can set arbitrary water depths for ideal SWAN simulations. However, we limited the water depth to 100 meters for two reasons.

1) Considering the practical situation. Offshore floating wind turbines are still in their infancy, and it is challenging to achieve large-scale development of floating wind energy in areas with water depths of less than 100 meters.

2) The problem of excessive data volume. In this research, we use a data-driven

machine learning model and implement it in WRF. However, this also leads to the consumption of more computational resources. For other sea areas, we need to retrain our machine learning model. In this study, we simply chose a water depth in the range of 53 to 98 meters to save computational resources. How the effect of the floating wind turbine piles on the waves would vary with water depth (>100 meters) is also an interesting topic.

Comment 6: Line 162: Your machine learning output variable is SWH, but roughness can also be a function of wavelength and wave age – would it be possible to include these parameters as well?

Response: This is a good comment. In other roughness parameterization schemes, roughness is also correlated with wave age. When WRF is coupled to SWAN, the user can choose one of the following three parameterization schemes.

1. COARE_TAYLOR_YELLAND

$$z_0 = max(1200 * H * (\frac{H}{L + 0.001})^{4.5} + 0.11 * \frac{v}{u_* + 0.001}, 1.59 \times 10^{-5})$$

2. COARE_OOST

$$c = max(\frac{L}{P + 0.001}, 0.1)$$

$$z_0 = max(\frac{25}{\pi} * L * min(\frac{u_*}{c}, 0.1)^{4.5} + 0.11 * \frac{v}{u_* + 0.001}, 1.59 \times 10^{-5})$$

3. DRENNAN

$$c = max(\frac{L}{P + 0.001}, 0.1)$$

$$z_0 = max(3.35 * H * min(\frac{u_*}{c}, 0.1)^{3.4} + 0.11 * \frac{v}{u_* + 0.001}, 1.59 \times 10^{-5})$$

where L is peak wave length, and P is peak wave period. The effect of the turbine piles on the wave energy is only in the effective wave height and has no effect on L and P. Therefore, the machine learning model only needs to output the significant wave height as a sole variable.

Comment 7: Line 164: For non-specialists, could you include at least a little more description of what these ML methods mean? What is 'Matern 5/2 kernel'? Any explanation why its fit is so much closer than for the other methods?

Response: Thank you for your suggestion. We apologize for missing some details. We describe each category of machine learning models in more detail in the Appendix.

The 'Maternal 5/2 kernel' is a kernel (covariance) function in Gaussian process regression (GPR).

Most GPR models have good performance, probably because the advantages of GPR is mainly in dealing with nonlinear and small sample data.

Comment 8: Line 184: My biggest issue might be with the justification of (17). Why *does a change in surface momentum flux get translated to a change in mean wind kinetic energy for an elevated layer? Aren't changes in wind speed related to vertical gradients of momentum fluxes? And changes in kinetic energy related to the product of momentum flux times vertical gradients of mean winds? Why would the impact of surface roughness be omitted above 100 m if it can exert a drag on the whole boundary layer?*

Response: This is a good comment. We will first discuss with you about wind speed (kinetic energy) and momentum flux. According to the Monin-Obukhov similarity theory (Monin, 1954), assuming that the momentum flux in the near-surface layer of the atmosphere is constant, the vertical gradient of the wind speed in this layer can be expressed by the following equation,

$$\frac{kz}{u_*}\frac{du}{dz} = \varphi(\frac{z}{L})$$

$$\frac{kz}{u_*}\frac{du^2}{dz} = 2u \cdot \varphi(\frac{z}{L})$$

You are certainly right about the relationship between wind speed (kinetic energy) and momentum flux. But the reason we did not use the above equation in our study is that the algorithm for φ in WRF is too complicated. We decided to use instead the equation derived by Fitch et al. (2012), which includes the unaccounted for wind stress (friction) that does the work.

And in this study, we believe that the reason the new parameterization only applies to heights below 100 m (Figure R2) is because this is approximately the maximum height of the near-surface layer (constant flux layer: momentum flux, heat flux, etc. vary less with height).

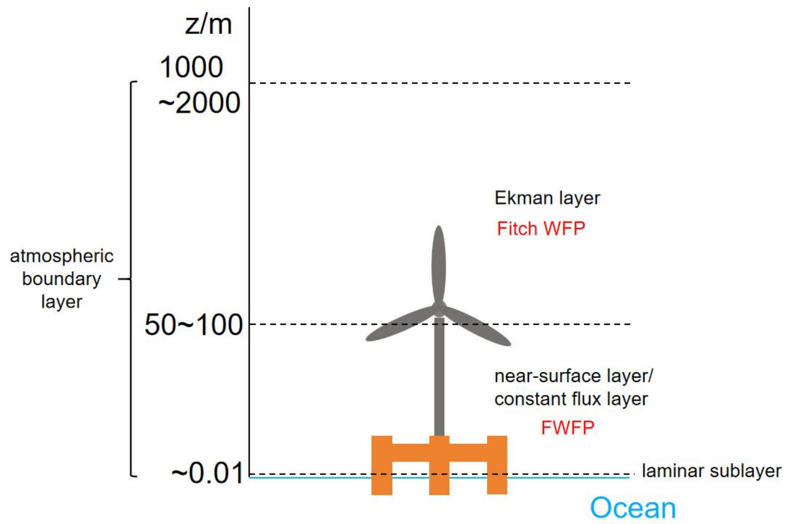

Figure R2 Composition of the atmospheric boundary layer.

Comment 9: Line 230: You validate SWH with satellite data – are there any in situ measurements of waves available?

Response: This is a good comment. We have had difficulty obtaining station observations. However, there have been numerous studies that have verified the accuracy of *Jason* satellite altimeter products.

Comment 10: Line 231: You say the total model simulation time is 18 hours, but here you say the model is run for an additional 2 days for further validation, and then you show figures of model output over apparently four days. You also mention 'winter scenario' (line 243) but there is no mention of other scenarios. Can you clarify the simulation periods used in the evaluation, and state the relevant times in the figure captions?

Response: Thank you for your suggestion. To validate the simulated SWH, we used *Jason-3* satellite data. However, if we had simulated for too short a period, it is likely that there would have been no satellite data available during that time to validate the SWH in the study area. Therefore, we simulated for a total of 6 days. This also helped to show that the coupled model performed well. We have made this clear in the revised manuscript. We also rewrote the caption of Figure 8. In addition, we have emphasized in line 287 that all later analyses are based on the simulations from 0600 UTC 1 January to 1200 UTC 1 January. We have redrawn Figure 8 to make it clearer.

Comment 11: Line 250: In the caption 'Power output differences' – between what? I assume this is default Fitch – new FWFP scheme. But then it is incorrect to call these 'underestimates' of the power output because you don't know what the truth is, they are sensitivities.

Response: Thank you for your suggestion. We have rewritten the caption of Figure 9, and what it shows is indeed the difference between the Fitch and FWFP schemes. The term 'underestimates' is indeed incorrect, and our purpose was indeed to examine the sensitivity of power output, wind speed deficit, and TKE to the FWFP scheme. We have therefore modified the wording in the latter analysis.

Comment 12: Line 351: The first mention of Taylor and Yelland belongs in the experimental design, not at the end of the conclusions. I also would not agree that it is a 'complex iterative computational method'. It is a simple expression of roughness length as a function of SWH and wavelength (not frictional velocity), unless I am missing something?

Response: This is a good comment. In other words, we consider the Taylor and Yelland scheme to be a 'complex iterative computational method' when using the coupled atmosphere-wave model. The Figure R3 shows the Taylor and Yelland expression from WRF/phys/module_sf_mynn.F. It can be seen that the roughness length is related to the significant wave height (HWAVE), the peak wave length (LWAVEP) and the frictional velocity (UST).

```
#if defined SWAN_COUPLING || defined WW3_COUPLING
# if defined COARE_TAYLOR_YELLAND
          ZNT(I)=MAX(1200.0*HWAVE(I)*
     &            (HWAVE(I)/(LWAVEP(I)+0.001))**4.5+
     &             0.11*VISC/(UST(I)+0.001),1.59E-5)
```

Figure R3 Taylor and Yelland expression in WRF.

The Figure R4 shows that the roughness length is in turn used to calculate the frictional velocity, which in turn is calculated to obtain the surface heat flux, moisture flux, and etc. The new frictional velocity is then passed through the loop code to the next roughness length calculation using the Taylor and Yelland expression. So we consider this a complex iterative algorithm to implement in a numerical model.

```
!-------------------------------------------------------------
!-----COMPUTE THE FRICTIONAL VELOCITY:
!-------------------------------------------------------------
!     ZA(1982) EQS(2.60),(2.61).
PSIX=GZ1OZ0(I)-PSIM(I)
PSIX10=GZ10OZ0(I)-PSIM10(I)
! TO PREVENT OSCILLATIONS AVERAGE WITH OLD VALUE
OLDUST = UST(I)
UST(I)=0.5*UST(I)+0.5*KARMAN*WSPD(I)/PSIX
!NON-AVERAGED: UST(I)=KARMAN*WSPD(I)/PSIX

! Compute u* without vconv for use in HFX calc when isftcflx > 0
WSPDI(I)=MAX(SQRT(U1D(I)*U1D(I)+V1D(I)*V1D(I)), wmin)
IF ( PRESENT(USTM) ) THEN
   USTM(I)=0.5*USTM(I)+0.5*KARMAN*WSPDI(I)/PSIX
ENDIF

IF ((XLAND(I)-1.5).LT.0.) THEN        !LAND
   UST(I)=MAX(UST(I),0.005)  !Further relaxing this limit - no need to go lower
   !Keep ustm = ust over land.
   IF ( PRESENT(USTM) ) USTM(I)=UST(I)
ENDIF
```

Figure R4 Code in WRF related to the calculation of frictional velocity.

Technical corrections:

Note that this whole manuscript could greatly benefit by a technical edit for English usage.   I have only indicated the most noteworthy instances below.

Line 35:   'The installed capacity of offshore wind energy…'   This should be the beginning of a new paragraph.   The previous sentence does not clearly relate to the rest of the paragraph.

Response: Thank you for your suggestion. We have split the second paragraph into two parts.

Line 36:   change 'offshore' to 'near shore'.

Response: Thank you for your suggestion. We have modified it, in line 40 of the revised manuscript.

Line 71:   'Morrison' should be 'Morison'.

Response: Thank you for your suggestion. We have modified it, in line 77 of the revised manuscript.

Line 225:   In Table 1, 'Duhia' should be 'Dudhia', 'CORE' should be 'COARE', 'Talyor' should be 'Taylor'.

Response: Thank you for your suggestion. We have modified the relevant contents of

Table 1.

We thank you again for giving us an opportunity to revise this manuscript, and look forward to hearing from you.

Sincerely,

Shengli Chen