

*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 R11). The weakening effect of such small diameter piles on the significant wave height is not obvious.

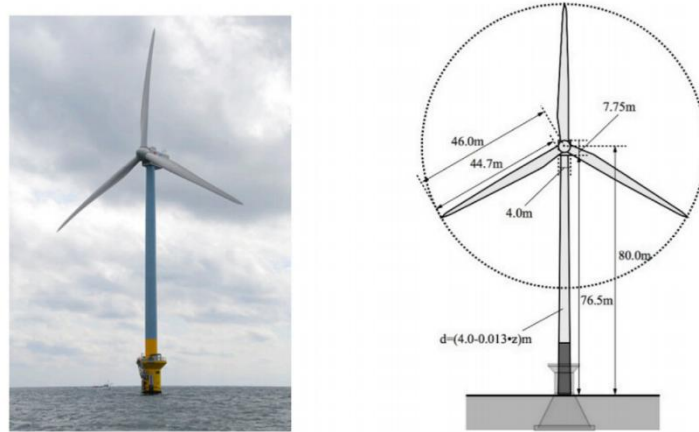


Figure R11 Wind turbine at Choshi demonstration site and its dimensions (Ishihara and Qian, 2018).

*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,

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

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

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  $\varphi$  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 R12) 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).

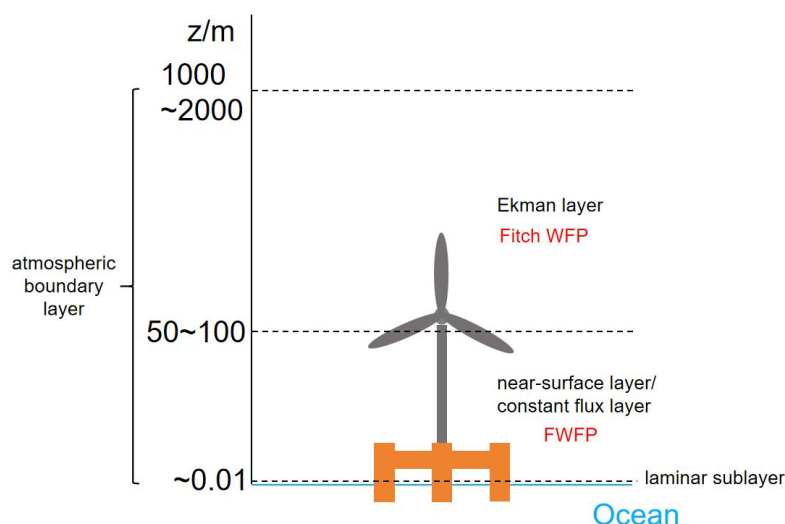


Figure R12 Composition of the atmospheric boundary layer.

*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 R13 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 R13 Taylor and Yelland expression in WRF.

The Figure R14 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=GZ10Z0(I)-PSIM(I)
PSIX10=GZ100Z0(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 R14 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.

*The authors implement a floating wind turbine parameterization in a coupled atmosphere-wave model. Their parameterization accounts for changes in wave properties due to the turbine's floating structure. In their wave parameterization, the authors develop a regression model, trained using a spectral wave model (SWAN), that accounts for the turbine's floating structure. The authors also modify the momentum tendency in the surface layer of the atmosphere. A source of momentum is included in the momentum tendency equation to represent changes in the momentum flux due to the floating turbine. Finally, the authors compare their floating turbine parameterization against the Fitch parameterization for a wind farm in the South China Sea.*

*The manuscript addresses a very interesting topic, namely the importance of including coupled atmosphere-wave models to evaluate the effects from offshore wind turbines in the flow overlarge regions. However, I have major concerns that should be addressed prior to publication, mainly about their modifications to the Fitch wind farm parameterization, which adds a non.physical source of momentum across the surface layer.*

**Responses to the comments of Reviewer #2:**

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

***Major comments:***

*Comment 1: Machine learning models: the manuscript lacks information about the ML models used therein. Also, there is no explanation of how the data are split into training and validation. Specifically:*

- a. The authors mention four different machine learning models. However, they do not provide information about neither of these models. Please include a more thorough description of each model, perhaps as an Appendix.*
- b. It seems the authors are training and validating the models using the same dataset. if so, this should be revised; otherwise, it is expected that the ML models are going to perform well. If not, please explain how you split the data for model validation*

**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.

We realized previously that we were not defining some of the data as validation data. So we made a modification. The SWH is taken from 2 m to 4 m with 0.1 m interval. The peak wave period is from 7.4 s to 7.6 s, 8.4 s to 8.6 s, 9.6 s to 9.8 s, 11.0 s to 11.2 s with an interval of 0.1 s, and the water depth is selected from 53 m to 98 m with an

interval of 5 m. This has a total number of 2520 ( $21 \times 12 \times 10$ ) experimental groups. We then select simulated data that do not include water depths of 58 m, 78 m, 98 m to train several machine learning (regression) models, since data from these three water depths would be used as validation data. The result of the validation is shown below (Figure R21), and the Matern 5/2 GPR model still performs best.

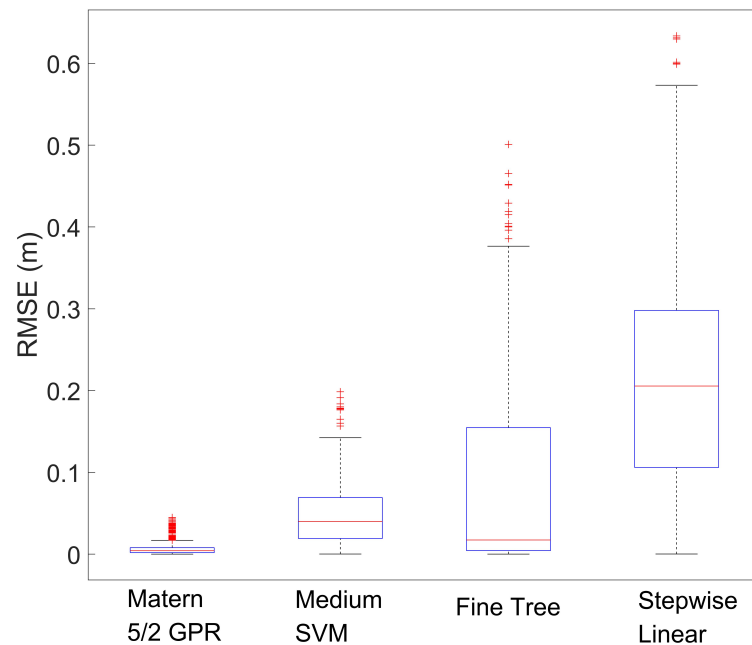


Figure R21. Box plots of RMSE for four typical ML regression models using validation data. The boxplots show the median (horizontal line), 25th to 75th percentile (box) and 5th to 95th percentile (whiskers). The whiskers extend to the most extreme data points not considered outliers, and the outliers are plotted individually using the '+' marker symbol.

*Comment 2: Momentum source across the surface layer (section 5.1): the authors include a non-physical source of momentum at turbine heights. Specifically:*

*a. I agree that changes in the momentum flux caused by variation in SWH affect winds close to the surface. However, these changes should be transmitted through modifications to the wall model (like in Jenkins et al, 2012; Paskyabi et al., 2014; Porchetta et al., 2021; Wu et al, 2020; Zou et al, 2018) rather than as an explicit source of momentum in the tendency equation over the bottom half of the turbine rotor layer.*

*b. What is the reasoning behind adding non-physical sources of momentum to across the surface layer? Also, shouldn't the source of momentum decay with height? if this is the case, then this should be rephrased as a modified wall model.*

*c. The references provided in Lines 41-43 suggest waves modify the wind profile through changes in surface stresses, not through injections of momentum across the surface layer: AlSam et al. (2015) and Yang et al. (2014) study how swell can modify wake propagation. Jenkins et al. (2012) use a coupled atmosphere-ocean model that modifies the wind field through changes in surface roughness. Kalvig et al. (2014) resolve waves with a moving mesh, thus the wind profile is effectively modified by*



changes in surface roughness. Paskyabi et al. (2014) develop a wall model that accounts for wave-induced momentum fluxes. Porchetta et al. (2021) and Wu et al. (2020) use an atmosphere-wave coupled model, where the winds are modified by waves through changes in surface roughness. Zou et al. (2018) also focuses on a wall model.

**Response:** This is a good comment. We agree that waves modify the wind profile through changes in surface stress rather than through momentum injection through the surface layer. Waves can change the roughness length of the atmospheric subsurface, which in turn affects the momentum transport from the atmosphere to the ocean and to the waves. In this study, we argue that the coupled model does not account for the significant changes in roughness caused by large floating platforms affecting waves. This implies that the estimation of momentum fluxes in the sub-grid is incorrect, i.e. the momentum transport from the atmosphere to the ocean and waves needs to be reassessed. We believe that the loss of kinetic energy in the grid is not only due to the turbines, but also to changes in kinetic energy due to unresolved wind stress (friction) work. Thus, this is indeed a physical source of momentum. In the new wind farm parameterization scheme, this source really does not decrease with height. This is because momentum fluxes, heat fluxes, vary less with height in the near-surface layer (constant flux layer). Since the maximum height of the near-surface layer is about 100 meters, we also considered that the new scheme is only applicable up to a height of 100 meters (Figure R22).

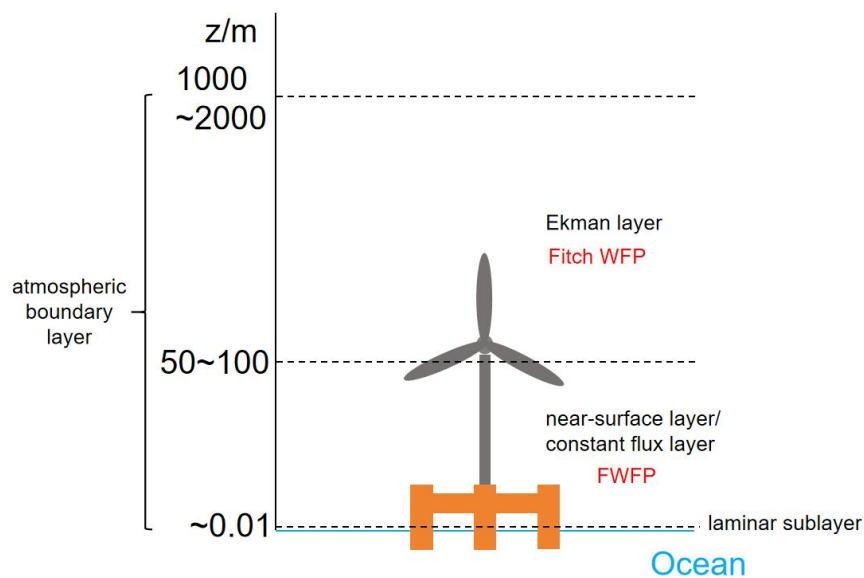


Figure R22. Composition of the atmospheric boundary layer.

*Comment 3: Model configuration in Section 5.2: The authors use a 12 km horizontal grid spacing for their simulations. However, Tomaszewski and Lundquist (2020) show such coarse grid scan produce unrealistic impacts over a very broad region. Please explain your choice of grid spacing.*

**Response:** Thank you for your suggestion. The horizontal resolution of the previous

WRF was really coarse, so we conducted new experiments. Two nested domains are used in WRF with their respective grid spacings of 9 and 3 km.

*Comment 4: Section 5.4: The authors conclude that Fitch overestimates wake effects. However, the FWFP is artificially accelerating wake recovery downstream of the turbines. Thus, it is expected to have higher power production estimates and lower wake deficits in the FWFP*

*Lines 243-244: Adding a source to the momentum tendency is expected to accelerate wake recovery downstream of the turbines. Thus, is it reasonable to say that that Fitch underestimates power output? Rather, the momentum source in the FWFP accelerates wake recovery; thus, momentum availability increases amplifying power production.*

*Lines 257-258: same as above.*

**Response:** This is a good comment. We agree that the statement "Fitch underestimates power output" is not reasonable and have reworded it. We also agree that the additional momentum source in the FWFP affects wake recovery. Thus, the increased momentum availability increases power output. However, we do not believe that the power output increases for all turbines (Figure R23). We also wrote in the last paragraph of the paper that the decrease in significant wave height does not necessarily lead to higher wind speeds in the near-surface layer. This is because the associated iterative algorithms in WRF are too complex, so reducing the significant wave height also has the potential to reduce near-surface wind speeds. Figure R24 shows that the roughness length in the Taylor and Yelland scheme is determined by the significant wave height, the peak wave length, and the frictional velocity. Figure R25 shows that the roughness length in turn is involved in the calculation of the new friction velocity. The new friction velocity then determines the heat flux, moisture flux, etc., and is looped into the calculation of the next roughness length. Thus, it cannot simply be assumed that the additional momentum source in the FWFP increases momentum availability.

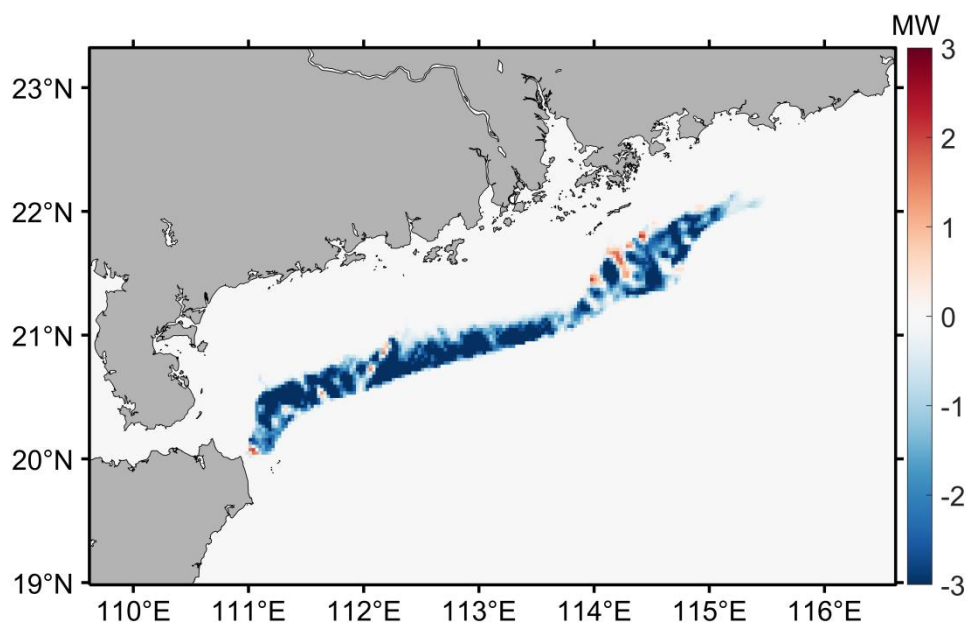


Figure R23. Power output differences between WRF-Fitch and WRF-FWFP cases.

```
#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 R24. Taylor and Yelland expression in WRF.

```
!-----COMPUTE THE FRICTIONAL VELOCITY:
!-----
!      ZA(1982) EQS(2.60),(2.61).
PSIX=GZ10Z0(I)-PSIM(I)
PSIX10=GZ100Z0(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 R25. Code in WRF related to the calculation of frictional velocity.

*Minor comments:*

1. *I recommend English language revisions throughout the manuscript.*

**Response:** Thank you for your suggestion. We have revised the entire manuscript.

2. *Lines 22-24: What about coupled meso-microscale simulations? Coupled mesoscale-LES simulations using WRF can capture these effects, however, at a higher computational cost.*

**Response:** Thank you for your suggestion. A brief overview of the coupled mesoscale-LES simulations is given in lines 25-27.

3. *Line 31: Please add punctuation as: "... sink on the mean flow. Most of..."*

**Response:** Thank you for your suggestion. We made modifications.

4. *Lines 27-44: I recommend splitting paragraph #2 in the introduction, perhaps at line 35.*

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

5. *Lines 43-44: I would argue that the current parameterization can be suitable for floating offshore wind farms. Rather, the atmosphere-only model in WRF does not*

*capture changes in roughness length over the ocean caused by the presence of floating turbines*

**Response:** This is a good comment. We agree that atmosphere-only model cannot be used when applied to offshore wind farms. However, we believe that only semi-submersible floating wind turbines with large floating platforms have the greatest impact on local waves (Figure R26). Current wind farm parameterization schemes are not applicable to such floating wind turbines.

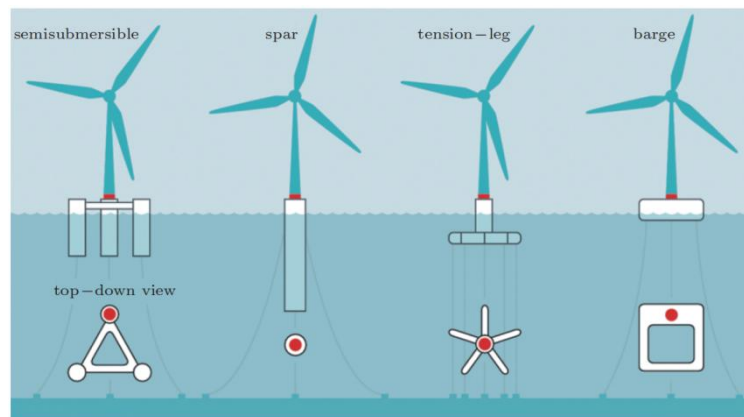


Figure R26. Floating wind turbine classification.

6. Line 74: Please explain why you chose  $d = 20$  m.

**Response:** Thank you for your suggestion. This is a common draft depth for semi-submersible floating wind turbines.

7. Captions should fully describe the figure. Please include additional information in all captions to make each figure self-explanatory. For example, include description of the different terms and symbols used in Figure 1, as well as the significance of the red contours.

**Response:** Thank you for your suggestion. We have modified all captions to include detailed information.

8. Figure 3: It is difficult to read the information within the grey area. Please use colors with higher contrast. Also, what is the meaning of the blue curves (presumably schematic for waves) to the side of the plot?

**Response:** Thank you for your suggestion. We have redrawn the figure. The blue curve on the right side of the plot represents waves.

9. Line 173: “The important point in the derivation ...” implies that the source of TKE in the Fitch parameterization is not important. Please rephrase.

**Response:** Thank you for your suggestion. We have revised this sentence.

10. Lines 193-201 and Figure 6: Please maintain consistency in your nomenclature (e.g., the authors use  $u_{*,wt}$  in Eq. 17, but  $ustwt$  in Figure 6)

**Response:** Thank you for your suggestion. We have redrawn the figure.

*11. Figures 11, 13, 14: It would be helpful to show the top and bottom of the turbine rotor layer for reference.*

**Response:** Thank you for your suggestion. We have redrawn the relevant figures. The diagram shows the hub height of the turbine and the top and bottom of the rotor with horizontal solid and dashed lines.

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

Sincerely,

Shengli Chen