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 × 12 × 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 R1), and the Matern 5/2 GPR model still performs best.

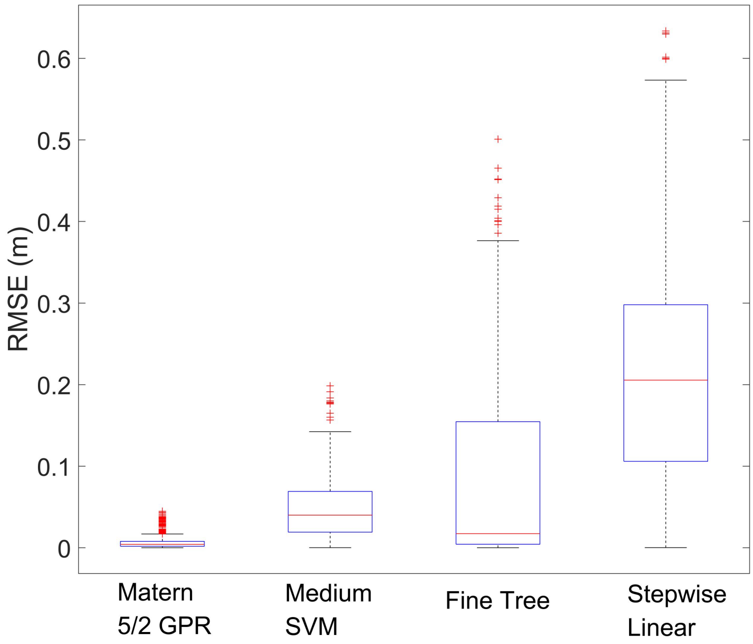


Figure R1. Box plots of RMSE for four typical ML regression models using validation data.The boxplots show the median (horizontal line), 25th to 75th percentile (box) and 5th to 95th percentile (whiskers). The whiskers extend to the most extreme data points not considered outliers, and the outliers are 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 etal., 2014; Porchetta et al., 2021; Wu et al, 2020; Zou et al, 2018) rather than asan 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. Kalviget 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 R2).

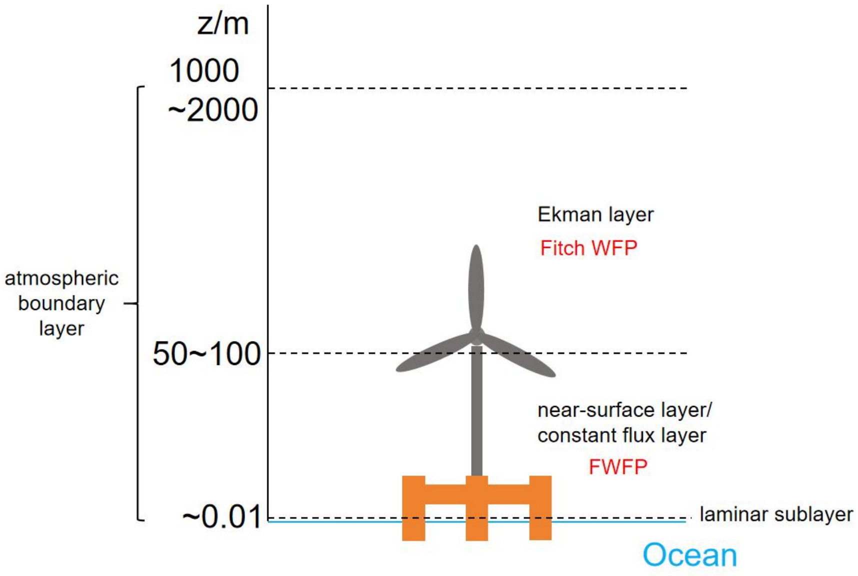


Figure R2. Composition of the atmospheric boundary layer.

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 R3). 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 R4 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 R5 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.

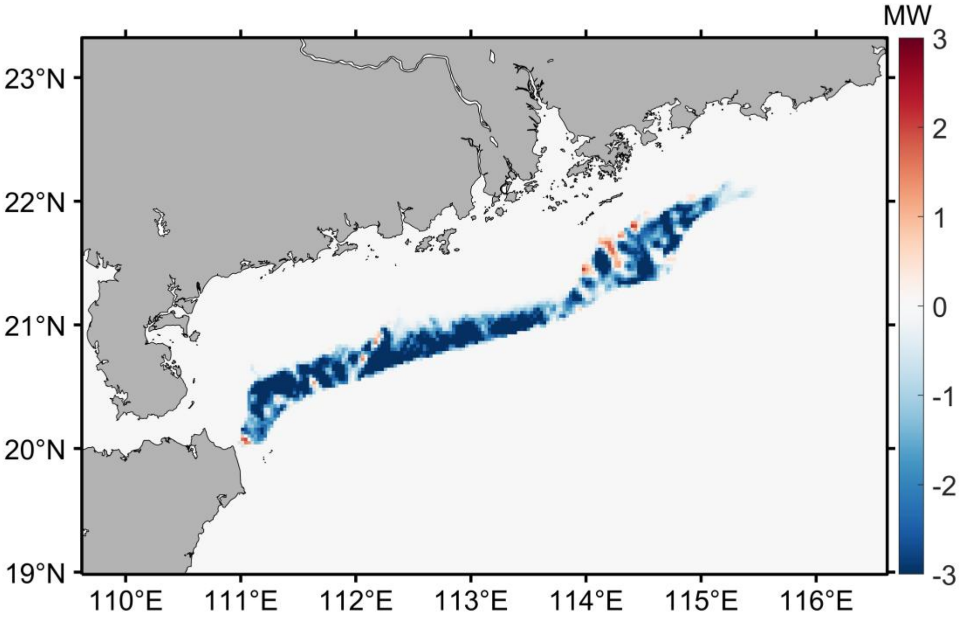
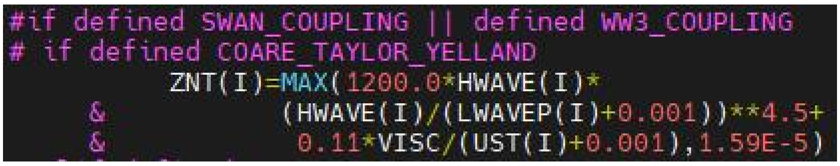
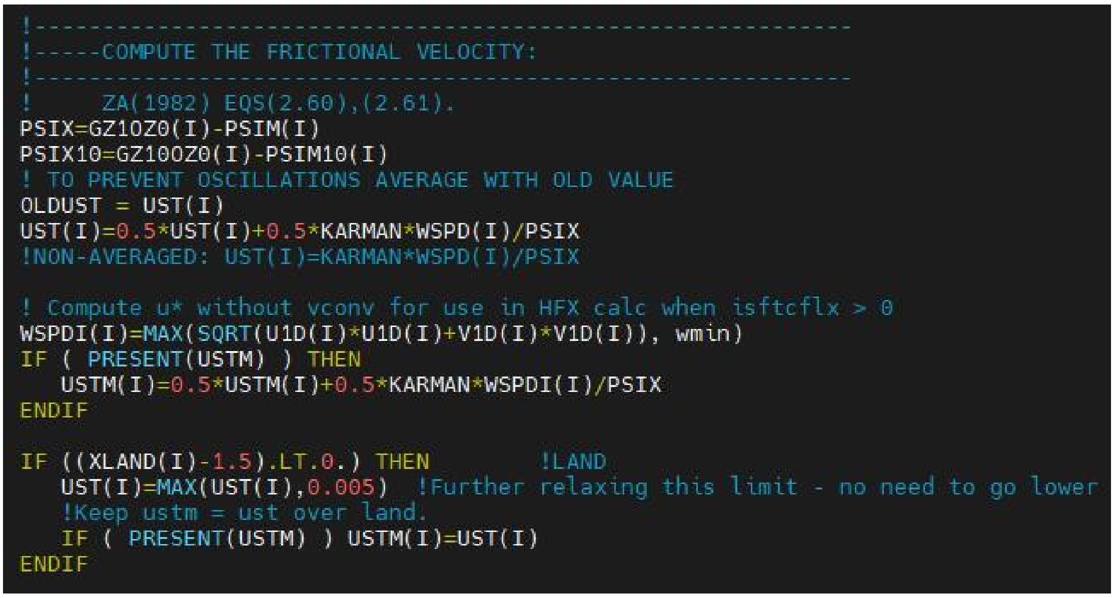


Figure R3. Power output differences between WRF-Fitch and WRF-FWFP cases.



```
#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 R4. Taylor and Yelland expression in WRF.



```
!------------------------------------------------------------
!-----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 R5. 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 R6). Current wind farm parameterization schemes are not applicable to such floating wind turbines.

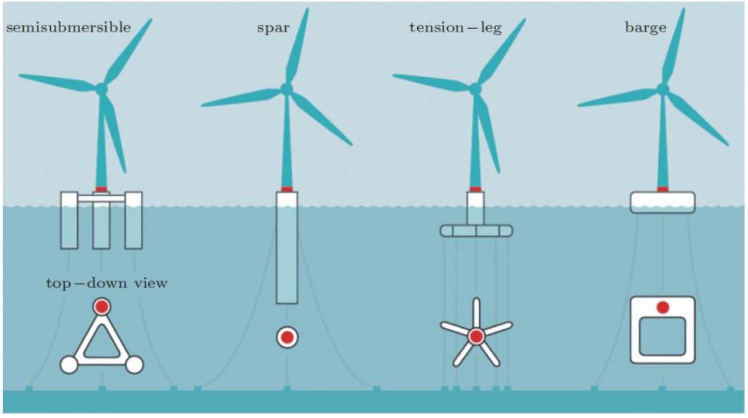


Figure R6. Floating wind turbine classification.

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