We thank the reviewers for their valuable feedback and comments. We provide responses to their comments and suggestions below in the Table in blew, point-by-point.

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| **Editor comment** | | |
| Thank you for your responses to the reviewers' concerns. As seen in the reviewer responses, there are still concerns regarding careful description of the LES, spin-up, the role of the SGS model, as well as the assessment of the goals of the paper (specifically shifting the narrative from "improving turbulence representation" to "characterizing the impact of coupling on turbulence fields".<br>Because I am unsure if the authors can see the reviewer responses in this second iteration, I have copied them below (for reviewer 1) and attached them here (for reviewer 2).<br>Please address these comments in your next iteration. | | |
| **Reviewer 1** | | |
| 1 | The authors response to my comments are appreciated, as are the inclusion of additional material to put the current work and its limitations into context with similar work. However, I would still like to see some additional points of clarification addressed:<br><br>Line 125: Somewhere here can you briefly describe the uncoupled simulations, specifically that they seem to include only WRF but not SWAN, | RC1 |
| | Thanks for your comment. Both the coupled and uncoupled WRF simulations use the same configuration; this has been clarified around line 123 of the revised manuscript. | Author |
| 2 | And then – related to Line 197 comment below – how roughness length is computed for the uncoupled case. You do mention the Charnock-based scheme on Line 241, and I believe this is in fact the default roughness scheme for the Revised MM5 Monin-Obukhov scheme that you mention in Line 146, but it would be good to state this explicitly in this section (and maybe restate around Line 197 when you present the surface roughness results). It might also be good to give the reference for this surface layer scheme as you do for the other physics options:<br>Jiménez, P. A., J. Dudhia, J. F. González-Rouco, J. Navarro, J. P. Montávez, and E. García-Bustamante, 2012: A Revised Scheme for the WRF Surface Layer Formulation. Mon. Wea. Rev., 140, 898–918, https://doi.org/10.1175/MWR-D-11-00056.1 | |
| | Thank you for the clarifying comment. The description of how roughness length is calculated in the uncoupled simulations has been added to both the Modeling System section (line 99) with Eqs 1 and 2. We have also added the citation for the Revised MM5 scheme. | Authors |
| 3 | Line 154: 'The PBL scheme assumes horizontal homogeneity within each grid cell, which contrasts with the LES domain, where turbulence is explicitly resolved….' I would say that the LES also assumes horizontal homogeneity within each grid cell; instead, I would say the actual contrast between LES and PBL scheme is that the PBL scheme resolves none of the turbulence whereas the LES partially resolves it. | RC1 |
| | As suggested, we have corrected the revised manuscript around line 160. | Authors |
| 4 | Line 213: The statistics shown in Figure 5 do not seem to be consistent with what are shown in Figure 6, I assume because the time periods are different. To make things easier on the reader, could you include the time period used for Figure 5, in the caption and/or Section 3.3, in the same way that you do for Figure 6? In particular, the LES models seem to have a systematic positive bias above about 150 m in Figure 6, but a relatively low bias in Figure 5. | RC1 |
| | Thanks for pointing out this. The caption of Figure 5 has been edited as suggested. | Authors |
| 5 | Line 238: '…although both with a systematic bias at all measured levels': change 'with' to 'have'. | RC1 |
| | As suggested, the wording has been revised. | Authors |
| 6 | Line 245: It still is not clear to me dynamically how the slight eastward directional deviation of LES vs. mesoscale is explained by excessive LES vertical wind shear. But in any event, the deviation between the two is only about 4 degrees, which might be less than the | RC1 |

Commented [AP1]: I guess here and in the revised manuscript you mean the "WRF simulations"? If so then please add this here and in the revision

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| | observational precision of standard direction measurements, and so might not have much significance. I might just leave out the part of this sentence after the comma. | |
| | Thank you for the comments and suggestions. The sentences have been changed as suggested. | Authors |
| 7 | Line 280: In the response to my general comment 2 the authors state in part: 'The purpose of these simulations is primarily to illustrate the limitations in producing resolved turbulence at a grid spacing of 100 m', referring to D05 and D06. While the response is appreciated, it seems that a similar statement belongs in the text at the end of Section 3.4, where these runs are discussed. The current text simply states that 'The D05 and D06 domains are included to illustrate how increasing horizontal resolution modifies the balance between resolved and SGS turbulence' without noting that these modifications, based on many previous LES studies, are expected to make the turbulent statistics more realistic (admittedly here without direct observational verification). Could some of the material from the discussion in lines 331-340 be moved here? | RC1 |
| | It is a good suggestion. We followed the suggestion and revised the text accordingly, including moving the text to the section at line 270 in the revised manuscript. | Authors |
| 8 | Line 315: 'All of the simulations have a bias in the upper atmospheric levels, which might be due to a bias in boundary conditions.' A) What are 'all'? Mesoscale only, or LES? and is this referring to Figure 5, Figure 6, or both? See my comment on Line 213. B) If, as I think, this is referring to the mesoscale simulations as described in Line 238 for Figure 6 (but also apparently true for Figure 5), then the bias is not at upper atmospheric levels (which are actually not that high at ~ 200 m) but at all levels, and I think the fact that it is at all levels actually makes it more likely that the bias is from the boundary conditions (because a bias in local physics would presumably be strongly height-dependent at thus would not be systematic at all vertical levels). | RC1 |
| | We thank the reviewer for this detailed comment. The statement has been clarified in the revised manuscript. The bias is assessed consistently in both Figure 5 and Figure 6. We find that the mesoscale simulations exhibit a positive bias at all levels, while the LES simulations show a negative bias below approximately 90 m and a positive bias above this height, as seen in both figures. We also corrected a labeling error in Figure 5 in the revised version. The text has been updated accordingly to improve clarity in the revised version at line 302 . | Authors |
| 9 | Line 350: Remove question mark. | RC1 |
| | As suggested, the question mark is removed. | Authors |
| | **Reviewer 2** | |
| 1 | Thanks for the clarifying comment. We now discuss (in Section 3.3) that the improved agreement of mean wind speeds simulated in domain 04 (d04) for the vertical levels lower than 100 m is due to, although perhaps counterintuitive, a mismatch of the simulated vertical profile in d04. The simulated profile in d04 shows higher wind shear than both the simulations in d03 (a mesoscale domain) and the observations. These are actual observations based on both the measurements and simulations, and a similar behavior is observed for the coupled simulations. The question is what is causing the excessive vertical shear in d04. The principal suspect is the SGS model, which is known to cause excessive vertical shear close to the surface (Sullivan 1994 and Gadde 2021). We clarify that this is a speculative comment in Section 3.3, since as you noticed we only simulated with one SGS model and over a particular event. We also acknowledge that, without turbulence measurements, we cannot verify whether the changes in the turbulence fields represent improvement on the way we simulate the atmosphere. However, the increase in turbulence fluxes in the coupled simulations is consistent with the expected physical response to wave-modified- surface roughness, and this clarification has been added to the turbulence section and conclusions.<br><br>I would like to thank the authors for adding the necessary clarification to the text However, a few points regarding the turbulence representation remain a concern: | RC2 |

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| | • Scope of Speculation: The authors note that their comments on the SGS model and turbulence are "speculative" due to the single event nature of the study. While this transparency is appreciated, the conclusion of the paper should reflect this limitation more prominently to ensure that the findings are not over-generalized by readers.<br>• Physical Realism: Since it is still unclear whether the altered vertical distribution of turbulence constitutes a more accurate representation of the offshore boundary layer, the authors should ensure that the revised conclusion does not conflate "increased intensity" with "increased accuracy." I encourage that the authors make sure the abstract and concluding remarks are consistent with these new discussions, specifically shifting the narrative from "improving turbulence representation" to "characterizing the impact of coupling on turbulence fields." | |
| | Thank you for your comment. We agree that the findings of this study should not be generalized. Owing to the lack of high-frequency turbulence observations, the results do not allow us to demonstrate increased accuracy; rather, they mainly illustrate the impact of coupling compared to the uncoupled simulations. These points have been clarified in the final paragraph of the Abstract and in the Conclusions section (line 379) of the revised manuscript. | Authors |
| 2 | As the reviewer points out, a large portion of the momentum fluxes rely on the SGS model when using a 100-m coarse LES. This is why 100-m LES are known as "coarse LES" or "poor-man's LES"(not high-resolution mesoscale). But note that the choice of 100 m is also intentional: this resolution is widely used in industry and operational multiscale modeling because it is computationally affordable (Vortex2018 and Kshetri 2025). We have clarified in the Simulations Subsection.<br><br>I appreciate the authors' response. One concern remained is if a significant portion of the momentum transport and near-surface shear is handled by the SGS model rather than being resolved, then the "benefit of coupling" reported in the manuscript may actually be a reflection of how the SGS scheme reacts to the SWAN-provided roughness, rather than a genuine improvement (or increase) in the resolved eddy structure. Without clarifying this, I believe it is difficult to discern if the reported 'benefits of coupling' are a robust physical signal or localized response of the specific SGS scheme to the updated roughness. | RC2 |
| | We thank the reviewer for this insightful comment. We agree that, particularly at 100 m grid spacing, a substantial portion of the momentum transport and near-surface shear is represented by the SGS model, and therefore the response to coupling may partly reflect how the SGS scheme adjusts to the wave-modified roughness rather than changes in resolved turbulent structures alone. To address this, we have revised the Discussion section (lines 334 and 345) to explicitly clarify this limitation and to adopt a more cautious interpretation of the coupling impact. In particular, we emphasize that the differences in near-surface shear and turbulent fluxes between the coupled and uncoupled simulations represent the response of the model to the coupling, rather than an unambiguous improvement in resolved turbulence representation. | Authors |
| 3 | Thank you for the clarifying comments. Regarding the wave boundary conditions, we used a hot start for SWAN, so the wave field was already developed at the beginning of the simulation, and the outermost domain is large enough for wave growth to occur before Authors reaching the RUNE area. In addition a storm situation is likely more reflected in wind-driven waves rather than swell which is another reason, why for this particular case we don't need wave spectra at the open boundaries. Concerning the physical processes, SWAN considers processes related to sea spray or nonbreaking -wave induced mixing, specifically by modeling white capping and wave induced breaking, which are the primary sources of sea spray. While SWAN does not directly simulate individual spray droplets, it incorporates advanced wave boundary layer models and whitecapping dissipation parameters that are essential for accurate sea-spray, wind input, and heat flux parameterizations, particularly in coupled atmosphere-wave models. We added text to explain these in the revised version, Section 2.3 (Simulations) | RC2 |

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| | I thank the authors for the clarification regarding the SWAN hot start and domain fetch, which addresses the initialization concerns. However, the response regarding sea spray and non-breaking mixing remains somewhat ambiguous. While whitecapping is indeed the source of spray, the physical impact of sea spray on the MABL (e.g., impact on enthalpy fluxes or potential drag reduction at high wind speeds) requires specific parameterizations within WRF, not just the wave model (SWAN). Similarly, nonbreaking wave-induced mixing is an upper-ocean process that would typically require an active ocean component (ROMS) to influence SST and, subsequently, the wind profile. I encourage the authors clarify in Section 2.3 that while the sources of these processes are resolved by SWAN, their direct feedback on atmospheric thermodynamic and momentum is not explicitly parameterized in this setup. Acknowledging this as a limitation would provide better context for the results, especially in the high wind regions of the storm. | |
| | We thank the reviewer for highlighting the need to distinguish between the representation of physical processes in SWAN and their feedback onto the atmosphere. We agree that, while SWAN accounts for wave processes such as whitecapping and breaking that are related to sea spray generation and upper-ocean mixing, their direct impact on atmospheric thermodynamic fluxes (e.g., enthalpy fluxes) and additional momentum adjustments is not explicitly parameterized in the current WRF–SWAN configuration, nor is upper-ocean mixing feedback through an active ocean model included. We have therefore revised Section 2.3 (Simulations, line 183) to explicitly state this limitation and to clarify that wave-related effects influence the atmosphere only indirectly through modifications of surface roughness and stress within the coupled surface-layer formulation. | Authors |
| 4 | All domains share the same spinup- period, despite the shorter total runtime of D05 and D06.In the comparison of the fluxes from D06, D5, and D04 (section 3.4 of the revised manuscript) the results from all domains are taken from the 2 hours after a spin-up of 8 hrs from the same initialization time. The purpose of D05 and D06 in this study is to illustrate how horizontal resolution affects the ratio of resolved to SGS turbulence. We now have clarified this point in the Simulations and turbulent fluxes Sections (2.3 and 3.4)<br><br>I thank the authors for clarifying the 8-hour spin-up period and the illustrative intent of the finer domains (D05/D06). However, I would like to emphasize that turbulence spinup in nested LES is a function of both time and spatial fetch. Given the large nesting ratios, even with 8 hours of runtime, the resolved structures in D06 may still be constrained by the boundary conditions from the much coarser D04. While I accept the use of D05 and D06 as a resolution sensitivity study, I recommend the authors include a brief statement in Section 4 (Conclusions) explicitly noting that the absolute values of fluxes in the finest domains should be interpreted with caution due to the limited domain size and potential fetch-related development artifacts. This would prevent readers from over-generalizing the specific variance peaks observed in the 4-m and 20-m simulations. | RC2 |
| | Thank you for this important comment regarding turbulence development in nested LES and the role of fetch and boundary conditions. We agree that, in general, turbulence spin-up can be influenced by both spatial fetch and nesting effects. In our configuration, the inner domains (D05 and D06) are sufficiently large to partially allow turbulence development, and the application of the CPM at the lateral boundaries further accelerates turbulence. Nevertheless, to avoid over-interpretation, we have added explanation at the end of Discussion Section (line 366) of the revised manuscript. | Authors |
| 5 | Thank you for the comment. Two different mechanisms contribute to the turbulence differences shown in Fig. 6. The first is a resolution related effect associated with the transition from the mesoscale domain (D03) to the LES domain (D04): switching from a PBL scheme to the Deardorff SGS model changes the partition between resolved and SGS turbulence, and the 100 m grid spacing limits how much of the turbulent spectrum can be explicitly resolved. This explains the differences between D03 and D04, independent of coupling. The second mechanism is the effect of wind–wave coupling itself, seen when | RC2 |

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| | comparing the coupled and uncoupled simulations within the same domain. Here, the WBLM modifies the surface roughness and -near surface stress, enhancing shear production and increasing resolved turbulence.- The finer domains (D05 and D06) further illustrate how increasing horizontal resolution shifts a larger fraction of turbulence from the SGS to the resolved scales. We have added clarifications on these aspects in the Discussion Section 4.<br><br>I appreciate the authors' explanation in Section 4 regarding the roles of resolution transition and WBLM-induced roughness. However, the mechanistic interpretation remains somewhat inferred rather than demonstrated. While the link between increased surface roughness and shear production is theoretically sound, the manuscript lacks the intermediate diagnostics (such as TKE budget components) to prove that the increased variance is specifically coming from the WBLM-modified shear. Turbulence intensity in this setup can be influenced by factors beyond Z0, including: Atmospheric Stability: Small changes in near-surface temperature gradients can shift buoyancy production. SGS Dissipation: The coupling may alter the mixing lengths or dissipation rates in the Deardorff scheme. Wave-induced Motion: "Resolved" variance may include coherent wave-induced motions rather than purely stochastic turbulence. Given that D04 is a "coarse LES" where the SGS model handles a significant portion of the momentum transport, I recommend that the authors rephrase their findings to emphasize that coupling increases model sensitivity and turbulence intensity, while maintaining a cautious tone regarding the "improvement" of the underlying physical representation. | |
| | We thank the reviewer for this comment. We acknowledge that the interpretation is inferred rather than explicitly demonstrated and therefore we have revised the text to adopt a more cautious tone, emphasizing that the coupling leads to a stronger response of the simulated fields to changes in surface forcing, rather than an improvement in physical representation. This has been clarified in the Discussion (line 333). | Authors |