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.

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:

- 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.
- 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.”

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.

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.

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 waveinduced 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, windinput, and heat flux parameterizations, particularly in coupled atmosphere-wave models. We added text to explain these in the revised version, Section 2.3 (Simulations)

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, non-breaking 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.

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)

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 spin-up 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.

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 comparing the coupled and uncoupled simulations within the same domain. Here, the WBLM modifies the surface roughness and -nearsurface 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.

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.