## Summary of Revisions

- Revised the turbulent dissipation framework based on Belcher et al., 2012 to avoid decomposing dissipation into separable shear- and Langmuir-driven components. We explicitly clarified this physical basis in the Methods section.

- Explicitly distinguished between diagnostics of Langmuir *forcing potential* (e.g., $La_t$) and diagnostics of Langmuir *mixing efficiency* (e.g., projected metrics and vertical velocity variance), clarifying the physical interpretation of each.

- Introduced the dynamically oriented Langmuir cell angle $\alpha_{\mathrm{LOW}}$ following Van Roekel et al., 2012 and added a quantitative assessment of wind–wave misalignment effects using the ratio $R_{LT}$, showing that misalignment typically reduces Langmuir-induced vertical velocity variance.

- Revised the exceedance-based diagnostics to emphasize their role as relative, seasonally normalized measures referenced to open-water conditions, rather than absolute comparisons across latitude or ice regimes.

- Replaced Figures 1–7 with revised versions that more clearly motivate the physical interpretation of Langmuir-based metrics, regime classification, intermittency, and the modulation of turbulent energetics by waves and sea ice.

- Removed speculative language linking enhanced turbulent energy directly to enhanced vertical mixing, and clarified throughout that the analysis diagnoses energetic availability for mechanically forced turbulence rather than realized mixing rates.

- Expanded discussion of the limitations associated with neglecting prognostic ocean stratification and buoyancy forcing, and identified fully coupled ocean–ice–wave models and stratified reanalyses as key priorities for future work.

- Added targeted evaluation of model inputs relevant to Langmuir diagnostics, including comparisons of winds, wave attenuation, and ice heterogeneity, and discussed associated uncertainties and their implications for the robustness of the conclusions.

# Reviewer Comments and Author Responses

**Manuscript:** *Langmuir Turbulence in the Arctic Ocean: Insights From a Coupled Sea Ice–Wave Model*

**Authors:** Aikaterini Tavri et al.

**Author Response:**

We thank the reviewers for their careful, constructive, and detailed assessment of the manuscript. We appreciate the time and effort devoted to providing thoughtful feedback. Below, we respond to all general and specific comments in detail. In response to the reviewers suggestions, we have revised the manuscript to improve clarity, strengthen the physical interpretation, and enhance the overall presentation. In particular, we have expanded and clarified the discussion of turbulent kinetic energy scaling, refined the description of the underlying assumptions, and better articulated the role of sea ice in modulating upper-ocean mixing processes. We believe these revisions have significantly improved the rigor, clarity, and coherence of the manuscript.

## Reviewer - 1

### General Comment 1

**Reviewer Comment:**

My first concern is on the use of an enhancement factor defined in Eq. (8) to scale the enhancement of TKE dissipation by Langmuir turbulence in Eq. (9). The Langmuir enhancement factor in Eq. (8) describes the enhancement of turbulent velocity scale, which is based on scalings of vertical velocity variance in a set of large eddy simulations described in Van Roekel et al., 2012. The TKE dissipation does not necessarily scale in the same way. In fact, it shouldn't scale the same way as it depends on the turbulent velocity scale cubed. One may instead use Eq. (5) in Belcher et al., 2012 to estimate the enhancement of TKE dissipation due to Langmuir turbulence. But I'm not sure it is possible to clearly attribute the TKE dissipation to shear-driven and Langmuir-induced component. This incorrect scaling of TKE dissipation may explain the mismatch between the results and theory in Fig. 5b. Since this study is based on the turbulence scalings (as summarized in Table 1), the choice of the scaling of TKE dissipation may significantly affect the conclusions and discussions, in particular the interpretation of Langmuir turbulence's influence on TKE dissipation in Section 4.3 and the impact of wind-wave misalignment on the dissipation ratio in Section 4.4.

**Author Response:**

We thank the reviewer for identifying this inconsistency in the original formulation. In the revised manuscript, we explicitly separate the treatment of velocity-based and dissipation-based diagnostics. Vertical velocity variance is diagnosed using the LES-based scaling of Van Roekel et al. (2012) as a measure of Langmuir turbulence potential, while TKE dissipation is estimated independently using the vertically integrated formulation of Belcher et al. (2012), which is consistent with the cubic velocity dependence and the underlying TKE budget. Because shear-driven and Langmuir-driven production interact nonlinearly in the vertically integrated equation, we do not attempt to attribute dissipation to individual source terms, but instead interpret the diagnosed dissipation as a net response to coupled wind–wave forcing. The revision establishes a physically consistent framework that is now detailed in Sections 3.4 and 3.5.

### General Comment 2

**Reviewer Comment:**

My second concern is that the turbulence scalings used in this study were derived in ice-free conditions. It is not clear how well these scalings describe the effect of Langmuir turbulence on the turbulent mixing in the presence of sea ice. While I understand that an assessment of the validity of turbulence scalings in the presence of sea ice may be beyond the scope of this study, a more careful discussion on this point would be helpful.

**Author Response:**

We agree with the reviewer that the Langmuir turbulence scalings employed here were derived primarily from ice-free LES and that their direct applicability in the presence of sea ice is uncertain. An explicit validation of these scalings under ice-covered or partially ice-covered conditions is beyond the scope of this study. In the revised manuscript, we clarify that the analysis based on these scalings is intended to represent *Langmuir turbulence potential*, rather than the realized turbulent mixing in sea ice conditions. We further elaborate on how sea ice modulates Langmuir forcing indirectly—by attenuating surface waves, altering wind–wave alignment, and modifying the partitioning of surface stress. These effects are explicitly incorporated into our model through the prescribed wave field, sea ice concentration, and effective surface stress. These clarifications place appropriate bounds on the interpretation of the results and highlight the need for future large-eddy simulation and observational studies to directly quantify Langmuir turbulence under ice-covered conditions.

### General Comment 3

**Reviewer Comment:**

Finally, the effects of surface buoyancy flux are probably significant in the turbulent mixing in the Arctic, for example, during ice formation/melting and in open waters between sea ice when air-sea temperature difference is large. A discussion on the effects of surface buoyancy flux versus the role of Langmuir turbulence would be helpful.

**Author Response:**

We note that buoyancy forcing is not explicitly represented in our coupled configuration, which is designed to isolate mechanically driven turbulence. This assumption is generally appropriate for ice-covered regions, where the upper ocean is often weakly stratified or stably stratified. However, we acknowledge that buoyancy forcing may locally dominate during periods of freezing or intense surface cooling. In the present analysis, we assume additivity among turbulent production mechanisms and treat Langmuir turbulence as a primarily mechanical process, distinct from buoyancy-driven convection. We now clarify this limitation in the manuscript and emphasize that future coupled implementations could explicitly incorporate surface buoyancy fluxes to assess their interaction with Langmuir-driven mixing.

## Specific Comments

Below we provide point-by-point responses to specific comments.

- **L60:** Define "LT potential."
  *Defined in manuscript*

- **Section 2 and 3:** Something wrong with the section title? *Fixed*

- **L91–92:** Was WW3 forced with the same ocean and atmosphere forcing?
  *Yes, both neXtSIM and WW3 used ERA5 wind and sea ice concentration forcing for consistency.*

- **L98:** delete the second "both"? *Fixed*

- **Eq. (2):** Why not account for ocean currents? Does it matter here?
  *We now explicitly clarify this assumption in Section 2.3, noting that typical MIZ surface currents are small compared to wind speeds and therefore contribute negligibly to the diagnosed atmosphere–ocean stress..*

- **L104**: $C_{ao}$ should not be in bold font? *No, it is a scalar not a vector.*

- **L107–108:** Be more specific on what do "the surface forcing of momentum pathways"? It would be helpful to list what variables in the GLORYS12 reanalysis and ERA5 were used.
  *Considered.*

- **L132–133:** In addition to wind-wave misalignment, another refined formulation is to account for the decay of Stokes drift with depth. Any comments on this?
  *In ice-covered and strongly stratified Arctic waters, the contribution of Stokes drift to Langmuir forcing is effectively confined to the near-surface layer. In our study region, mixed layers are typically deeper than the e-folding depth of short surface waves, and under-ice wave attenuation further suppresses near-surface orbital velocities. As a result, the surface Stokes drift* $u_s(0)$ *used in our analysis already represents an upper bound on the wave-driven momentum available to drive Langmuir turbulence. Explicitly accounting for the full vertical decay of Stokes drift would therefore yield even weaker Langmuir forcing, reinforcing, rather than altering, our primary conclusions.*

- **L153:** Van Roekel et al., 2012 is probably a more appropriate reference here.
  *Considered*

- **L155 and L176: \citep -> \citet** *Updated*

- **Eq (9): Also (11)**. As I mentioned in my general comment, I don't think the effect of LT on TKE dissipation can be estimated in this way.
  *Updated*

- **L173**: Not sure this separation can be done.
  *Considered and corrected.*

- **L181**: "Langmuir scaling" -> "Langmuir number" *Corrected*

- **L197-201:** It would be helpful to elaborate more on the physical meaning of this metric. The frequency of OW conditions in different seasons depends on the location? Also, OW conditions depends on the seasons?
  *The purpose of the OW_Exceedance metric is to quantify how often local forcing beneath ice (for a given grid cell) shows magnitudes typical of open-water conditions within the same season. Physically, it represents the relative occurrence of open-water-like turbulence forcing (wind, waves, or stress) under partial ice cover. The seasonal medians are computed separately for each season to account for the strong climatological variability of wind and wave conditions (e.g., stronger forcing in autumn–winter, weaker in summer). Thus, the metric inherently captures both spatial and seasonal differences in open-water occurrence, allowing comparison of under-ice conditions to seasonally typical open-water states rather than to a fixed global threshold. We have expanded the description and metrics in the section to clarify its utility.*

- **L206:** The distribution does not seem narrow to me. It ranges from 0 to 0.03 m/s? And the seasonal variability is greater than Stokes drift
  *Removed*

- **L207-208:** It's variation between seasons does not seem to be bigger than wind stress to me.
  *Section removed*

- **L212-213:** What are the discontinuities in the exceedance rates?
  *It is unclear what the reviewer is asking here.*

- **Fig 1:** What is the area of analysis in these statistics? The area shown in panels (d), (e), (f)?
  *Using sea ice concentration thresholds we separated the OW and sea ice domains and the statistics are calculated first for grid cells that SIC* $< 0.15$ *and then compare wth gridcells that SIC* $\geq 0.15$. *In the updated version this figure is removed.*

- **Fig 1b:** Maybe adjust the range of horizontal axis to reduce the empty space?
  *Considered*

- **L225**: "Asymmetry" between what?
  *Corrected*

- **L237**: Not sure the thresholds described below are physically motivated. The effects of waves on the mixing not only depend on the absolute value of Stokes drift, but also its ratio

over friction velocity (thus Langmuir number)? What additional information is provided by the distribution of surface Stokes drift as compared to the distribution of Langmuir number?
*Removed*

- **L239**: The definition of a MIZ day is confusing. At least one grid cell satisfies the MIZ condition over the whole Arctic Ocean?
*The term MIZ day was not intended to denote a domain-wide classification for a given day, but rather to describe local conditions within grid cells that fall inside the MIZ. The exceedance metric is computed per grid cell and only for time steps when that cell satisfies the sea ice condition (SIC* $> 0.15$*). Consequently, the maps in Fig.1 represent spatially resolved exceedance frequencies within ice-covered or MIZ grid cells, not all grid cells on days when any MIZ region exists in the domain. We have revised the text to clarify this definition.*

- **L240**: Why put the figure in the Appendix if it is discussed in such details here?
*Removed*

- **L255-256**: Not sure this conclusion is sufficiently supported by the analysis so far.
*Removed*

- **L259-260**: A Langmuir number of La_t = 0.4 also corresponds to strong Langmuir turbulence? It's also inconsistent the definition of mixing regime in Eq (15).
*Corrected*

- **Eq. (15):** The regime boundaries seem arbitrary. How were they determined? Are the results sensitive to the choice of these boundaries? .
*The regime thresholds are intended to be physically motivated rather than purely empirical. We adopt the specific numerical boundaries following Li et al. (2019), who used comparable regime partitions to summarize the transition from shear-dominated to wave-influenced surface-boundary-layer mixing in global forcing conditions. Our conclusions do not rely on these values as sharp "phase transitions": they provide a compact way to stratify a continuous spectrum of forcing into interpretable categories..*

- **L276-277:** Why use the number of grid cells instead of the total area? Different grid cells may have different sizes.
*Because the analysis is performed on common, fixed grid, grid cell areas are effectively uniform, and counting grid cells provides a consistent measure of regime occurrence. Moreover, our metrics are local and categorical (i.e., regime classification at each grid cell), rather than areal-integrated quantities. We have clarified this point in the revised manuscript to avoid ambiguity regarding grid representation and aggregation.*

- **L306**: Why "subgrid variability"? Isn't it the variability across neighboring grid cells?
*Corrected*

- **L325-326**: This is due to the wrong scaling of TKE dissipation?

- **L331**: “Lusing” -> “using”
  *Removed*

- **L391-400:** It might be helpful to check the partitioning between swell and wind-waves in the MIZ and their directions. Also their contribution to the Stokes drift. I’d expect the misalignment between wind and waves to be stronger in the MIZ than in the ice-free waters. But it may not significantly affect the surface Stokes drift if locally generated wind-waves are also strong.
  *In the present configuration, WAVEWATCHĨII diagnoses Stokes drift from the full, ice-attenuated wave spectrum and does not explicitly separate swell and wind-wave contributions. As a result, we cannot explicitly attribute the Stokes drift to individual wave components. However, the influence of directional misalignment, regardless of spectral origin, is explicitly accounted for through the projected Langmuir number which reduces the effective wave forcing when wind and waves are misaligned. This metric therefore captures the net dynamical impact of mixed swell and wind-wave conditions on Langmuir forcing. We note that our results are consistent with the expectation that wind–wave misalignment is more prevalent in the MIZ, while its impact on surface Stokes drift depends on the balance between incoming swell and locally generated wind waves. Exploring the spectral partitioning of Stokes drift and its directional dependence represents a natural and valuable extension of this work, and we now highlight this explicitly as a direction for future analysis.*

- **L447-448:** How was the subgrid variability captured?
  *Corrected to local scale variability*

- **Appendix A:** I think Table A1 and Figure A1 may be move in the text where they are referred to.
  *We intentionally placed Table A1 in the Supplemental materials and replaced Figure A1. These provide supporting information on model variables rather than results central to the main scientific narrative this keeping these elements in a separate file helps maintain focus and readability in the main text, while still making the relevant information readily accessible when referenced.*

**References:**

- Van Roekel, L. P., Fox-Kemper, B., Sullivan, P. P., Hamlington, P. E., & Haney, S. R. (2012). The form and orientation of Langmuir cells for misaligned winds and waves. *Journal of Geophysical Research: Oceans*, *117*(C5).

- Belcher, S. E., Grant, A. L., Hanley, K. E., Fox-Kemper, B., Van Roekel, L., Sullivan, P. P., ... & Polton, J. A. (2012). A global perspective on Langmuir turbulence in the ocean surface boundary layer. *Geophysical Research Letters*, *39*(18).

- Brenner, S., Rainville, L., Thomson, J., Cole, S., & Lee, C. (2021). Comparing observations and parameterizations of ice-ocean drag through an annual cycle across the Beaufort Sea. *Journal of Geophysical Research: Oceans*, *126*(4), e2020JC016977.

- Brenner, S., Horvat, C., Hall, P., Lo Piccolo, A., Fox-Kemper, B., Labbé, S., & Dansereau, V. (2023). Scale-dependent air-sea exchange in the polar oceans: Floe-floe and floe-flow coupling in the generation of ice-ocean boundary layer turbulence. *Geophysical Research Letters*, *50*(23), e2023GL105703.
- Lee, A., Hutchings J., Horvat, C., Tavri, A., and Pearson, B. (2025). Impact of Surface Waves on Mixing and Circulation in a Summertime Leads. Submitted in *The Cryosphere.*

## Reviewer 2 - Major Comments

### Section 3 – Model Configuration Details

**Reviewer Comment:**

More details are required in the model configuration section. (a) The authors never mention which attenuation scheme is used in WW3. (b) What is the regional domain cutoff for your regional model? (c) What are the lateral boundary conditions used for WW3? (d) You specify that atmospheric and oceanic forcing are for NeXtSIM—does WW3 receive the same forcing? (e) Please explain what "oceanic boundary conditions for NeXtSIM" means. Does this include both lateral boundary conditions and oceanic forcing? (f) You specify that "Our simulation spans the period 2018–2022 over a pan-Arctic domain with 25 km nominal resolution"—does this assume both NeXtSIM and WW3 are defined on the same mesh? (g) What is the advantage of this configuration over just analyzing sea-ice data and ERA5 wave fields? Is it simply to get Stokes drift? Did you consider using Webb (2011) to estimate Stokes drift from $H_s$? (h) The waves should also modify the ocean, and this cannot happen—can you comment on this potential impact?

**Author Response:**

We appreciate these detailed configuration questions. We answer here each of the questions:
(a) The attenuation scheme used correspond to the switch IS2 and IC2 in the WW3 model. It combines 3 attenuations processes: scattering, friction, and inelastic dissipation due to the repeated flexure of sea ice. These processes and their effects are discusse in Boutin et al. 2018. This combination of attenuation processes has been shown to provide reasonable wave-in-ice estimates for events that occurred the Barents Sea (Boutin et al., 2018), Beaufort Sea (Ardhuin et al., 2018) as well as MIZ extent consistent with observations from ICESat-2. (Boutin et al., 2022).
(b) We cut off the regional domain south of 54degN.
(c) We provide the southern lateral boundaries in WW3 with wave spectra from the global hindcast from Ifremer: Data from WAVEWATCH-III simulations, from the project «Modélisation et Analyse pour le Recherche Côtière » (MARC) https://marc.ifremer.fr, Ifremer, University of Brest, CNRS, IRD, Laboratoire d'Océanographie Physique et Spatiale (LOPS), IUEM, Brest, France.
(d) Yes, atmospheric forcings are the same (hourly ERA5). WW3 does not use any oceanic forcings (the effects of ocean currents on waves is not accounted for).
(e) neXtSIM does *not* have forced lateral boundary conditions, only forcings. At the lateral boundaries of the domain,sea ice neXtSIM will flow out as if there was no resistance and flow in as if the ice state outside the boundary was the same as that inside it (Olason et al., 2025).
(f) They do not share the exact same mesh, because neXtSIM is a Lagrangian model that uses a moving triangular mesh, while WW3 is run on a stereographic grid. However, the mesh was built to have an resolution equivalent to the grid used by WW3. As neXtSIM runs, this mesh moves and deform, but undergoes regular "local remeshing" to avoid too high deformations of the triangles and conserve the resolution. Every coupling timestep, neXtSIM interpolates the

exchanged quantities from its mesh to the exchange grid, which is the same as the one we use to run WW3.

(g) We appreciate the reviewer's suggestion and the reference to Webb (2011). While empirical relations between Hs and surface Stokes drift are effective in the open ocean, they assume fully developed, ice-free seas. In the Arctic,wave spectra is strongly attenuated and directionally filtered by sea ice. The neXtSIM–WW3 configuration provides a physically consistent treatment of these effects. Specifically, WW3 provides wave statistics (Hs, fp, directional spreading) and a bulk estimate of Stokes drift that accounts for attenuation by sea ice through parameterized energy loss. neXtSIM provides the evolving ice concentration fields that constrain wave propagation and the effective surface stress partition. ERA5 does not represent the wave attenuation in a realistic manner, it simply assumes ice is land above a certain concentration threshold. neXtSIM-WW3 is an existing dataset, where the wave attenuation in ice has been, if not demonstrated in every circumstances, at least discussed in 2 publications (Boutin et al., 2021;2022). It is also self-consistent throughout the time period (doesn't depend on observations through data assimilation).

(h) We agree that, in the present configuration, the ocean is not dynamically coupled to the wave field. The coupling is one-way through surface stress and Stokes drift forcing. Consequently, the diagnosed dissipation and Langmuir metrics represent the potential turbulent forcing rather than the fully realized oceanic response. Including an interactive ocean component would allow the wave-induced momentum and TKE to be redistributed vertically and laterally, modifying local stratification and mixed-layer depth. However, previous coupled studies (Li et al., 2019) show that these feedbacks mainly alter the magnitude of $\varepsilon$ while preserving the spatial patterns of Langmuir forcing. We have clarified this limitation in Section 5 and now explicitly describe our diagnostics as representing Langmuir turbulence potential rather than resolved mixing.
In addition, each section in the manuscript has been expanded to include this information.

### Section 3.1 – Momentum Flux Formulation

**Reviewer Comment:**

Using an absolute-wind formulation may overestimate the momentum flux into the ocean since momentum loss due to wave generation is neglected. What is the potential impact of this choice? It may be possible to estimate this from GLORYS data.

**Author Response:**

We agree that using the bulk absolute-wind formulation ($\tau_{ao} = \rho_a C_{ao}|u_a|u_a$) neglects the portion of atmospheric momentum transferred to wave growth, which may slightly overestimate the effective ocean stress. In our coupled setup, the sea-state dependence of surface drag is not represented because WAVEWATCH III and neXtSIM are not dynamically coupled. Thus, the stress applied to the ocean represents an upper bound on the true momentum flux into the mixed layer. This assumption primarily affects the absolute magnitude of $u_*$ and $\varepsilon_{\text{shear}}$ during active wave-growth conditions, but it does not alter the spatial patterns or relative Langmuir enhancements analyzed here. The effect is minimal under high–sea-ice conditions where wave growth is suppressed. We have clarified this limitation in the revised text and noted that future coupled implementations could incorporate wave-modified stress terms or evaluate corrections using sea-state diagnostics from GLORYS.

### Section 3.2 – Definition of $\alpha_L$

**Reviewer Comment:**

$\alpha_L$ is not introduced well. I suggest 'dynamic orientation of the Langmuir cells relative to the wind direction ($\alpha_L$)' or something similar. I understand not including $\alpha_L$, but an $\alpha_{LOW}$ is proposed at the end of Van Roekel et al 2012 that could be used here. Given the overestimation with $\theta_{ww}$ produces a muted response, I would expect $\alpha_{LOW}$ to be less as well. But it would be useful to discuss this better.

**Author Response:**

We have now clarified that $\alpha_L$ represents the dynamic orientation of the dominant Langmuir cells relative to the wind direction, which modulates the effective wave–wind coupling strength. Following the suggestion, we incorporated $\alpha_{\mathrm{LOW}}$ from Van Roekel et al. (2012) to test the effect of Langmuir cell reorientation under wave–wind misalignment. In our revised analysis, $\alpha_{\mathrm{LOW}}$ is defined as the empirical correction angle that minimizes the misalignment between the Stokes drift and the wind stress directions, such that $\theta_{ww} - \alpha_{\mathrm{LOW}}$ represents the effective alignment of the Langmuir circulation with the mean flow. We compute the adjusted cosine alignment $|\cos(\theta_{ww} - \alpha_{\mathrm{LOW}})|$ and show that it produces a more physically consistent distribution compared to $|\cos(\theta_{ww})|$, particularly in high–sea ice or strongly misaligned regimes.

### Section 3.2.1 – Turbulence Scalings and TKE Relations

**Reviewer Comment:**

(a) Use of the scaling- The scalings from Van Roekel et al 2012 were derived from destabilizing LES conditions primarily. I'm not aware of any work examining LT and the scaling in stabilizing conditions. Therefore, it's not clear how applicable the VKE scalings, LaT etc… are to the arctic. (b) The relationships between Eqns (9–11) are unclear—what is the wave-driven TKE contribution? If Eqn (9) is total dissipation, Eqns (9) and (11) seem redundant. (c) It would be helpful to explicitly state that these dissipation relationships emerge from a vertically integrated turbulence kinetic energy budget.

**Author Response:**

(a) Indeed, the turbulence scalings of Van Roekel et al 2012 were derived from idealized large-eddy simulations under destabilizing surface forcing, where convective mixing reinforces Langmuir cell overturning. In contrast, Arctic mixed layers are often stably stratified due to ice melt and freshwater input, which suppresses overturning and modifies the partition between shear-driven and Langmuir-driven turbulence. We have added a new paragraph clarifying that our use of the Van Roekel scaling is intended as a diagnostic framework for relative enhancement potential, not as a direct representation of absolute dissipation in stratified conditions. We also note that the resulting Langmuir number and enhancement factors should be interpreted as indicators of the potential for Langmuir forcing, with the effective magnitude likely reduced under stable Arctic conditions. Also, we have expanded our discussion to mention that ongoing studies (e.g., Lee et al. 2025 (preprint);Brenner et al. 2023) are working to resolve Langmuir turbulence under partially ice-covered and weakly stratified regimes, providing a pathway toward stratification-aware parameterizations.

(b) In the revised manuscript, we clarify that we do not interpret wave-driven (Langmuir) turbulence as a separately identifiable contribution to the dissipation budget. Instead, equations in 2.4.1 are intended to describe diagnostic scalings within a vertically integrated TKE framework, rather than additive components. To avoid confusion, we have reformulated the section and the Langmuir enhancement factor as a diagnostic excess relative to this shear-only reference, and we no longer refer to a distinct wave-driven TKE contribution. This revision removes any implication of linear additivity or separable dissipation pathways and aligns the formulation with the nonlinear interaction of wind-, wave-, and buoyancy-driven turbulence in the mixed layer.

(c) We agree with the reviewer and have added explicit clarification in the revised manuscript. All dissipation relationships used in this study arise from the *vertically integrated turbulence kinetic energy budget* of the ocean surface boundary layer. In this framework, $\varepsilon_{\text{shear}} = u_*^3/H_{ML}$ represents the depth-averaged dissipation associated with shear production, while the Langmuir-enhancement term reflects additional production and turbulent transport driven by Stokes drift. This clarification is now stated directly in the Methods section.

**Section 4 – Results and Interpretation**

**Reviewer Comment:**

(a) Figure 1: Why does Hs exceed but not Us(0)? I usually expect Hs and Us to be related. Is there any way to calculate these exceedance rate from observations? How well does the WW3-NeXTSIM coupled model reproduce observed statistics?

(b)Wouldn't the exceedance statistic (eqn 14) be biased since the OW cells are systematically located at lower latitudes, and experience different forcings than ice covered cells higher latitude cells?

**Author Response:**

(a) In response to the reviewer's comment, we have revised this section and now present a different figure that directly supports the associated discussion. We nonetheless address the physical interpretation raised here. Although both $u_{s(0)}$ and $h_s$ are related to wave energy, they respond differently to ice-induced spectral attenuation. The surface Stokes drift $u_{s(0)}$ is controlled primarily by the high-frequency tail of the wave spectrum, which is strongly damped under partial ice cover. In contrast, significant wave height $h_s$ integrates energy over the full spectrum and is therefore less sensitive to the loss of short waves. As a result, $h_s$ can occasionally exceed open-water (OW) median values near the ice edge, whereas $u_{s(0)}$ rarely does. At present, comparable exceedance statistics cannot be derived directly from observations, as continuous measurements of $u_{s(0)}$ and $u_*$ under sea ice are not available. Nevertheless, the simulated spatial patterns and seasonal variability of $h_s$ in the WW3–neXtSIM framework are consistent with satellite altimeter climatologies (Arduin et al., 2018, Stopa et al., 2018) providing confidence in the realism of the modeled wave statistics.

(b) We agree that OW grid cells are preferentially located at lower latitudes and therefore experience different wind and wave forcing than ice-covered regions. The exceedance metric in Eq. (16) is not intended as an unbiased comparison of absolute forcing magnitudes across latitudes, but rather as a *relative diagnostic* of how frequently ice-covered conditions approach or exceed the typical seasonal OW baseline. By normalizing against the OW seasonal median, the metric highlights regions where forcing beneath ice becomes dynamically comparable to open-water conditions. We now explicitly clarify this interpretation in the revised manuscript and emphasize that the exceedance statistic should be viewed as a relative, not absolute, measure.

### Section 4.3 – Mixing Interpretation and Stratification

**Reviewer Comment:**

The conclusion at the start of section 4.3 doesn't seem supported well by evidence. This could be fixed by specifying " in the MIZ" as opposed to "in the Arctic". The current phrase suggests this is pan-Arctic.

**Author Response:**

We agree that the current wording could be misinterpreted as implying a pan-Arctic enhancement of dissipation, whereas the evidence supports this conclusion primarily within the marginal ice zone. We have revised the opening sentence.

**Reviewer Comment:**

L 355 - The text here is speculative. Have you examined ocean stratification over this period? This would be a useful compliment to your analysis. Even for your mixing discussion, you have more mixing energy, but there is no guarantee there is more mixing without examining stratification.

**Author Response:**

Our analysis focuses on the energetic availability for turbulence within the mixed layer, as diagnosed from a vertically integrated TKE framework, rather than on realized vertical mixing rates or entrainment efficiency. Direct examination of ocean stratification was not possible within the present framework, as the coupled WW3–neXtSIM configuration does not include prognostic upper-ocean stratification or buoyancy profiles. We have therefore revised the text to avoid speculative language and now explicitly state that our results describe conditions favorable for enhanced turbulent activity, rather than definitive increases in mixing or vertical exchange. We agree that combining energetic diagnostics with stratification metrics (e.g., mixed-layer depth variability or buoyancy frequency) would provide a valuable complement to this analysis, and we now note this as an important direction for future work using fully coupled ocean–ice–wave models or observationally constrained reanalyses.

**Section 4.4 – Kernel and Subgrid Variability**

**Reviewer Comment:**

Please clarify what the connection between the kernel analysis is and subgrid variability. It's a measure of local heterogeneity but using a spatial kernel doesn't say anything about variability below the model grid scale. There certainly is a lot of spatial variability, but this doesn't mean it's subgrid

**Author Response:**

We agree and have replaced subgrid with local spatial variability throughout. The text now clarifies that kernel analysis measures horizontal gradients in modeled fields, not unresolved subgrid turbulence.

## Minor Comments

- **L58 & L73:** Replace "WaveWatch III" with the official name "WAVEWATCH III".
  *Corrected throughout.*
- **L75:** Remove "fully."
  *Corrected.*
- **L76:** WW3 already defined.
  *Removed redundancy.*
- **L143:**tilda is above the 2 in 25km?
  *Fixed.*
- **Figure 2:** caption the Median 15% and 80% SIC contours are overlaid in black and blue. Based on the image, the 15% SIC is actually BLUE and the 80% line is black - make sure the listed order of the contours match
  *Corrected caption and figure.*
- **L239:** the concept of a MIZ day is confusing... please clarify if you mean that a "MIZ day" is defined as when ANY grid cell in the entire domain on a given day is between 15-80% SIC. If so, I would expect every 'day' in the time series to be classified as a MIZ day, which would make this metric essentially meaningless. Is there any spatial requirement for a grid cell to be located within the MIZ in this definition? In other words, please clarify if these exceedance values (in Fig A) are only valid for grid cells within the MIZ, or ALL grid cells on a day where any MIZ cell is present within the domain (which would be 100% of the time).
  *The term MIZ day was not intended to denote a domain-wide classification for a given day, but rather to describe local conditions within grid cells that fall inside the marginal ice zone. The exceedance metric (Eq. 16) is computed per grid cell and only for time steps when that cell satisfies the sea-ice condition (SIC* $\geq 0.15$*). Consequently, the maps in Fig.1 represent spatially resolved exceedance frequencies within ice-covered grid cells, not all grid cells. We have revised the text to clarify this definition.*

- **Figure 3:** results - what is the definition of persistence? ”
  *Added definition as the consecutive-day duration of a given LT regime.*

- **L330–332:** Add missing reference and fix equation/typo.
  *Added citation and corrected inline equation.*

- **L375–376:** Reiterate overestimation when using $\theta_{ww}$.
  *Noted*

- **L391–397:** Clarify difference between analyses 1 and 2 and mention Van Roekel et al. (2012) Fig 16 confirmation.
  *Expanded discussion and included direct comparison.*

- **L402:** Remove “comprehensive.”
  *Corrected.*

- **L420 & L447:** Replace “subgrid” with “fine-scale.”
  *Corrected throughout.*

- **L438–440:** - what are the implications of overestimates from Langmuir diagnostics? Are you making connections to things like the KPP enhancement factor being based on LaT? Couldn’t this capture the regime if Stokes drift is dynamic (say from WW3?)?

*While dynamic Stokes drift from WAVEWATCH III improves Langmuir forcing estimates, our results show that bulk* $La_t$*-based diagnostics still overestimate Langmuir activity under sea ice by assuming persistent enhancement. In the Arctic, Langmuir turbulence is intermittent, often coexists with shear, and is strongly modulated by wind–wave misalignment and wave attenuation by ice. These effects are not captured by scalar* $La_t$*-based enhancement alone, motivating regime-aware parameterizations. .*

- **L452:** Clarify “integrating directional wave spectra.”
  *Clarified*

- **L452:** “Fully coupled” implies inclusion of an active atmosphere.
  *Revised*

## References

Ardhuin, F., Boutin, G., Stopa, J., Girard-Ardhuin, F., Melsheimer, C., Thomson, J., Kohout, A., Doble, M., & Wadhams, P. (2018). Wave Attenuation Through an Arctic Marginal Ice Zone on 12 October 2015: 2. Numerical Modeling of Waves and Associated Ice Breakup. *Journal of Geophysical Research: Oceans*, *123*(8), 5652–5668. https://doi.org/10.1002/2018JC013784

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## Reviewer 3 - Major Comments

### 1. Model Fidelity

**Reviewer Comment:**

While the manuscript cites earlier studies validating this model, some evaluation specific to the present application is needed. In particular, comparisons of modeled winds, shear, and wave fields (especially short waves, which strongly influence Stokes drift) would provide important context. Because the LT parameterizations are sensitive to these inputs, even brief error estimates or uncertainty ranges would help clarify the robustness of the conclusions. It would also be useful to discuss how well the model captures heterogeneous ice concentration features (e.g., leads), which can locally enhance Stokes processes.

**Author Response:**

We thank the reviewer for this constructive suggestion. In the revised manuscript, we have added a dedicated subsection titled *"Evaluation of model inputs relevant to LT diagnostics"* (Section 2.2). This new section provides targeted evaluation of the model components most relevant for Langmuir turbulence. First, we include a comparison between the model's 10-m winds and CCMP v3.1 over 2018–2022, which reveals a consistent mean-state bias while demonstrating that synoptic variability is well captured. This directly constrains uncertainties in the modeled surface shear $u_*$. Second, we summarize published validation of the neXtSIM representation of sea ice extent, deformation, and lead structure, including quantitative skill metrics (RMSE and mean bias) from (Olasson et al., 2025), which show that the model shows reasonable agreement with observations. Third, we highlight established validation of the WW3 IS2+IC2 attenuation scheme (Ardhuin et al., 2018; Boutin et al., 2022), which reproduces observed short-wave decay and realistic MIZ extent, providing context for uncertainties in the modeled Stokes drift $u_{s(0)}$.These additions provide a concise assessment of the atmospheric, sea ice, and wave components that are used for our LT diagnostics.

### 2. Key Conclusions and Metrics

**Reviewer Comment:**

The primary focus on regime classification raises questions of utility. Why is the frequency of transitions between regimes the most relevant measure? Should this instead be linked to event duration or intensity?

**Author Response:**

We appreciate the reviewer's question and agree that the utility of a regime-based framework depends on how regime variability is quantified. Our use of transition frequency is motivated by its role as a compact, dimensionless diagnostic of *regime stability*: it directly measures how often the balance between wind-, wave-, and ice-mediated forcing reorganizes at a given location, independent of absolute forcing magnitude. As such, it provides a spatially consistent, first-order indicator of whether local mixing conditions are persistent or intermittently forced.

We agree that transition frequency alone does not fully characterize regime behavior, particularly with respect to persistence and intensity. To address this, we have expanded the analysis and revised Figure 3 to explicitly link transition frequency to regime duration. Panel (d) now relates the mean time between transitions to the longest continuous duration of a single regime within MIZ grid cells, demonstrating a strong, highly non-linear relationship: regime persistence collapses rapidly once transitions become frequent, with a characteristic timescale of order several weeks. This shows that transition frequency is not an abstract metric, but a direct proxy for the loss of sustained forcing regimes. In addition, the revised text clarifies that transition frequency is used here not as a measure of event intensity, but as a diagnostic of intrinsic forcing instability.

**Reviewer Comment:**

The discussion introduces two compelling applications: (i) how wave-driven forcing may evolve in a changing climate, and (ii) implications for tracer transport and stratification. These questions seem ideally suited for this model framework. Even a preliminary analysis—for instance, a future-scenario run or a waves-on vs. waves-off experiment—would help demonstrate the broader utility of the work. Additionally, the statement that “wind–wave alignment strongly influences LT-driven mixing” may be overstated given the results in Figure 7. A more quantitative phrasing (e.g., “up to X% effect in the MIZ”) would provide a more measured conclusion.

**Author Response:**

We thank the reviewer for recognizing the broader applicability of our modeling framework and for these constructive suggestions. We agree that extensions such as future-scenario simulations or waves-on vs. waves-off sensitivity experiments would provide valuable demonstrations of how wave-driven forcing may evolve under changing ice and wind conditions. These experiments are well aligned with our framework and are currently being planned as part of a follow-up study. In the present work, our focus was to establish and validate a physically grounded diagnostic for characterizing the balance between wind- and wave-driven mixing across heterogeneous sea ice conditions. We have clarified this scope in the revised text and added statements outlining how the same approach can be applied to future-scenario or process-specific analyses in subsequent work. Regarding the statement that “wind–wave alignment strongly influences LT-driven mixing,” we agree and have revised it to a more measured description.

## 3. Domain of Analysis

**Reviewer Comment:**

The spatial and seasonal definition of the analysis domain needs to be clarified. In maps, regions outside the study area should be removed or masked. Presenting this earlier in the results (e.g., with characteristic winds, peak wave heights, or other basic descriptors) would give readers helpful context.

**Author Response:**

We have revised Section 2.1 and Figure 1 caption to more clearly define the analysis domain (70–85°N, 45°W–180°E).

### 4. Presentation and Scope

**Reviewer Comment:**

The manuscript currently introduces more metrics than are fully justified by the conclusions. Several variables appear in the methods but are not explored in the results, while additional metrics are introduced later in the analysis. A more streamlined focus on the most relevant parameters would strengthen the narrative.

**Author Response:**

We agree that focusing on a smaller set of key diagnostics improves clarity and coherence. In the revised manuscript, we have streamlined the presentation by emphasizing the parameters most directly linked to our conclusions, and moved secondary metrics to the supplementary material.

## Minor Comments

- **Line 34:** Out of curiosity, have you also applied this model in the Southern Ocean? Given its energetic wave climate, it may be an equally or more interesting test case .
  *The coupled setup has not yet been applied to the Southern Ocean, but we plan to extend the analysis there.*
- **Figures/Visuals:**
  - **Figure 1:** The exceedance plots, especially panels (e, f), are difficult to interpret and appear saturated. Consider simplifying (e.g., show only u*), or adopt a different approach to highlight relative values.
    *We have considered a different visualization, as the information provided from this metric is useful.*
  - **Figure 2:** The averaging domain is unclear. Does it include the entire region shown? Also, labels such as "wave-dominated" and "shear-dominated" (introduced later, L268) could be used here. Color contrast between LT-active and mixed regimes should be improved.
    *Yes, it is the entire region in the plot. Comment considered and figure is updated.*
  - **Figure 3:** The line label in panel (a) is unreadable. In panel (b), clarify the domain for SIC; the 20% threshold seems surprisingly low.
    *Label enlarged.*
  - **Figure 4:** The increase in La_T at moderate SIC (likely due to larger fetch) should be noted in the text.
    *Considered.*

  - **Figure 6:** SIC would be clearer as a black line rather than shading. Caption should clarify that VKE refers to the upper ocean.
    *Updated figure.*

- **Line 330:** Reference missing.
  *Corrected.*

- **Lines 364–365:** Statement "confirms LT effects" too strong.
  *Rephrased to "suggests a strong association between LT indicators and enhanced vertical mixing."*

**References**

Rogers, W. E., Dykes, J. D., Wang, D., Carroll, S. N., & Watson, K. (2012). *Validation test report for WAVEWATCH III* (No. NRLMR7320129425).

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