Response to Reviewers - Second Round

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

Authors: Aikaterini Tavri et al.

General Comments

Reviewer 1

Reviewer Comment:

I think the clarify to this revised version is much improved compared to the first version. However, I think another round of careful revision is necessary. In particular, I think the authors should acknowledge the uncertainties in the scalings of vertical velocity variance and TKE dissipation based on idealized LES when describing the results and make sure not to over-interpret the results or overstate the conclusions. Some examples can be found in my specific comments below. Some of the conclusions on how external forcing affects the LT-enhanced mixing (e.g., the effect of misaligned wind and waves) are readily expected from the scalings used in this study. Rather than emphasizing too much on those effects themselves, I think it would be more helpful to focus on assessing how frequent these scenarios occur in the Arctic and under what conditions.

Author Response:

We thank the reviewer for their additional feedback. Below we provide responses to all comments. We have carefully addressed the issues raised and we have revised the manuscript to more clearly acknowledge these limitations and avoid over-interpretation of the parameterized responses.

Specific Comments

L26: "generate" -> "enhance"? *Updated*
L30-32: There are already a few implementations of LT parameterizations in ocean general circulation models, e.g., Fan and Griffies, 2014, Li et al., 2016, Ali et al., 2019
Updated
L53: It might be helpful to define "LT mixing potential" here. Or, since "LT potential" is defined in the next paragraph, maybe use a more descriptive term such as "the potential of LT in enhancing the mixing". *Considered*
L58 and throughout the manuscript: "LT potential" -> "LT mixing potential"? *Updated*
L59: What do the authors mean by "independent of oceanic dynamical adjustment"? *Clarified*
L60: "LT mixing-supporting conditions" -> something like "conditions that favor the formation of LT?" *Updated*
L61-64: Probably emphasize on the effects of misaligned wind and waves but leave the details on the metrics to the methods section? Otherwise it might be necessary to define both turbulent

Langmuir number and the projected turbulent Langmuir number here.
Updated
L64: It is unclear what do the authors mean by “surface turbulent mixing regimes” before introducing how different regimes are defined.
Corrected
L81: What do the authors mean by “WW3 exchange grid”? Is it the grid WW3 is running on? Or is it a different grid?

We have rewritten this part for clarity. To provide context to the reviewer, the neXtSIM uses an unstructured moving mesh, which is not convenient to exchange the quantities through the OASIS coupler (its interpolation libraries do not cover this case), we interpolate the exchanged quantities from the mesh to a fixed Eulerian grid within neXtSIM, which we call the exchange grid. In this case, we took this grid to be the same as the grid we run WW3 on, in order to limit the number of interpolations (the coupler is just exchanging the variables, no interpolation). More details can be found here: Ólason, E., Boutin, G., Williams, T., Korosov, A., Regan, H., Rheinlænder, J., Rampal, P., Flocco, D., Samaké, A., Davy, R., Spain, T., & Chua, S. (2025). The next generation sea-ice model neXtSIM, version 2. *EGUsphere*, 1–33. https://doi.org/10.5194/egusphere-2024-3521
L84-85: Rephrase? Is the ice properties of the inflow prescribed at the boundary? *Updated*
L94: “, surface waves” -> “, and surface waves” *Corrected*
L99-101: Not sure how this is analogous to Li et al. (2019)? *Updated*
L150: What do the authors mean by “contribute momentum”? *Clarrified*
L158: The phrase “two primary dynamical inputs” is vague. *Rewritten*
L161: McWilliams et al., 1997 is more appropriate for the definition of turbulent Langmuir number. *Considered*
L168: Harcourt and D’Asaro 2008 is probably more appropriate here. *Considered*
L176: The phrase “dynamic orientation angle” is vague. Why “dynamic”? *Removed*
L182: Is a horizontal resolution of 25 km sufficient to resolve the MIZ?
This line refers specifically to the inability of the 25 km model resolution to resolve the vertical structure of the Lagrangian shear required for direct evaluation of the Langmuir cell orientation angle, a_L. This is independent of the model’s ability to represent the horizontal structure of the MIZ. We have rewritten this section in the manuscript for clarification. L183: LOW refers to law of the wall, not low order waves. The Eulerian shear is approximated by the law of the wall in Van Roekel et al., 2012. *Thank you for pointing this typo out, it is now corrected.*
L190-192: It also reduces the projected wind-driven shear in the Langmuir cell direction, as the authors mentioned above in the discussion of (7). I didn’t follow the authors’ argument here to substitute \alpha_LOW} for \alpha_L in (7). Essentially (9) is a practical way to estimate the projected Langmuir number without actually running LES to resolve the Eulerian shear. But the way the authors put it here make it sound like that there is physical reasons to use \alpha_LOW} rather than \alpha_L.

We agree that the wording was unclear and have revised the section for clarification.

L216-217: So these metrics really depend on the open-ocean benchmark, which depends on the simulation domain and the forcing conditions? *Yes, the metrics depend on the OW benchmark by design; they provide a relative measure of when ice-covered regions experience* *open-ocean–like forcing, rather than an absolute threshold.* L218-219: Didn't see how these numbers were determined in Li et al., 2019.

The regime thresholds used in Eq. (12) are not directly derived from a single analytical criterion, but instead represent a one-dimensional approximation of the two-parameter regime space identified in LES studies (e.g., Van Roekel et al., 2012; Li et al., 2019). As illustrated in Fig. 11 of Li et al. (2019), mixing regimes are formally defined in the joint (La_t, h/L_L) space, where Langmuir-, shear-, and convection-dominated turbulence occupy distinct regions. Because we cannot explicitly resolve buoyancy forcing or the stability parameter h/L_L, we adopt a reduced classification based solely on La_t. The threshold values are inferred from the clustering of regime boundaries in LES-derived parameter space (e.g., Li et al., 2019, Fig. 11), where $La_t \sim \mathcal{O}(0.4)$ marks the transition into Langmuir-dominated turbulence and $La_t \sim \mathcal{O}(1)$ indicates shear-dominated conditions. We emphasize that this classification collapses a two-dimensional regime diagram into a one-dimensional diagnostic and should therefore be interpreted as a qualitative indicator of the dominant mixing mechanism.

L229: It would be helpful to make it clear somewhere in this section that the area of each grid cell is the same so that statistics based on the number of grid cells is equivalent to statistics based on the area.

Considered

L293: Any comments on why it's lowest in summer? Is it because the higher OW benchmark? Is it due to wind stress or Stokes drift?

We have updated this section in the manuscript and included our clarifications in lines 290 to 300.

L300: So the reason the exceedance is lowest in summer is that waves are even lower (strongly suppressed by sea ice) or is that wind exceedance is low?

The comment is addressed in the manuscript in lines 294 to 296.

L305-306: Any comments on which is more important? *Answered above and demonstrated in the updated manuscript.*

L307: Please define "Stokes transport".

updated L308-310: It would probably be helpful to comment on the differences of the OW conditions between seasons.

Considered. L327-328: This statement is misleading. The reason it appears to penetrate farther beneath the ice is because the ice fraction is lower and the damping of waves is smaller, not necessarily because the waves are stronger? Waves are probably stronger in the winter? *We agree that the apparent extension of reduced La_t in summer and fall reflects reduced ice concentration and weaker wave attenuation, rather than stronger wave forcing. We have revised the text accordingly.*

Fig 2: Dark blue and black lines look very similar. Maybe use the line styles to distinguish the two?

Corrected in the figure caption. Fig 3: Swap panel labels (b) and (c)? Inconsistent with the

caption and the text.
Corrected
L341-342: How to understand the moderate regimes transition frequency (green colors) in regions of the consolidated interior pack ice?
Moderate transition frequencies within the consolidated ice indicate intermittent variability in the mixing forcing. Episodic openings (e.g., leads) can permit limited fetch and short wind-wave generation, temporarily enhancing Stokes drift. In addition, many grid cells reside near regime thresholds ($La_t \sim O(1)$), so even small fluctuations in forcing can trigger repeated transitions. We have clarified this in the manuscript.

L355-358: I didn't follow these statements.
Rewritten for clarity. L365-367: But why specifically around certain values of SIC?
Updated in the manuscript.
L378: Why "at scales smaller than the model grid"? These are heterogeneity at resolved scale.
Updated in the manuscript.
L378-380: Why? I don't see how this conclusion on the boundary layer parameterization is drawn from the results here. *Corrected*

Fig. 4: Hard to see the white lines in the plot, especially the line corresponding to La_t = 0.43. *Updated* L388: \log_10} *Corrected*
L405-406: What do the authors mean by "the efficiency with which that forcing is converted into vertical motions"? *Clarified and updated in the manuscript*

L411: Are the "two dynamically contrasting locations" two selected grid points or two selected regions? *Each region represented a 3x3 kernel mean. This section has been omitted from the analysis as it seemed redundant and more targeted plots have been added.*

Fig 6: I'm not sure how useful this metric of \Delta La is other than showing that there is a difference due to wind-wave misalignment. Since La increases as the magnitude of Stokes drift decreases, large La means small Stokes drift. And differences between two large values of La do not have much physical meaning as both indicates small Stokes drift. It probably makes more sense to show the ratio of the two La, rather than the difference between the two. *Thank you for this comment. We considered and changed this section.*

L419: What do the authors mean by "geometric suppression"? *Clarified and updated in the manuscript*

L420-421: They do not seem to occur at the same time — elevated VKE seems to occur earlier. *Removed and corrected*
L429-430: I don't understand this sentence. *Explained and refined.*
L441: Apparently there are cases with R_LT}>1 shown by the gray dots in Figure 7. *Removed.*

Fig 7: So what does it mean if R_LT}>1? Also, what do the authors mean by "geometric control"? In addition, I find the discussion around Fig 7 very confusing. If I understand it correctly, R_LT} is estimated from the two scalings of VKE with and without accounting for the effect of wind-wave misalignment using (20). Most of the conclusions shown here is directly expected due to the factor of \cos2̂(\alpha_LOW}) in (20), right? Thank you for this comment. We agree that the interpretation of R_LT} and the associated discussion were not sufficiently clear. As the analysis was largely based on the cosine dependence already present in the formulation, we have removed this section to avoid redundancy and potential confusion.

L452-454: To show this, I think it is more helpful to show the distribution of wind-wave misalignment \theta_ww} and perhaps also \alpha_LOW}, rather than R_LT}? The relations between R_LT} and \alpha_LOW} and \cos2̂(\alpha_LOW}) as shown in Figure. 7 are largely expected from (20). But Figure 7 does not show how frequent large misalignment between wind and waves occurs in the Arctic. *Considered and replaced in the manuscript*

L510-512: This certainly requires additional support. Maybe focusing on the conditions for LT rather than LT itself. *Corrected*

L530-532: I don't understand how this is conclusion supported by the results in this study. The intermittency and regime dependence of potential Langmuir turbulence is assessed in this study using scalings that based on time-mean forcing, right? *Corrected.*

L532: What do the authors mean by "regime-aware"? *Clarified in the manuscript*

L534-535: Again, I don't see how this conclusion is supported by the results of this study either. It is known from the scaling (20), and more precisely the LESs from which this scaling is derived, that wind-wave misalignment reduces the effect of LT on enhancing the mixing. I think what this study can provide is how frequently such conditions occur in the Arctic? *Corrected and added the suggested figure.*

L541: How well is the MIZ resolved with a horizontal resolution of 25 km?

With respect to horizontal resolution, a grid spacing of 25 km does not resolve fine-scale floe distributions or narrow leads within the MIZ. However, it is sufficient to capture the large scale spatial extent, seasonal evolution, and regional variability of the MIZ, which typically spans O(10–100 km) in width. The neXtSIM–WW3 framework has been shown to reproduce realistic ice-edge position, MIZ extent, and wave–ice interaction patterns at this resolution (Boutin et al., 2022; Ólason et al., 2025), supporting its suitability for basin-scale analyses. We therefore interpret our results as representative of the large-scale organization and frequency of LT-supporting conditions within the MIZ. This has been clarified in the revised manuscript.

References

Boutin, G., Williams, T., Horvat, C., & Brodeau, L. (2022). Modelling the Arctic wave-affected marginal ice zone. *Philosophical Transactions: Mathematical, Physical and Engineering Sciences*, *380*(2235), 1-17.

Ólason, E., Boutin, G., Williams, T., Korosov, A., Regan, H., Rheinlænder, J., ... & Chua, S. (2025). The next generation sea-ice model neXtSIM, version 2. *EGUsphere*, *2025*, 1-33.

Li, Q., Reichl, B. G., Fox-Kemper, B., Adcroft, A. J., Belcher, S. E., Danabasoglu, G., ... & Zheng, Z. (2019). Comparing ocean surface boundary vertical mixing schemes including Langmuir turbulence. *Journal of Advances in Modeling Earth Systems*, *11*(11), 3545-3592.

Reviewer 2

Reviewer Comment:

This revision has answered most of my concerns and is vastly improved from the previous version. There is some very nice new analysis that sets the directions for future work. I only have a few relatively minor concerns for the authors and recommend minor revisions.

Author Response:

We thank the reviewer for their additional feedback. Below we provide responses to all comments. We have carefully addressed the issues raised and we have revised the manuscript to clarfy the requested points.

Minor Comments

L182 - why does the 25km resolution limit the computation of the lagrangian shear? Is this an argument about patchiness in the wave field that is not resolved?
Yes, exactly that's what it is. At 25 km resolution, Lagrangian shear is derived from spatially averaged wind and wave fields, which smooth small scale variability in wave direction and attenuation.

L192 - extra we – 'we we substitute'
Corrected

Caption of Figure 4 - make consistent, don't use Left/right, but (a) and (b)
Corrected

L378 - I don't think this is supported. You can't say anything about scales smaller than the model grid here, at least if you are assuming 25km grid in your analysis. If you are thinking for a CMIP class model at 100km this is fine.
Corrected

L384 - specify median mechanical dissipation calculated with Equation (22)
Corrected

L388 - P90 is not defined, I assume this is exceedance of the 90% of the distribution? Also an issue with subscripting on $\log_{10}$
Corrected and defined in the manuscript

Figure 6 - What are the red lines in (a) and panel c? I'm guessing $\Delta_{L}a_{t}butthisisnotdefined.Alsowhyisthedel$
$1, thisseemstoimplyLa_{p}roj < 0whichisnotpossible.I'mmissingsomething.$
The red lines were showing the Langmuir misalignment $\Delta La = La_{\mathrm{proj}} - La_{t}$, *where* La_{t} *is the wind-aligned tu*
$424 - wouldbeusefultoreferencespecificpanelsinFigure6$
Corrected

L477 - I'm not convinced SIC is the primary regulator. From your text, I would expect sea ice thickness to be the leading factor. You suggest this in L481-482
Expanded in the updated manuscript.

L509-510 - Noting that this also supports your argument about episodic dissipation, you'd expect more misalignment for episodic events. *Considered and corrected.*