

Response to Reviewer 2 for Manuscript egusphere-2026-2292

Wave-phase-aware model implementation in WRF-LES v3.8.1 for turbulence-resolving simulations of turbulent flow over monochromatic waves

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Response to Reviewer 2

General Comment

In the manuscript, the authors implement in WRF and conduct an assessment of three models for representing waves in marine atmospheric boundary layer (MABL) simulations: a wave-phase-resolved model and two wave-phase-aware models (one from Princeton and one from Johns Hopkins). The implementation of these models in WRF is an invaluable contribution to the community. However, the manuscript in its current form falls somewhat short on analysis and assessment, and further efforts are required in revision, both in terms of model formulation, presentation, and simulation set-up.

Response to General Comment: We thank the reviewer for the positive assessment and constructive comments.

We have substantially revised the manuscript to strengthen the model formulation, presentation, numerical setup, and quantitative assessment. The three approaches are now described using consistent notation and a common surface stress framework, and the numerical implementation in WRF has been clarified. We have also reorganized and enlarged the key figures, added quantitative comparisons, and revised the discussion to more clearly identify the differences and limitations of each approach. The same outer Runge–Kutta time step is now used among the three models for each corresponding wave condition.

Following the terminology of [Ayala et al. \(2024\)](#), WPM has been renamed MOSD throughout the manuscript and source code. Additional code checks also identified minor numerical robustness issues in WDM and a correction to the velocity sampling height used by MOSD. These issues were corrected, the relevant simulations were rerun, and the updated source code is provided with the revised submission.

The specific revisions are described in our responses below.

Comment 1

The abstract is light on drawing any conclusions regarding comparisons between the two phase-aware models. The results indicate that they are similar, and this would be an important point to note.

Response 1: We thank the reviewer for this suggestion.

We agree that the comparison between the two wave-phase-aware models should be stated more clearly in the abstract. We have revised the abstract to note that WDM and MOSD exhibit the same general dependence of the mean flow response on wave regime, while the magnitudes of their deviations from MOW differ. We have also added quantitative ranges for these deviations and clarified that both models capture the reversal of wave-induced momentum transfer at sufficiently large wave age but underestimate the wave-induced velocity fluctuations relative to MOW.

Comment 2

The notation in Eq. 2 is confusing with the use of overbars. In the paragraph above, the authors state that this corresponds only averaging, which is clearly not the case for the subfilter stress tensor.

Response 2: We thank the reviewer for identifying this ambiguity.

We agree that the overbar was used inconsistently to denote both LES filtering and temporal averaging. We have therefore removed the overbar notation and now define the deviatoric component of the SFS stress tensor inline. We have also clarified that all resolved flow variables are LES-filtered at the grid scale Δ , while $\widehat{(\cdot)}$, $\overline{(\cdot)}$, and $\langle \cdot \rangle$ denote test filtering at 2Δ , temporal averaging, and horizontal spatial averaging, respectively.

Comment 3

The formulations of the models are difficult to follow due to differences in the notation. Looking at the references, the authors seem to use the notation from the original works, but it must be presented here in a consistent unified manner (e.g., relative velocity is different definitions and symbols).

Response 3: We thank the reviewer for this suggestion.

We agree that retaining the notation of the original studies made direct comparison difficult. We have therefore reorganized Section 2.2 using a consistent notation across the three approaches. The surface-relative airflow velocity for MOW is now denoted by $\mathbf{u}_{r,s} = \mathbf{u} - \mathbf{u}_s$ (Equation (5)), whereas the horizontal airflow velocity in the wave-following frame is denoted by $\mathbf{u}_{r,c}^m = \mathbf{u}_h - \mathbf{c}^m$, with $m \in \{\text{WDM}, \text{MOSD}\}$ (Equation (7) for WDM and Equation (11) for MOSD). Their distinct physical meanings, sampling heights, and filtering operations are now stated explicitly.

For the wave-phase-aware models, the total lower-boundary momentum transfer is now written in Equation (2) as the sum of the unresolved wall stress and parameterized wave-induced pressure stress, with the corresponding pressure contribution in MOW obtained directly from the resolved field. A separate subsection now presents the common wall-stress formulation, while the model-specific pressure-stress terms are described using the unified notation.

Comment 4

The authors state in Sec. 2.2.2 that the wave phase is not resolved in WDM (and WPM). This is not the case. The waves are horizontally (so phase) but not vertically resolved. The nomenclature should be tightened up.

Response 4: We thank the reviewer for this clarification.

We agree that the original wording was inaccurate. WDM and MOSD retain horizontally resolved wave-phase information over a flat lower boundary, while the wave geometry is not resolved in the vertical direction and the lower boundary remains flat. In contrast, MOW explicitly represents the moving wave geometry and resolves the associated pressure response. We have corrected this distinction throughout the revised manuscript and no longer state that the wave phase is unresolved in WDM or MOSD.

Comment 5

The mathematical notation is poor. In Eq. 19, there is a mix of tensor (LHS) and vector (RHS) notation. In Eq. 24, the RHS is nonsense with a vector velocity and a height gradient; the RHS must be a vector so a correction required. Exactly what correction is made to WDM in Eq. 24 is unclear given this vector notation inconsistency. (The alignment correction in Eq. 24 is unneeded for these 1D cases anyway...)

Response 5: We thank the reviewer for identifying these inconsistencies.

We agree that the original equations mixed vector and component notation. The former Equation 19 has been rewritten in fully component-wise form in the revised manuscript (now Equation 12), with the directional contribution expressed as $(\partial h^\Delta / \partial x_i) / |\nabla h^\Delta|$ for each component i .

The former Equation 24 has been removed. The revised WDM formulation (now Equation 8) uses only the scalar projection $\mathbf{e} \cdot \nabla h^\Delta$ and its corresponding Heaviside function. The additional $\nabla h / |\nabla h|$ alignment factor has been eliminated. Reversal of the momentum transfer direction is instead represented through the local component-wise factor $\text{sgn}(u_{r,c,i}^{\text{WDM}})$, following the later WDM implementation of [Aiyyer et al. \(2024\)](#). For the present one-dimensional aligned-wave cases, this factor simply reverses the streamwise pressure-drag contribution when the local phase-relative velocity changes sign; no additional alignment correction is applied.

Comment 6

A major opportunity is missed to compare the formulations of WDM and WPM. My impression is that, beyond the drag coefficient formulation, there is difference in the dependence on flow velocity in WDM compared to a dependence of relative velocity times height gradient in WPM (difficult to tell exactly from the vector inconsistency in Eq. 24). Is this indeed the sole difference? Which is correct?

Response 6: We thank the reviewer for highlighting this missed comparison.

We agree that the velocity dependence is an important distinction, but it is not the only difference between the formulations. We have revised Section 2.2 to present both models using consistent notation and to clarify their underlying assumptions. For the present aligned monochromatic waves,

the pressure-drag magnitudes scale approximately as

$$\tau_{13}^{p,\text{WDM}} \sim \left(\frac{P}{1 + Q(ak)^2} \right) u|_{z=z_1} |u|_{z=z_1} - c| (ak)^2, \quad \tau_{13}^{p,\text{MOSD}} \sim \frac{1}{\pi} |u|_{z=L} - c|^2 (ak)^2.$$

Thus, both formulations have a leading $(ak)^2$ dependence, although the WDM steepness dependence is additionally modulated by the factor $P[1 + Q(ak)^2]^{-1}$. Their velocity dependence also differs: WDM combines the absolute airflow velocity with the phase-relative velocity, whereas MOSD depends quadratically on the phase-relative velocity.

The models also differ in their derivation, force direction, velocity sampling height, and filtering. WDM is based on an incoming momentum flux and frontal area argument, uses an empirical steepness-dependent coefficient, and samples the grid-scale LES velocity at the first off-wall level without additional test filtering. MOSD is derived from ideal potential flow over locally planar windward wave segments, directs the pressure force along the local surface gradient, and uses the test-filtered velocity at $L = \Delta x$.

Neither formulation can be regarded as universally correct because each relies on different modeling assumptions. In the present comparisons, MOSD follows the MOW and available DNS pressure-drag trends more consistently, particularly at larger wave age, whereas the relative performance for the mean velocity depends on the wave regime. We have revised the discussion to make these distinctions and limitations explicit.

Comment 7

WDM and WPM use different formulations for the wall model. How much does this influence the results? Would WDM be improved by using the WPM wall model formulation rather than the empirical exponential damping?

Response 7: We thank the reviewer for raising this point.

We apologize that the original presentation made the simulation setup unclear. Although Section 2 described the native wall models associated with WDM and MOSD, all comparative simulations in the original submission already employed the same roughness-based quadratic wall-stress framework, the same prescribed background roughness length, and the first off-wall level for input velocity sampling. Therefore, differences between the native WDM and MOSD wall models do not influence the results presented in the manuscript.

To make this clearer, the revised manuscript now presents the common wall-stress framework in a separate subsection before describing the numerical implementation (Section 2.3). The native WDM and MOSD wall formulations have been moved to Appendix A and are explicitly identified as formulations that were not used in the comparative simulations. This is a clarification and reorganization of the original setup rather than a change to the wall-stress treatment used in the simulations.

Because the WDM discrepancies occur while using the same wall-stress framework as MOSD, they arise primarily from the WDM pressure-drag formulation rather than its native wall model. Using the native MOSD wall model could alter the unresolved wall-stress contribution, but it would not directly change the velocity dependence of the WDM pressure-drag term. The exponential damping analysis, now presented separately in Appendix B, targets this pressure-drag behavior and is not a replacement for the wall model. The primary model comparison continues to use the original, undamped WDM pressure-drag formulation.

Comment 8

For WDM, in the 2024 paper, the authors have an additional factor to deactivate the model when the waves are faster than the flow speed. Is this included? (This might fix the drag; see below.)

Response 8: We thank the reviewer for pointing this out.

The swell-specific correction introduced by Aiyer et al. (2024) is not included in the present WDM implementation. We clarify that the local sign treatment, $\text{sgn}(\vec{u} - \vec{c})$, was already included in the original numerical implementation of WDM presented by Aiyer et al. (2023), although it was not explicitly shown in the published formulation. Its role was subsequently clarified by Aiyer et al. (2024). In the present vector formulation, this treatment is applied componentwise as $\text{sgn}(u_{r,c,i}^{\text{WDM}})$, allowing the pressure-drag direction to reverse when the local phase-relative velocity becomes negative. The separate swell correction proposed by Aiyer et al. (2024) for these conditions is not included.

We agree that the swell correction would reduce the excessive negative pressure drag predicted by WDM at large wave ages. We have clarified this distinction and limitation in Sect. 2.2.2 and Appendix B. The primary comparison uses WDM without the swell correction, while Appendix B examines an empirical wave age damping over the range considered here, $8 \leq c/u_* \leq 24$, and notes that the swell-specific treatment of Aiyer et al. (2024) may be more appropriate at larger wave ages.

Comment 9

The comparisons between LESGO and WRF are very confusing and incomplete. WPM is implemented in LESGO, so why is this also not included in the comparisons? For WDM, since this is not the original implementation, is the comparison with LESGO really verification? Why not just compare with the results in the Aiyer paper? The verification should either be systematic and complete (all models) or removed.

Response 9: We thank the reviewer for this comment.

We agree that the limited LESGO comparison was incomplete and could be misinterpreted as a formal verification of the WDM implementation. We have therefore removed the LESGO comparison entirely, and no longer make a code-to-code verification claim. The WDM and MOSD implementations are instead assessed consistently against MOW under the common WRF-LES framework, together with comparisons to available published reference data where applicable.

Comment 10

For the comparison simulations, the number of grid points for the phase-resolved simulations is only 32 compared to 22. However, the vertical grid spacing near the wall is nearly 10x smaller! The grid in the phase-resolved simulations must be highly stretched. How so? Are the results grid independent for both phase-resolved and phase-aware?

Response 10: We thank the reviewer for raising this point.

The MOW grid is strongly nonuniform, with the vertical levels concentrated near the moving surface and the grid spacing increasing toward the domain top. In the present simulations, MOW explicitly resolves the wave geometry and associated pressure response and uses a near-surface spacing of approximately $\Delta z = 0.12$ m. In contrast, WDM and MOSD use a flat lower boundary, and the first off-wall grid point must remain above the prescribed unresolved wave surface. We impose $a_{\text{max}} < 0.85\Delta z/2$, which requires $\Delta z \simeq 2.5$ m for the maximum amplitude considered and allows the

same 22-level grid to be used for all wave steepnesses. We have clarified these model-specific grid constraints in Section 5.

Table 1: Comparison of normalized pressure drag, D_p/u_*^2 , obtained from MOW using the standard 32-level and 22-level vertical grids.

c/u_*	ak	32-level grid	22-level grid
8	0.10	0.092	0.009325
8	0.18	0.240	0.029291
8	0.25	0.382	0.047638
10	0.10	0.108	0.008172
10	0.18	0.283	0.023763
10	0.25	0.403	0.040035

The 22-level MOW results are 87–92% lower than the corresponding 32-level results, so we retain the 32-level grid for MOW in the present study (see Table 1 below). These tests demonstrate the sensitivity of MOW pressure drag to the coarser vertical grid for the cases examined, but they do not constitute a systematic grid convergence or grid independence study and do not establish a general minimum vertical resolution for MOW. We therefore do not claim strict grid independence for either model class. We have added this vertical grid sensitivity analysis to the Appendix of the revised manuscript (Appendix C).

Comment 11

The models should be run with the same time step, and the models should be run with the same wall models.

Response 11: We thank the reviewer for this comment.

We agree that the same outer time step should be used for a direct comparison. We therefore use the same outer Runge–Kutta time step for all three models within each corresponding wave condition. A time step of $\Delta t = 0.01$ s is used for all cases except $c/u_* = 8$, $ak = 0.10$, for which $\Delta t = 0.005$ s is used for all three models. MOW uses four acoustic substeps, whereas WDM and MOSD use six; however, these substeps advance only the fast acoustic modes, while the momentum tendencies and wave-induced pressure forcing are advanced using the common outer time step. The resulting acoustic Courant numbers remain below the practical stability limit of 0.5 (Skamarock et al., 2008). These details are now reported in Section 5.

Regarding the wall model, all comparison simulations already employed the same roughness-based quadratic wall-stress framework, the same background roughness length, $z_{0,b} = 2 \times 10^{-3}$ m, and the first off-wall model level for input velocity sampling. The velocity entering this common wall-law reflects the respective lower-boundary kinematics: MOW uses the surface-relative velocity projected parallel to the explicitly moving surface, whereas WDM and MOSD use the horizontal surface-relative velocity over a flat boundary. The native wall models from the original WDM and MOSD studies are not used in the comparison and are included only for reference in Appendix A. We have revised Section 2.3 and Figure 1 to make this common treatment explicit.

Comment 12

In the verification results in Fig. 2, why does the total stress indicate an oscillation near the surface in WRF?

Response 12: We thank the reviewer for noting this feature.

The near-surface oscillation was observed in configurations with near-isotropic horizontal and vertical grid spacing and also occurs in otherwise identical flat-wall WRF-LES simulations, indicating that it is not caused by WDM or the imposed waves. Since the verification/sanity check comparison has now been removed from the revised manuscript, this figure and the associated discussion no longer appear in the paper.

Comment 13

For the verification results in Fig. 3, the only meaningful conclusion is that the values closest to the surface are roughly the same, so the model must be correct. I note that the grids near the surface are also different. Are these differences due to a model error, discretization error, etc.? I would not really call this exercise “verification”; it is really a sanity check to make sure that the implementation is more or less ok and need not appear in the manuscript without more rigor.

Response 13: We thank the reviewer for this comment.

We agree that this comparison does not constitute a formal verification, and we have therefore removed the WDM comparison between WRF-LES and LESGO from the study.

Comment 14

In Fig. 4, the main issue is the large negative drag predicted by WDM. Those authors already acknowledge this in the 2023 paper; the model was reformulated in the 2024 paper to deactivate when the waves are fast, which might address the large negative drag. Have the authors implemented the most recent WDM?

Response 14: We thank the reviewer for raising this point.

The WDM used in the primary comparison follows the original monochromatic wave formulation of [Aiyer et al. \(2023\)](#), rather than the swell-specific extension introduced by [Aiyer et al. \(2024\)](#). Our implementation includes the local component-wise sign correction, $\text{sgn}(u_{r,c,i}^{\text{WDM}})$, which was present in the original WDM source implementation although not stated explicitly in the published formulation. This correction allows the pressure-drag direction to reverse when the local phase-relative velocity changes sign, but it does not limit the resulting drag magnitude.

The 2024 formulation introduces a separate swell treatment: when the wave is locally faster than the airflow, the original WDM contribution is deactivated and replaced by a wave age-based, phase-averaged swell parameterization. This treatment is not included in the present implementation. We agree that it could reduce the excessive negative pressure drag predicted by the original WDM at large wave ages. We have clarified this distinction in Section 2.2.2 and Appendix B. The primary comparison retains the original WDM formulation, while Appendix B examines a separate wave age-dependent damping over the range considered here, $8 \leq c/u_* \leq 24$, and notes that the swell treatment of [Aiyer et al. \(2024\)](#) may be more appropriate for strongly swell-dominated conditions.

Comment 15

The first full paragraph on page 19 is somewhat redundant with the previous.

Response 15: We thank the reviewer for noting this.

We agree and have removed the redundant discussion. The detailed motivation, formulation, and evaluation of the wave age-dependent WDM correction have now been consolidated in Appendix B, while the main text retains only a brief summary and reference to the appendix.

Comment 16

The main event of the analysis is Fig. 6. However, the commentary on the utility of the phase-aware models compared to the phase-resolved model is rather limited. Please expand to provide guidance to the community on when to use each model when. There is some of this in the conclusions but is missing from the main text.

Response 16: We thank the reviewer for this suggestion.

We agree and have expanded the discussion to clarify the practical tradeoff among the approaches. In the present simulations, MOW requires substantially finer near-surface vertical resolution to represent the moving wave geometry and associated pressure response, whereas WDM and MOSD retain the wave vertically unresolved and can be applied on coarser near-surface grids.

The tradeoff is therefore between physical fidelity and grid resolution requirements. MOW is preferable when detailed wave-induced flow structure is required, whereas WDM and MOSD provide wave-phase-aware representations on coarser grids when the primary quantities of interest are mean flow and bulk momentum transfer statistics. For the cases examined here, MOSD generally remains closer to the MOW mean flow response than the original WDM formulation. We do not assign a general computational speedup because the minimum reliable resolution and cost of each approach were not established through a systematic grid refinement study.

Comment 17

The wave-induced velocities are very different quantitatively and qualitatively. I do not see how any claim could be made that they are similar. More text is devoted to trying to make this claim than to a systemic discussion of Fig. 6.

Response 17: We appreciate the reviewer highlighting this inconsistency in the original discussion.

The original manuscript already presented the primary comparison using a common color scale and reported that the wave-induced velocity amplitudes predicted by the wave-phase-aware models were approximately one order of magnitude smaller than those obtained with MOW. However, we agree that the subsequent statements describing the fields as exhibiting “similar behavior” or “reproducing the overall spatial pattern” were not supported by the quantitative and qualitative differences shown in the figure.

We have removed these statements and revised the discussion to distinguish the broad upstream inclination observed with increasing wave age from quantitative agreement among the models. The revised text now states that WDM and MOSD do not reproduce either the amplitude or the detailed spatial distribution of the wave-induced velocity field resolved by MOW. We have also added a discussion explaining that agreement in mean flow quantities does not imply recovery of the underlying wave-induced flow structure and that MOW is preferable when these detailed phase-dependent features

are required.

References

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