Response to Reviewer 3

Title: Implementation of a sigma coordinate system in PALM-Sigma v1.0 (based on PALM v21.10) for LES study of the marine atmospheric boundary layer

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We take this opportunity to thank the editor and reviewers of our paper for their kind collaboration to the improvement of this manuscript. We have taken into account all the concerns raised and we have made suggested modifications, marked by yellow background in the revised manuscript.

Comments and responses

Reviewer #3

General comments:

Wave-atmosphere interactions are crucial for accurate weather and climate prediction. In this study, the authors developed a sigma coordinate system within the PALM model that directly resolves wave phases. Several case studies are employed to demonstrate the model's results. This modeling tool holds significant potential to provide deeper insights into wave-atmosphere interactions and to benefit the broader research community. Overall, the paper's structure is clear and easy to follow. However, the verification of the model results needs to be strengthened before I can recommend publication. Detailed comments are provided below:

Response: We thank the reviewer for the positive evaluation of our work and for the constructive suggestions. All comments have been carefully considered, and detailed responses are provided below.

Major Comments:

1. Section 5: Do you include the roughness length used to describe the drag from unresolved small-scale waves riding on the larger-scale resolved swell, as done in Peter Sullivan's model? If so, what is its value?

Response: Thank the reviewer for this question. Yes, we adopt a constant roughness length of $2*10^{-4}$ m, which is the same value used in the model of Sullivan et al. (2008). This has now been explicitly stated in Section 3.4.3 of the revised manuscript, right after Eq. (31).

2. Figure 3: When comparing your results for the OW case with those from Sullivan's OW case (Fig. 6 in their study), your results exhibit much larger wind fluctuations that extend to a much higher layer. Why is this?

Response: We thank the reviewer for raising this important point. The stronger and more vertically extended wind fluctuations in our OW case compared to Fig. 6 of Sullivan et al. can be explained by several factors.

First, the instantaneous fields shown in Fig. 6 of Sullivan et al. are limited to a vertical extent of approximately 100 m above the wave surface, which captures only the lower part of the atmospheric boundary layer, so the turbulent structures occurring at higher elevations are not visible in their figure, making a direct visual comparison of the vertical extent of fluctuations difficult.

Second, a comparison of instantaneous velocity snapshots is inherently ambiguous, as such fields are strongly influenced by the transient state of turbulence. The snapshot shown by Sullivan et al. may correspond to a relatively inactive turbulent phase, whereas the snapshot presented here reflects a more energetic realization.

To provide a more quantitative comparison, we have therefore complemented Fig. 3 with vertical profiles of the variance of the vertical velocity component. These statistical measures are less sensitive to the choice of snapshot and better characterize intensity of fluctuations in the vertical velocity. The resulting variance profiles show good agreement with the corresponding statistics reported by Sullivan et al. (see their Fig. 13), as demonstrated by a direct comparison provided in Appendix C (Fig. C1(b)).

3. Wind profiles comparison: Your wind profiles for the OW and SW cases show substantial differences. However, in the results from Sullivan's code, these two experiments exhibit only small differences in the wind profile (See their Fig. 10). I suggest that the authors compare their model results with previous LES/DNS simulations to verify their findings.

Response: To address this point, we have performed a direct comparison between our results and those reported by Sullivan et al. by adding a new figure to Appendix C (Fig. C1), which includes profiles of the streamwise velocity, the variance of the vertical velocity component, and the wave-induced pressure stress for the NW, FW, OW, and SW cases.

This comparison shows that while discrepancies exist in the mean streamwise velocity profiles, our results exhibit very close agreement with Sullivan et al. in terms of the variance of w for all wave configurations. Since these fluctuations in the vertical velocity are primarily driven by wave-induced motions, especially near the surface, this agreement provides strong validation of the capability of PALM-Sigma to capture wave-induced turbulent structures in the atmospheric boundary layer.

The differences observed in the mean wind profiles may not be attributed to the modeling of wave effects but arise from two other aspects. In particular, the Coriolis parameter (and thus the Ekman balance) depends on geographical latitude, which differs between our setup ($f = 1.18 \times 10^{-4}$ 1/s) and that of Sullivan et al ($f = 1.0 \times 10^{-4}$ 1/s). Moreover, the inversion-layer height in our simulations is set to 600 m, compared to 400 m in Sullivan et al., which can also affect the vertical distribution of momentum and the resulting mean wind profiles.

Regarding the SW case, our results indeed differ from those of Sullivan et al., who report very similar wind profiles and pressure-stress distributions for the OW and SW configurations. In our simulations, by contrast, the flow appears much less sensitive to the underlying steady wave geometry. This difference is not associated with vertical velocity fluctuations, as the variance of w in the SW case is very small in both studies and shows close agreement with Sullivan et al. (Fig. C1(b)). This indicates that the discrepancy primarily arises from differences in the wave-induced pressure field and its contribution to the momentum flux. In Sullivan et al., the pressure perturbations associated with steady waves exhibit a relatively large magnitude (see their Fig. 8), while in our simulations, by contrast, the pressure perturbations induced by stationary waves are considerably weaker and more symmetrically distributed along the wave surface (Fig. 5), resulting in a much smaller net momentum effect.

Our results indicate that the absence of wave propagation and associated surface particle motions substantially reduce pressure-modified momentum transfer, whereas Sullivan et al. reported comparable pressure stresses for the OW and SW cases despite the much lower relative wind speed in the SW configuration. Given that the relative wind speed in the OW case exceeds that of the SW case by approximately 12.5 m/s, stronger drag would physically be expected in the OW case. The contrasting behavior between the two studies suggests that the flow response to steady wave geometry may depend sensitively on model formulation and therefore warrants further investigation. This issue is now explicitly discussed in Appendix C of the revised manuscript.

4. Energy budget: Did you check the energy budget of the LES simulations? Are they closed?

Response: Though a full, term-by-term kinetic energy budget was not explicitly output in the present simulations, we did examine the temporal evolution of the domain-averaged kinetic energy as a check of the overall energy behavior of the LES.

A new figure (Fig. A2) has been added showing the time series of total kinetic energy for the NW, FW, and OW cases over the full 20 h simulation period. After an initial adjustment phase associated with inertial oscillations, the total kinetic energy in all cases converges toward a statistically steady level and remains bounded, without any trends of monotonic growth or decay. This stable long-term behavior indicates that the dominant energy sources and sinks are in balance and that no spurious numerical energy accumulation occurs. We therefore use the total kinetic energy as an integral diagnostic of numerical stability and overall energetic consistency, as now clarified in the revised manuscript.

5. Page 9: L188: A is not defined.

Response: We thank the reviewer for pointing this out. The parameter A (wave amplitude) was not explicitly defined in the original manuscript. This has now been corrected by adding a clear definition at its first occurrence in the revised text.

6. Page 11, L240: A is wave amplitude, not wave height, right?

Response: The parameter A indeed denotes the wave amplitude rather than the wave height. This has been corrected accordingly in the revised manuscript.

7. L340: marine atmospheric boundary layer has been defined as MABL. Use it

Response: We thank the reviewer for this comment. The abbreviation *MABL* is now used consistently throughout the revised manuscript.

8. In Fig. 8, TKE_w should not be used for the wave kinetic energy, which is misleading. It should be WKE.

Response: This has been corrected in the revised manuscript by replacing TKEw with WKE in Fig. 8.

9. Figure 4: I suggest changing the colormap in the left panel. As it stands, it is not easy to discern at which height the wind approaches the geostrophic wind described in the text.

Response: We thank the reviewer for this helpful suggestion. Instead of changing the colormap, we added white dashed lines to the profiles in the left panels of Fig. 4, representing the geostrophic wind of 5 m/s, to more explicitly indicate the height at which the mean wind speed approaches the prescribed geostrophic wind.

10. Y-axis clarification: Is the y-axis used in your figures based on height above the mean sea surface level or above the wave surface?

Response: We thank the reviewer for this question. The vertical coordinate shown on the y-axis in all figures is defined relative to the mean sea surface level.